CHEW: Introductory survey

In recent years it has become customary to discuss the structure of the nucleon in terms of two regions. We talk about the outer part where $\pi$ mesons predominate and the Yukawa picture gives us a pretty good basis for discussion. I shall refer to this region as the pion cloud. Then there is the inner region where even in conventional terms you would expect virtual anti-nucleons and strange particles to play a big role and where it is possible that usual notions about space and time break down. I shall refer to this region as the core.

The dividing line between the core and the pion cloud is of course not precise, but is generally placed somewhere between $0.2$ and $0.4 \times 10^{-13}$ cm on the basis of both theoretical and experimental arguments, some of which I shall mention later on.

Actually it turns out to be worth while to divide the outer region again because the tail of the pion cloud is especially amenable to theoretical analysis. So I will refer to this region as the fringe or the tail of the pion cloud and begin my discussion with this region. In momentum or energy space this would be the low energy region. A study of this part of the nucleon structure is closely connected with the problem of determining the Yukawa coupling constant.

A. The Asymptotic Region

To get oriented in the question of the coupling constant let me remind you of a way of thinking about the electromagnetic coupling constant $\alpha$ which is more appropriate to the present discussion than the usual classical definition. The electromagnetic coupling constant $\alpha$ could be defined as the expectation of a certain operator $Q$, which is the total electromagnetic charge operator, taken for a state vector which represents the single charged particle in question.

$$e = \langle \psi, Q \psi \rangle$$

$$Q = \int d^3x \left( \Box \phi \right)$$
It is then possible to prove that the Thomson cross section for zero energy photons is just:

\[ \sigma_{\text{Thomson}} = \text{const. } \chi \left( \frac{e^2}{mc^2} \right)^2 \]

There are no corrections to this regardless of how many radiative effects you want to take into account until you go to energies greater than zero. So we may define, if you like, the electric charge \( e \) as a measure of the strength of the coupling between the charged particles and zero energy photons.

The counterpart of this statement in configuration space is to say that if you go far enough away from a charged particle, the electrostatic potential which it produces is exactly \( e/r \), where this is the same \( e \), with no corrections asymptotically. Of course if you come close in, the structure of the particle will begin to matter and there will be complications.

One of the most significant developments in the past few years in field theory has been the recognition of properties of the Yukawa interaction which are analogous to these properties of the Maxwell interaction. This allows us to define and measure the pion nucleon coupling constant in a relatively precise fashion. These properties have found their most useful expression in the so-called dispersion relations. Let me make a little detour.

The history of the meson dispersion relations is such a confused one that I can't attempt to recount it here. Let me simply say that on the basis of microscopic causality, Lorentz invariance, and certain other properties shared by all conventional local field theories, it seems possible to obtain relations between transition amplitudes and integrals over their imaginary parts.

Microscopic causality, which seems to be the key new feature here, is the assumption that no signals ever propagate faster than the velocity of light, no matter how short the distance involved. Many conjectured modifications of conventional field theories, such as for example those which involve a fundamental length, will surely violate microscopic causality. Heisenberg's non-linear theory, for example, is not microscopically causal.

Several years ago Anderson, Davidson and Kruse (Phys. Rev. 100, 339, 1955) roughly verified the meson dispersion relations for forward scattering. These were relations which had been written down by Goldberger (Phys. Rev. 99, 979, 1955; also Goldberger,
Miyazawa and Oehme, Phys. Rev. 99, 986, 1955). The tendency since that time has been to believe that at least at low energies, these dispersion relations are rather accurately obeyed.

Very recently the Bologna group under Puppi has reinvestigated the forward dispersion relations in detail over a wide energy range and found something strange. The $\pi^+ p$ scattering by itself satisfies the dispersion relation well but the $\pi^- p$ scattering shows a systematic and substantial discrepancy. The original Goldberger relations did not include electromagnetic corrections, and one can think of many such which apply to the $\pi^- p$ system and not to the $\pi^+ p$ system. But so far theoretical estimates have not found any corrections large enough to account for the observed discrepancy. If electromagnetic effects are not to blame, then either the experiments are wrong or we have evidence already for a breakdown of conventional space time concepts at short distances. It is obviously of great importance to have this question settled.

If the dispersion relations are obeyed, at least at low energies, then we may use them to define and measure the Yukawa interaction constant. For example, the following is one of the dispersion relations first written down by Goldberger, Miyazawa and Oehme:

$$\text{Re} \frac{F_{\pi p}(\omega) - F_{\pi^+ p}(\omega)}{\nu} = \frac{g^2}{M^2} \frac{1}{\nu^2 - \frac{1}{4} \frac{1}{M^2}} + \frac{P}{2\pi^2} \int \frac{dy}{y} \frac{\sqrt{y^2 - 1}}{y^2 - y^2} \left[ \sigma_{\pi^- p}^{\text{total}} - \sigma_{\pi^+ p}^{\text{total}} \right],$$

$$g \sim \left( \psi_\nu', \int d^3 x \ J(x) e^{i q \cdot x} \psi_p \right),$$

$$J(x) = (\Box - \mu^2) \phi(x)$$

$$q^i_v = p^i - p$$

$$q^i_o - q^2_o = \mu^2$$

$$q^i_o = p^i_o - p_o$$

$$P^2_v - P^2 = P_o^2 - P_o^2 = M^2$$
This is particularly appropriate for the determination of the coupling constant although there are many other relations which you can write down also. In this relation the "f's" are the forward scattering amplitudes, " $\nu$ " is the laboratory meson energy in units of the meson rest mass energy. The important thing for the present discussion is the constant $g$. It is defined not as the expectation of something, as the electric charge $e$, but it is a matrix element connecting single nucleon states of different momenta $p$ and $p'$ of a certain operator which you might call the total mesonic charge. In any case this operator is related to the fundamental meson field by taking not the d'Alembertian but the Klein-Gordon operator. This definition of the coupling constant was given quite a long time ago by Lepore and Watson (Phys. Rev. 76, 1157, 1949). It seems to be the most convenient one now. The meson momentum $q$ which appears here is related to the two nucleon momenta by the conservation laws. Formally momentum and energy are conserved and you might say that this is the matrix element for the absorption of a meson by a physical nucleon. However, if you take a second look, you realize that it isn't possible to absorb one real particle by another particle and you wonder how it is possible to make sense out of these relations. An extension of this matrix element to complex momenta must be made and you have to show of course that mathematically the extension is well defined. Recently, Bogoliubov and Symanzik, as well as others have succeeded in putting the necessary extension on a firm basis and we now have, within the framework of local field theory at least, a precise and practical definition of the Yukawa constant $g$.

You see from the form of the relation that as the meson energy approaches $1/2M$ (in the units we are using this is a very, very small energy, about $1/13$ of a meson rest mass; it is practically zero), the leading term completely dominates the amplitude (becomes infinite as a matter of fact). Of course, you cannot measure directly at this energy because in the laboratory the lowest energy to which you can go is $\nu = 1$. But since you have the explicit functional form, it is possible to extrapolate down to this energy quite uniquely, assuming that the experiments are all adequate. This has been done by Haber-Schaim (Phys. Rev. 104, 1113, 1956) who thereby found a value for the coupling constant. The value which is given here is not $g$ itself, but $g^2 (\mu /2M)^2$. 
The coupling constant here defined occurs not only in this dispersion relation but also in others of the same type. The other dispersion relations can also be used together with the zero energy limit property, to determine $g^2$. There are at the moment, to my knowledge, no other dispersion relations in which the available experimental information is sufficiently complete to make as good an extrapolation as Haber-Schaim was able to make here. But in at least four cases enough is known to allow some kind of determination:

1. From the dispersion relation for forward spin flip scattering, (the previous was non spin flip), Davidon and Goldberger obtained $g^2 = 18$.

2. From electric dipole photo production, the case first emphasized by Kroll and Ruderman, Bernardini has obtained $g^2 = 12$.

3. From magnetic dipole photo production Koester and Mills have about 14.

4. From the 33 scattering phase shift by itself (Chew and Low) you get $g^2 = 14$, or perhaps 15.

All these determinations, except the one by Haber-Schaim, involve some guessing in order to make the necessary extrapolation; so it is hard to state the uncertainties.

Corresponding to the zero energy limit theorems for scattering processes, there is the principle, analogous to the property of the electromagnetic field that at sufficiently large separations the potential energy of two charges is exactly given by $e^2/r$, that the internucleon potential, at large separation distances approaches:

$$\frac{f^2}{\mu^2} T_1 \cdot T_2 (\sigma_1 \cdot \nabla)(\sigma_2 \cdot \nabla) \frac{e^{-\mu r}}{r}$$

Japanese theorists have been particularly active in exploiting this fact and recently have come up with a determination of the coupling constant based on it. Iwadare, Otsuki, Tamagaki and Watari find

$$f^2 = g^2 \left( \frac{\mu}{2M} \right)^2 = 0.82 \pm 0.15$$

or

$$g^2 = 14 \pm 3$$
a value \( f^2 = 0.08 \pm 0.01 \) from the known properties of the low energy two nucleon system, principally the quadrupole moment of the deuteron. This determination is supposed to be independent of assumptions about how the potential behaves at distances less than 1.5 times the pion Compton wave length. Furthermore, the Japanese have shown that no existing experimental information is in conflict with such a tail for the nucleon force. On the basis of all this I would suggest that it may soon be time to make a new entry in the table of physical constants: \( g^2/\hbar c = 15 \pm 3 \).

Of course whether or not such a constant is really capable of precise definition seems to depend on the validity of the dispersion relations.

### B. The Intermediate Region

We see that there is good reason to think one understands the structure of the outer-most part of the nucleon in terms of the Yukawa theory. This is the same as saying we know the matrix elements for the emission and absorption by a nucleon of very low frequency pions. Now let's see what we understand in the frequency range \( \mu \leq \nu \leq 3\mu \). This corresponds in configuration space to the intermediate range, outside the core but not so far out that the asymptotic limits have been reached. It is in this region that the cut-off static model has been useful. This model is a mutilation of the original Yukawa theory which ignores anti-nucleons, and in fact nucleon recoil altogether, but is adjusted to give the same zero energy or long-distance limits as the fully relativistic theory. There is no compelling reason to believe that the cut-off model should work in any region except the asymptotic, because it neglects all components of the nucleon structure except for P wave pions (the tail of the pion distribution is entirely P wave). The justification of the cut-off model comes from its success.

The notion of a core occurs naturally in the cut-off model because without nucleon recoil the theory is badly divergent unless the source of pion mesons is spread out in space. For example, the charge density of the pion cloud at small radii goes like \( 1/r^5 \), so that the total charge in the pion cloud is quadratically divergent, if you take a point source. If you spread out the source in configuration space or alternatively cut out the high frequencies in momentum space, you eliminate the divergence, but of course introduce a new parameter which can either be described by a cut-off energy or a core radius. So in the cut-off model you have two parameters: the coupling constant, which we now say is determined, and the cut-off energy, \( \Omega_{\text{max}} \), or core radius, \( r_0 \).
With the observed coupling constant, and a sufficiently high cut-off energy, the model predicts a low energy resonance in the 33 state for pion nucleon scattering, with the other three P wave phase shifts remaining small. The approximate relation between the position of the resonance, \( \omega_{\text{res}} \), and the cut-off energy is:

\[
\omega_{\text{res}} \approx \frac{\mu^2}{f^2 \omega_{\text{max}}^2}
\]

So the observed \( \omega_{\text{res}} = 2 \mu \), leads to

\[
\omega_{\text{max}} \approx 6 \mu
\]

If we just crudely take the reciprocal \( \omega_{\text{max}} \), that gives us some idea of the core size: \( r_0 \approx \omega_{\text{max}}^{-1} \approx 0.2 \mu^{-1} \).

The shape of the resonance in the 33 state is quite well described by a relation which can be derived from the cut-off model:

\[
\frac{q^3}{\omega^*} \cot \delta_{33} = \frac{3}{4f^2} \left( 1 - \frac{\omega^*}{\omega_{\text{res}}} \right)
\]

You see here a comparison of the predicted straight line with the existing phase shifts. Here \( q \) is meson momentum and \( \omega^* \) meson energy.

The cut-off model makes no statement about scattering in S, D or higher partial waves. Experimentally the scattering is weak in these states at low energy, which justifies to some extent our ignoring them in the intermediate regions of the pion cloud of the nucleon.

**Fig. 1**
One may inquire as to whether the P wave predictions of the cut-off model are consistent with the fully relativistic Yukawa theory. The most effective approach to this question so far has been through the dispersion relations. Oehme showed that if contributions to the dispersion integrals from energies above the 33 resonance are ignored then the P wave parts of the dispersion relations are almost identical to the Low equations for the cut-off model. The main difference is the absence of the cut-off factor from the relativistic dispersion relations. But if the 33 resonance is really dominant, then the cut-off factor is unimportant. In this sense we can say that the P wave predictions of the cut-off model satisfy the relativistic dispersion relations. It is not yet possible, of course, to obtain all of these predictions directly from the dispersion relations without using the cut-off model as a guide. For example, the position of the 33 resonance could be anywhere without violating the dispersion relations. Also, as shown by Castellejo, Dalitz and Dyson (Phys. Rev. 101, 453, 1956) there may be an arbitrary number of zeros in the scattering amplitudes without violating the dispersion relations. Once the position of the 33 resonance is determined however, and the zeros ruled out, then the shape of the resonance and the general behavior of the small P phase shifts is determined by the dispersion relation. In a certain sense then the cut-off model may be discarded and predictions made directly in terms of the coupling constant and the resonance energy rather than the coupling constant and the cut-off energy.

This approach has recently been followed by Goldberger, Low, Nambu and myself, in the problem of photo meson production. Starting with the fully relativistic photo-meson dispersion relation the assumption was made that only the 33 resonance is important in the dispersion integrals. In this way one arrives at a theoretical formula involving $f_2^2$, $e^2$ and the magnetic moments of neutron and proton as well as the pion nucleon scattering phase shifts. These formulas are consistent with the predictions of the cut-off model when the nucleon mass is set equal to infinity and they agree with experiment about as well as can be expected; that is, most experimental phenomena up to the resonance can be reproduced within about 20%. I think that this afternoon either Goldwasser or Koester will show a comparison of the data with these theoretical formulas. The main sources of uncertainty arise from the S wave dispersion integrals because we have as yet no adequate model for the S wave pion nucleon interaction to guide us.

A discrepancy in the photo-meson picture at present is the negative to positive ratio at threshold. The dispersion approach
which I have outlined predicts this to be 1.3. This result could be altered if certain S wave dispersions integrals turn out to be unexpectedly large. But in view of the observed smallness of most such integrals in the photo-meson problem, we are puzzled by the experimental value for \( \frac{\pi^- \pi^+}{\pi^+} \) found by Bernardini and co-workers, which is about 1.8. The resolution of the current uncertainty over the Panofsky ratio would help settle this point.

It is possible to make some statements about S and D wave pion nucleon scattering on the basis of relativistic dispersion relations, but this subject is still so cloudy that I would prefer to pass over it, and proceed to the electromagnetic structure of the nucleon. The S wave scattering problem almost surely involves the core of the nucleon in an important way.

In view of our success in understanding the interaction of free pi mesons with nucleons up to an energy of about 2 \( \mu \), one expects to be able to predict some properties of the charge current distribution of the pion cloud in this intermediate region. (General principles require these charge current distributions to be the same for neutrons and protons, but with opposite signs.) For example, the second radial moment of the magnetic moment distribution of the pion cloud, assuming only a P wave component in the cloud, comes from such large distances from the core that it is only weakly dependent on the core radius. At small distances the magnetic moment density of the pion cloud, \( M_{\pi} \sim 1/r^4 \). So with the \( r^2 \) from the volume element and the additional \( r^2 \) from the second moment, the second moment \( (M_{\pi} \ r^2) \) becomes relatively independent of the core radius.

Salzman has calculated \( (M_{\pi} \ r^2) \) and finds a result which agrees well with the full second moment, which of course includes the core currents, as measured in Stanford electron proton scattering experiments. The average value for the magnetic moment radius as measured and as calculated is \( r_M \approx 5 \mu^{-1} \). The reason for this result, theoretically, turns out to be that the magnetic radius is approximately the geometric mean of the meson Compton wave length and the core radius: \( r_M \approx \sqrt{\mu^{-1}r_0} \). This result, by the way, also comes out of relativistic perturbation theory, where instead of the core radius, you have the nucleon Compton wave length, which of course is numerically the same thing. It is not understood why the relativistic perturbation theory should give a sensible answer in this particular case, when it gives so many nonsensical answers.
You may ask if it is reasonable to compare the complete magnetic moment radius with a theoretical calculation based on the pion cloud alone. I would say that a priori we expect that the small radius of the core means that even if it had a magnetic moment it would not contribute much to the average radius of the moment.

Even though the pion charge density is more singular by one power of $r$ than the magnetic moment density, the second radial moment of the charge of the pion cloud ($\rho_{\pi} r^2$) is still only logarithmically dependent on the core radius, so perhaps again one might be able to make a reasonable calculation. Salzman, as well as Treiman and Sachs and some others have calculated ($\rho_{\pi} r^2$) and find a result which is about $3/4$ of the observed second moment of the total proton charge ($\rho_p r^2$) as measured at Stanford. However the core must make a substantial contribution here, in contrast to the case for the magnetic moment, because we know from low energy neutron-electron scattering that ($\rho_n r^2$) for the neutron is almost zero. In other words, in the neutron, the core contributes a positive ($\rho_n r^2$) which almost cancels the negative ($\rho_{\pi} r^2$).

This surprisingly large value for the second charge moment of the neutron core has led to speculation that we have evidence here against the validity of the local field concept at short distances. I am personally not ready to reach such a drastic conclusion because, for example, the perturbation calculation in the relativistic local theory, as pointed out some time ago by Foldy, actually leads to a nucleon core contribution of the required order of magnitude. The reason for this is peculiar, to be sure. You find that the nucleon core charge in the relativistic perturbation theory is actually confined within a distance $1/2M$ which is of course completely within what we are calling the core. But you get a large second moment, nevertheless, because the charge density changes sign right at the origin. Of course, one might turn the argument around and say that the large observed radial moment of the core charge supports this rather peculiar aspect of the perturbation method.

Another explanation, however, has also been suggested. It is that even in the intermediate region the normal pion distribution occasionally produces nucleon anti-nucleon pairs which then interact with the electromagnetic field to give an effective charge which has the same sign for both neutron and proton. It turns out that for this to happen at least three pions have to be involved. In terms of the Feynman diagram:
Two pions will always give the result that the effective charge is opposite in sign for neutron and proton but the three pion combination can give you the required equality of sign for neutron and proton. I don't know of any real calculations of such a mechanism because the matrix elements involved are so unfamiliar to us. Perhaps if we knew the annihilation cross section leading to three pions one could make some sort of an estimate.

In principle one can distinguish experimentally between these two possibilities; that is (1) if the trouble comes from peculiar behavior right inside the core, changing the sign of the charge distribution, or (2) from something peculiar happening out in the pion cloud. In the first case you would expect that the form factor for the charge distribution would not go to zero at large momentum transfers. At least it would go to zero much more slowly than if the structure stretches out into the intermediate region.

Probably one would not worry so much about the large value of \( \frac{P_c}{r^2} \) if it did not seem that the core contributes so little to the anomalous moment. The magnetic moment of the pion cloud (in contrast to the second radial moment of the magnetic moment which is independent of the core radius) is linear with the core radius and therefore is not a quantity we can feel very sure about. But one can make a calculation with \( \omega_{\text{max}} \approx 6 \mu \) as, for example, has been done by Miyazawa, and one finds about two nuclear magnetons for the pion contribution. This is positive for the proton and negative for the neutron. And of course, this result is not far from the observed anomalous moments. So you would say that there is not much left over for the core. However, even more convincing than this are two
other arguments. First, if the core does contribute, you would not expect the contributions to be equal and opposite for the neutron and proton, as is approximately true experimentally. And finally, the magnetic moment form factor for large momentum transfers which has been measured at Stanford, shows no particular evidence of a large contribution from small radii. It seems to be a fairly uniform distribution. Thus it seems likely that one has to understand a core which has a large average charge radius, but at the same time a small contribution to the anomalous magnetic moment. If the 3-pion mechanism turns out to be adequate for the building up of a large charge radius it must not give much contribution to the magnetic moment at the same time, or you will not have solved the problem.

Now let me turn to the manifestation of this intermediate region in the two nucleon force problem. In principle, one should be able to calculate the internucleon force down to distances of about $1/2 \mu^{-1}$ or $2/3 \mu^{-1}$ since we have seen that in other respects the P wave pion cloud picture is successful in the intermediate region. If we have two nucleons and the cores do not overlap, you would expect the nuclear force to be understood in terms of the pion cloud. Unfortunately technical difficulties have thus far prevented a clear comparison of theory and experiment. Theorists have grown used to thinking of the internucleon force in terms of a static local potential which indeed it is at large distances. In the fringe region this is a completely correct concept. But in the intermediate region, we have neglected to consider the proper description of the two nucleon system when the nucleon motion is not negligible. This non-static effect turns out, as I will try to show, to be significant.

The order of magnitude situation is as follows: The internucleon potential energy, due to the exchange of a single pion, is essentially:

$$V_1 \sim \int^2 e^{-\mu r} \frac{1}{\mu^2 r^2} \frac{1}{r}$$

The potential energy due to simultaneous exchange of two pions is:

$$V_2 \sim \int^4 e^{-2\mu r} \frac{1}{\mu^4 r^4} \frac{1}{r}$$
The ratio between these two is:

\[
\frac{V_2}{V_1} \sim \int_0^2 \frac{e^{-\mu r}}{\mu^2 r^2}
\]

Now let's estimate the relative non-static effect. This will be of the order of the nucleon kinetic energy or its potential energy (these are likely to be of the same order) divided by the average total pion energy which by an uncertainty argument is just the reciprocal of the distance that the pion has gone from the nucleon.

\[
\frac{\text{Nucleon Kinetic energy}}{\text{Average pion energy}} \approx V_1 r \approx \int_0^2 \frac{e^{-\mu r}}{\mu^2 r^2}
\]

the same result found above for \( \frac{V_2}{V_1} \). So if you try to go beyond the exchange of one pi meson then you must take into account nucleon recoil and you must deal with nonstatic forces.

Now, of course, you would expect a priori that both two-meson exchange and recoil effects are small in the intermediate region because if you evaluate the above with \( \mu^2 = .08 \) you find that it does not approach unity until you get quite close to the core. But the central force part of \( V_1 \) turns out to be accidentally small by about a factor of \( 1/7 \). So that a large fraction of the observed central force has to be due to two-meson exchange. The bulk of the tensor force, on the other hand, does come from \( V_1 \) and here theory and experiment are in satisfactory agreement. This has been shown by many calculations, the most exhaustive being those of the Japanese theorists under Taketani.

Most treatments of the central force problem in the intermediate region have attempted to find an effective static potential which replaces the recoil effects by some additional, but still static terms. These attempts, however, have not led to unambiguous results and great controversy still rages on the subject. My own belief is that the static approach has to be abandoned, even if it means solving integral rather than differential equations, before we shall know for sure whether the central force, in the intermediate region, is correctly described in terms of the P wave pion cloud.
C. The Core

Let me turn finally to the core itself, that dark and forbidding region where we understand so little. Empirically speaking there are a few facts. I have already emphasized the rather large value of the second moment of the core charge, at least for the neutrons, and the probably small contribution of the core to the anomalous magnetic moment. From nuclear force analyses we know that the effect of the core is repulsive in the two nucleon system. The most recent Los Alamos phenomenological analysis places the effective radius of the core in the force at \(0.5 \times 10^{-13}\) cm. Half of that, which is what we should compare here, would be \(0.25 \times 10^{-13}\) cm. Japanese meson theoretical potentials have led to similar results. Gartenhaus showed that such core radii are consistent with a cut-off energy \(\Omega_{\text{max}} \approx 6\mu\) in the static model, and since \(6\mu \approx M\) or about twice the mass of a K meson, we can't be surprised at the core radius.

So we know the approximate size of the core and some of its bulk properties. But that's about the end of the story for the present.

Clearly, we cannot expect to understand much more until there are successful theories to describe the components from which the core is made. Besides high energy \(\pi\) mesons there are surely also virtual anti-nucleons and strange particles. Quantitative theories to describe phenomena involving any of these components are to my knowledge non-existent. Perhaps we shall hear about some before the end of this conference. Of course, speculations have already been made, but I think that any discussion of them would be premature in a talk such as this. Let me close therefore with the summarizing remark that we understand the outer fringe of the nucleon rather well, we understand some aspects of the intermediate region, but we do not understand anything about the core.

DISCUSSION

SACHS: I should like to ask about the Tamm calculation. Does that mean that the pairs are produced in the intermediate region and not just in the core region?

CHEW: The region is determined by the three meson masses rather than by the mass of the nucleon, and therefore the range will only be 50% less than the normal pion range.
SACHS: Doesn't that bring it well into the core region?

CHEW: Well, it is a question of a factor of 1.5. You see, the normal pion cloud comes from a two meson contribution; this is a three meson contribution. So it is 50% smaller.

SACHS: I would like to remark that you don't really have to produce these pairs way out in order to get the effects you want, because there can be an interference phenomena there. If you have high correlation between the nucleon and anti-nucleon pairs, you could get effects that are large, even if they occurred at a relatively small radius, just by positive correlation.

CHEW: Well, that would involve the changing of sign of the charge density, I would presume, as I mentioned.

SACHS: If you are speaking of the change in sign at the origin, I do not believe it is the same effect.

BREIT: I should like to ask the speaker how much he believes the Gartenhaus potential?

CHEW: Without further work on the proper way to handle the non-static effects, I would not have much confidence in it. It is possible that it is right, but there is an uncertainty as to how to handle these non-static effects.

BREIT: Wouldn't you think that if a large part is played by the potential at small distances, the results are questionable?

CHEW: I certainly think they are questionable. If there are actually important contributions from the force at small distances, one has to be very careful.

MARSHAK: As far as non-static forces are concerned, I suppose a spin orbit force would take care of some of the recoil effects.

PEIERLS: But this would be only one example of many types of velocity dependencies.

KLEIN: I take a somewhat more optimistic view toward the calculation of the central forces than Dr. Chew did. I think that non-static effects are confined almost entirely to the core.
CHEW: Yes, but the contribution from two more mesons to the potential will have the same shape.

KLEIN: But we know from direct calculations that this estimate of the ratio of the two meson to four meson contribution is unreliable.

CHEW: Well, the Japanese don't agree with this. They simply don't believe that that is the right way to calculate. I don't know. I think that the question is not yet settled.

FUBINI: I was asked by Dr. Cini to say a few words about possible corrections to dispersion relations. This work was done by Agodi, Cini, and Vitale. They were looking for possible corrections due to the pion mass differences or due to electromagnetic or strange particle effects. They wanted to see if these corrections would be large enough to account for the discrepancies found by Puppi in the $\pi - \bar{p}$ dispersion relations. The result is negative. The mass difference gives some small unphysical region, and they have computed the effect of this region. They did not get a big effect. There was also a preliminary investigation of the electromagnetic effects, such as production of photons in intermediate states. This also turned out to be small. Finally, as expected, the effect of the strange particles is simply to redefine the coupling constants. So the conclusion is that if more accurate experimental data confirm Puppi's results, either there is violation of charge independence in pion-nucleon interaction or violation of charge independence in hyperon-nucleon interaction, or perhaps more probably, violation of microscopic causality.

MATTHEWS: I would like to point out that there may be a relation between the large second moment of the core charge distribution and the observed large nucleon anti-nucleon annihilation processes.

HOFSTADTER: Electron scattering from nucleons

I would like to give a progress report on the Stanford experiment scattering electrons on nucleons. As many of you know, the machine was turned on recently after a rather long lapse and we have higher currents than before and higher energy, so that it is now possible to extend the previous measurements. I would like to mention some of the people who are associated with this work. In
the talk I will not specify with which part they have been connected; F. Bumiller, H. Ehrenberg, U. Meyer-Berkhout and M. R. Yearian.

The experiments attempt to find the electromagnetic structure of the nucleon. This is done by scattering electrons from the proton to study its structure, and from the deuteron to study the structure of the neutron. In addition, experiments have been carried out with somewhat heavier nuclei in order to find out the structure of the neutron from these nuclei.

The general procedure is to see if you can find, from the angular distributions and from the absolute cross sections, the form factors that are associated with the nucleons, with the charge structure and the moment structure. I haven't the time to go into the details, so I will simply write down an expression we have used that fits the angular distributions. This is the modified Rosenbluth formula (Rev. 79, 615, 1950) with form factors put in to allow for the structural effect:

\[
\frac{d\sigma}{d\Omega} = \left(\frac{e^2}{2E_0}\right)^2 \frac{\cos^2 \frac{\theta}{2}}{\sin^4 \frac{\theta}{2}} \left(1 + \frac{2E}{Mc^2} \sin^2 \frac{\theta}{2}\right) \left\{F_1^2 + \frac{q^2 + \mu^2}{4Mc^2}\right\} \left\{2(F_1 + \mu F_2)^2 \tan^2 \frac{\theta}{2} + F_2^2\right\}
\]

where \(q = \frac{2}{\lambda} \frac{\sin \theta/2}{\sqrt{1 + \frac{2E}{Me^2} \sin^2 \theta/2}}\).

The left hand factor is the Mott scattering with a center of mass correction. In the bracket \(q\) is the momentum transfer and \(\mu\) is the anomalous moment of proton or neutron. \(F_1\) is the Dirac form factor and \(F_2\) is associated with the anomalous part of the moment or the Pauli part. These are to be determined by experiment.

Now I would like to turn to some of the data to illustrate how you fit this formula. The experiments have been done recently at energies higher than those we have been able to reach before, but the data are quite preliminary. We are now at 600 to 650 Mev.
In Figures 2 and 3 we have the logarithm of the cross section versus the laboratory angle. In order to avoid saturation troubles in the magnet at these high energies, we restrict ourselves to rather large angles so that the energy of the scattered electrons falls in a region where the magnets can handle it safely. Such a region is, say, 75 degrees to 135 degrees.

The most important thing about these data is that now we are about to get absolute values of the cross sections. They are not very well determined at the moment but we hope they will be improved considerably in the near future. The two heavy black points on the graphs are absolute values. I don't know exactly how big an error to put on them at the moment, but it would probably be larger than shown. Also shown on the graphs are some form factors (that is, the cross sections with the form factors included in it), for several values of RMS radius $a$. The angular distribution curves are for a model we have used successfully before, that has fitted all of the data up to the moment. This
is a hollow exponential. There is nothing sacred about it; it is just one of a number of models that fit.

You see that the absolute values are in quite good agreement with these curves, an indication that at the higher energies the data essentially are going to give more or less the same information we've had before. There is one further point to notice on these figures. The theoretical curves are absolute values, so are the boldface points. You can slide the relative experimental points (light points), if you wish, up and down a little to test the angular fit. And you see, within the experimental error you can't really distinguish between \( a = 0.3 \) and \( a = 0.8 \). The experiments, however, will get better. These data represent only a couple of runs. But it is interesting that at these higher energies, if you can believe anything from such meager data, the angular distribution tends to be higher up at large angles. If this proves to be so as we go to still higher energies, it indicates that there is something more dense at the center of the nucleus.

I have now said all I am going to say about the proton and I'd like to speak about the neutron.

The obvious way of investigating the neutron suggests itself: One should look at the deuteron at a high energy and at a large angle. This is because the small angle scattering is charge scattering, but at large angles it is magnetic moment scattering. The neutron has a magnetic moment -1.91 as compared with 2.79 for the proton. If you look at the cross section formula and put \( F_1 \) equal to zero for the neutron, then, roughly speaking the scattering goes as the square of the magnetic moment and you should have about half as much scattering from the neutron as from the proton. Therefore, under conditions of large angles and high energy, one should be able to see a contribution due to the neutron which would not be there at small angles. So for quite some time experiments on the deuteron have been carried out.

Since the binding energy of the deuteron is low and the potential well is not very deep, you would expect that the proton and neutron would behave almost independently under conditions of a very large momentum transfer in the impact. Sometime ago we decided to look at the deuteron in the position near the energy where the free proton shows its elastic scattering peak. Calculations by Jankus, and improvements on his calculations to take into account the form factor, show that at large angles and high energies the cross section for the
deuteron should be equal to the cross section of the proton plus the cross section of the neutron, with a small correction which may be as high as 5%. We will neglect the 5% correction for the moment and just say that the deuteron cross section is equal to the proton plus the neutron cross sections.

First, in Fig. 4 we have the CH$_2$ and C experiments, at 500 Mev and 135°. This is the free proton peak superposed on the carbon background. The tail drawn through the experimental point to the left of the peak (the two sets of experimental points mingle and it is hard to tell them apart) is the largest tail we can put on the proton curve. We don't believe it is there, but we put it there just to show the limits of error. The elastic peak appears at a position corresponding to about 260 Mev.

Fig. 5 shows the data on deuterium. The free proton peak at about 113 in Fig. 4 is very narrow compared to this distribution. The carbon
background, of course, underlies the peak in Fig. 4. This we know and can subtract. Now the scattering from the deuteron is in the nature of a continuum because you are scattering from the proton and the neutron while they are in motion. You therefore have a Doppler shift, so to speak, of the energy of the scattered electrons. So you have to compare an area as in Fig. 5, after subtraction of the carbon background, with the area under a sharp peak. This of course has its difficulty, and that is the main reason why we can't produce very accurate numbers at the moment. Fig. 6 shows our best idea of the comparison after putting all the data together.

The sharp peak is due to the proton; the broad one is the deuteron peak, as best we can determine it at the moment. If you subtract from the broad area the area under the sharp peak, then you get the residue that is due to the neutron. Our best number at the moment is that the neutron scattering amounts to $0.5 \pm 0.25$ of the scattering from a free proton. The number you get from the cross section formula, if you assume that the neutron's magnetic moment is spread out with the same functional behavior as the proton's, is $0.46$. Therefore we will say that to the first approximation the neutron's magnetic moment has the same extension as the proton's magnetic moment.

These numbers, $0.5 \pm 0.25$, can be put back into the form factor expression in order to determine a radius. Of course, this gives you a very much sharper definition because the form factor varies so violently with the radius. This enables you to say that the magnetic radius of the neutron is the same as that of the proton within $\pm 10\%$.
At small angles, the scattering by the neutron should be due to its charge, essentially. Since its charge is zero, you expect practically no scattering. This is found to be true. At 500 Mev and 90°, for example, it is very hard to find the neutron scattering; it is less than about a tenth of the proton scattering. So this is in accord with what one expects.

Finally we tried to find out whether this whole picture makes sense by seeing whether in slightly heavier nuclei one would find the neutrons behaving in the same ways. The case that suggests itself is Be⁹ because Be⁹ has a loose neutron on the outside. We therefore investigated Be⁹ and compared it with something fairly close to it, namely C¹². In order to make the comparison, however, you take, so to speak, 2/3rds of C¹², which is Be⁸. The assumption (and you will see perhaps how well it is borne out) is that under these conditions of high momentum transfer, all the nucleons behave independently and their scatterings add incoherently.

I want to show you first the results of the experiment on Be⁹. It was carried out about a year ago at 600 Mev and 135° by E. E. Chambers and the author.

Fig. 7 shows the Be⁹ cross section versus energy of the scattered electrons. The elastic peak (right-hand-end of the graph) is negligible, of course, under these conditions. The lower curve represents 2/3rds of the scatterings of C¹². You notice that there is a little area left over. That area can be compared with the scattering from a free proton, represented by the smaller square at the bottom. You see that they are comparable. If one had a point neutron, the area would be given by the large

![Graph showing the Be⁹ cross section versus energy of the scattered electrons.](image-url)
square in the lower left corner. So this is roughly in agreement with the other measurements, except that the neutron seems to scatter a little more than it does in the deuterium experiment.

Now we have a more accurate experiment at 500 Mev. Fig. 8 shows a curve for Be$^9$ and C$^{12}$ for the same number of nucleons in the target. The Be$^9$ curve shows a peak near that of the free proton. Normally, you would expect that with the momentum distribution in the Be$^9$ nucleus the peak should rise on the right and come down on the left, roughly symmetrically. It probably does; and this second peak to the left represents electrons which have been scattered in the Be$^9$ nucleus from the various nucleons and have produced mesons. The sharp drop on the far left is an instrumental cut off. It is easy to get the real curve, which no doubt falls off more slowly, but in these experiments we haven't gotten it so far. Of course, in these peaks you also have the momentum distribution of the nucleons folded in.
If you take 2/3rds of \( ^{12}\text{C} \) and compare it with the \( ^{9}\text{Be} \) curve, then you get the indicated area as in Fig. 9 as the difference. Comparison with the proton area gives you 1.1 for the ratio. That is approximately what the other experiments indicated.

We also investigated \( ^{6}\text{Li} \) and \( ^{7}\text{Li} \). These differ in that there is one more neutron in \( ^{7}\text{Li} \) than in \( ^{6}\text{Li} \). The data are shown in Fig. 10. The \( ^{6}\text{Li} \) data are the ones in squares and the \( ^{7}\text{Li} \), in circles. They are pretty close together all the way, but if you add up all the numbers you find that \( ^{7}\text{Li} \) is fairly consistently above \( ^{6}\text{Li} \). \( ^{7}\text{Li} \) scatters a bit more than \( ^{6}\text{Li} \), corresponding to a cross section ratio of 0.6. So it is in reasonable agreement with the other results.

I'll now make a final summary of what I think are the best values of cross section ratios at the moment, from all these experiments.
You observe that the scattering from a single neutron in a heavier
nucleus is more than the .5 of that of the proton, as in the deuteron.
But you have to apply a correction to the scattering from heavy
nuclei because of the binding energy. That is to say, the peak of
the momentum distribution curve in the heavier elements is always
shifted to lower energies. That corresponds, if you like, to scatter­
ing at slightly lower energies. The form factors are different and
you have to apply a correction amounting roughly to 30%. Any cross
section that you get from a heavy nucleus should therefore be
divided by about 1.3 to compare with the deuteron. The correspond­ing
correction in the deuteron is only about 6%. I don't know how big
the errors are. If you take all the nucleons together you get to the
numbers listed on the right. Roughly speaking, all the numbers are
consistent with the value for the deuteron. This gives the neutron
then a magnetic moment which has an extension within 10 or 15 per
cent of that of the proton.

These experiments will be continued. In particular, we will
go to smaller angles so that one can try to see the charge scattering
from the neutron. You can see that is going to be a difficult job.

Recent experiments with liquid deuterium and liquid hydrogen
targets favor the larger values of the $\sigma_n/\sigma_p$ ratios, as found in the
heavier nuclei. At $135^\circ$ and 500 Mev the present best ratio
is $1.0 \pm 0.2$. The low value $(0.5 \pm 0.25)$ reported for $CD_2$ above
is undoubtedly connected with large errors involved in making the
carbon subtraction and in possible depletion of deuterium in the $CD_2$
by radiation damage by the electron beam.

**DISCUSSION**

**OPPENHEIMER:** I have a question, but I am not sure that
you want to answer it in the form I put it. I believe that both for the
magnetization and the charge, you have very good values for the RMS
radius. In the case of the magnetization, you also have a fairly good
idea of the limits of hardness. What are the limits of hardness on
the charge? Let me put it like this: how much could this be a composite
structure with x% limited to the core? What would be the value of x?

**HOFSTADTER:** We really don't have enough data to answer
that question at all, but I hope sometime we will. That is obviously
one of the interesting things to look at. It requires study of the
scattering at smaller angles.

**SCHEIN:** Is there the possibility that the size and structure
of the electron in the scattering has to be taken into account? What
does one know about these questions?

**HOFSTADTER:** Well, the only thing that we have done is to
assume a point electron, allowing for radiative corrections. This
gives, in a sense, the size effects you are talking about. On the
theoretical aspects of that I think you should direct your question to
the experts.

**KLEIN:** Aren't you approaching an energy where a static
form factor is no longer applicable?

**HOFSTADTER:** Yes, I think so. Perhaps that is responsible
for the deviation (Figures 2 and 3) for large angles.

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**NAMBU:** Speculations on a new neutral meson

I would like to present here another attempt at explanation of
the nucleon charge distribution found in the Stanford electron scatter­
ing experiments. I will assume that there is another neutral meson,
which I shall call $\pi_0'$. It is a neutral vector meson of isotopic spin
zero, and a mass two to three times that of the ordinary pion.

If you assume the existence of such a meson, then it should
be able to decay into these modes:

(a) $\pi_0' \rightarrow \pi_0 + \gamma , 2\pi_0 + \gamma , \pi^+ + \pi^- + \gamma$

(b) $\pi_0' \rightarrow e^+ + e^- , \mu^+ + \mu^-$

(c) $\pi_0' \rightarrow \pi^+ + \pi^-$
As far as decay rates are concerned, the first is the dominant mode, while (b) and (c) are lower by a factor of $\alpha$.

Process (b) would mean that a nucleon can emit a virtual pi prime and this can interact with the electron.

That would give you an additional interaction between the nucleon and the electron. The coupling constant is the same for the proton and neutron in this process (1). The additional form factor arising from this interaction must have the same sign both for proton and neutron, whereas the usual pion cloud interaction (2) has opposite sign for proton and neutron. The range of this interaction would be roughly speaking of the order of one half the pion Compton wave length. Since in the case of the protons the two form factors add up, while in the case of neutrons they subtract and may cancel each other, the charge distribution for the proton could be big, whereas the charge distribution for the neutron could be small without bringing in consideration of the core.

Since I have introduced a new particle, of course I'll take full responsibility for all the consequences of this, and I'm afraid that the experimentalists will jump at me if I claim that such a possibility exists.

First, we consider the decay of the pi prime. The most rapid mode would be the decay (a) into a pi and a gamma, and the pi would also decay. Rough calculation shows that the life time of the pi prime for decay into this mode is of the order of $10^{-19} - 10^{-20}$ sec. So this is a very fast reaction. You would therefore not observe this particle directly, but certainly it would add a source of $\gamma$-rays.
in any high energy reaction. The energy of this particular gamma ray would be rather large. This is one of the consequences.

Second, at high energies the charge exchange reaction
\[ \pi^- + p \rightarrow \pi^0 + n \]
could occur. This might be responsible for the second maximum of the pion nucleon scattering around 800 Mev.

Third, there would be an additional nuclear force. Since this is an isotopic scalar and spacial vector particle, the force will be similar to the repulsive force between like charges. So this would be a repulsive force between two nucleons with a range \( \lesssim 0.7 \times 10^{-13} \text{cm} \).

Fourth, you have to consider also the contribution of the \( \pi_0' \) to the anomalous moment of the nucleon. I have not investigated the effect in detail. As you know, the anomalous moment consists of two contributions, namely, from the pion cloud and from the core. The core part consists of an isotopic scalar part and an isotopic vector part. The contribution due to the virtual emission and absorption of the new pi prime tends to cancel the isotopic scalar part of the core moment. I think that this is favorable to this theory.

Finally, I would say that if it is energetically possible, strange paricles should sometimes decay into pi prime and something else, for example, \( \gamma \) -rays.

DISCUSSION

PEIERLS: You seem to know so much about this particle; about its decay modes and lifetime. But one has the impression that the discrepancies that this was invented to cure are not yet very firmly established.

CHAMBERLAIN: Is there any comment about the force between nucleons and anti-nucleons?

NAMBU: This will be attractive because it has a property similar to the Coulomb interaction.

PICCIONI: How do you explain the second maximum? I see that \( T=1/2 \), which makes it go in the right direction, but why does it give a maximum? Secondly, would this process not be visible in the cloud chamber in the vicinity of the maximum because the cross section at that point is so large? We have not seen it. If this were the reason for the second maximum I think we could hardly have avoided seeing the gammas or the neutral pions.
NAMBU: Yes, I am aware of these difficulties.

YENNIE: There is a formal point here. This heavy vector particle would be similar to a photon in symmetry properties and very much like the photon introduced to cancel out the infinities in electrodynamics.

NAMBU: You mean the Feynman cut-off?

YENNIE: Yes. This sort of cut-off has the difficulty that probability is not conserved. In other words the electric charge would have to be essentially imaginary.

NAMBU: That kind of difficulty would be avoided by this theory. They Feynman cut-off particle does not have the property of decaying into other particles.

YENNIE: One way of checking up on this would be by positron scattering from the proton. This effect should have the opposite sign in the interference term, I believe.

NAMBU: I think that's right; I'm not sure.

GATTO: In the annihilation of an anti-nucleon against an emulsion nucleus the number of $\pi^0$ is approximately one half the number of charged $\pi$ by charge independence. Assuming for $\pi^0$ the same average energy as for charged $\pi$ one should still find a reasonable fraction of the total energy carried away by such $\pi^0$.

NAMBU: Yes, I know there are many weak points about this theory from an experimental point of view.

BERNARDINI: Compton effect on protons

I am presenting the results, and only the results, of an experiment on the Compton effect by protons in the energy region between the threshold for meson production and approximately two times the rest mass of the pion. This experiment was done by Hansen, Yamagata, Filosofo, Odian, Auerbach and myself. The experimental details can be found in the report of the CERN Conference of 1956.
In Fig. 11 we show only that we have reason to believe that the experiment is correct. As a by-product of the experiment on Compton scattering, an experiment on photo production of neutral pions has been done. This experiment checks very nicely the previous results obtained by Caltech as shown in Fig. 11, where our points are plotted together with the "strip" which locates the averages of the Caltech data.

In Fig. 12 we show the 90 degree Compton cross section in the center of mass system.

The data are plotted together with the experiment done previously at M. I. T. The trend of the curve is quite obvious. We can call it the anomalous dispersion of the proton because at 200 Mev and above the contribution due to the pionic structure of the nucleon is dominated by virtual transitions to the isobaric state, (the 3/2, 3/2 state) as discussed, for example, by Austern.
If you like to make a rough interpretation of the data, you take the Thomson amplitude in the center of mass system and add the amplitude associated with the isobaric state, which you can get directly from the photo-production of neutral pions. Then you take into account properly the interference effects between spin flip and non-spin flip terms, as Yamaguchi has done (unpublished). This yields the solid line which seems to fit pretty well.

The situation up to, say, this morning was such that you would like to improve this rough model by adding extra terms which are connected with the pionic structure of the nucleon, and also by considering the fact that the nucleon electromagnetically is not only a charge but is also a magnet. This magnetic moment is supposed to have form factors. The agreement between theory and experimental points (up to this morning) was getting worse and worse. The more terms you added, the worse was the agreement. To straighten the situation out we considered dispersion relations. We took as a guide a paper kindly sent to us by Capps. By practically repeating step by step Capps' evaluation, we found the dotted curve. In this calculation an amplitude associated with a point proton with anomalous moment has been added to amplitudes associated with pion nucleon intermediate states of angular momentum 1/2 and 3/2, connected with electric dipole and magnetic dipole absorption, respectively. Well, that was the situation and we though that this discrepancy required serious investigation.

In addition you may see in Fig. 13 that also the angular distribution was not in agreement with ex-
The theoretical curves are calculated with the dispersion relations of Capps. But this morning Dr. Chew mentioned to me that Zachariason, Watson, and Karplus had done some more refined calculations. According to Chew they used a cut off model and got a curve which at 90 degrees exactly fits the experimental data up to 240 Mev. The only thing I will mention explicitly is the low energy side of Fig. 12. There has been worry about the contribution of the electric dipole absorption at these energies. Evidently the Berkeley theorists obtained a minimum contribution around the meson production threshold, associated with the electric dipole effect. I don't know the origin of this conclusion.

TELEGDI: Compton effect on protons

These remarks might be of some interest to the theorists. The comparison with theory is particularly interesting in the low frequency limit, and in Chicago we have nothing but low frequency limit machines.

As we learned from Oxley last year, measurements of the Compton effect with some effort toward good statistics have been run in Chicago for the last two years. I have been associated with part of it, and the experiment was concluded during the last two months by Oxley, and I shall present the results. This part of the experiment and its method was reviewed both in last year's Rochester Conference and in the proceedings of the Pisa Conference.

Table of Oxley's Results

Cross Sections of the Proton Compton Effect in $10^{-32}$ cm$^2$/steradian at 65 Mev.

<table>
<thead>
<tr>
<th>ANGLE</th>
<th>EXP.</th>
<th>KLEIN-NISHINA</th>
<th>POWELL</th>
</tr>
</thead>
<tbody>
<tr>
<td>0°</td>
<td></td>
<td>2.37</td>
<td>2.44</td>
</tr>
<tr>
<td>70°</td>
<td>0.82 ± 0.08</td>
<td>1.26</td>
<td>1.41</td>
</tr>
<tr>
<td>90°</td>
<td>0.84 ± 0.03</td>
<td>1.04</td>
<td>1.28</td>
</tr>
<tr>
<td>120°</td>
<td>0.91 ± 0.04</td>
<td>1.21</td>
<td>1.52</td>
</tr>
<tr>
<td>150°</td>
<td>1.12 ± 0.05</td>
<td>1.62</td>
<td>1.95</td>
</tr>
<tr>
<td>180°</td>
<td></td>
<td>1.81</td>
<td>2.16</td>
</tr>
</tbody>
</table>
The Powell (J. L. Powell, Phys. Rev. 75, 32 (1949)) formula is the Klein-Nishina formula for a particle having an anomalous point magnetic moment.

You are all aware of the fact that it is particularly difficult to calibrate both incident and outgoing photon flux. In fact most of last year was spent mainly on this. Finally the calibration was done by different means, and also against an ion chamber that is being calorimetrically compared to the identical Bureau of Standards instrument. It is anticipated that due to this new calibration all experimental values will be raised by about 12 per cent.

In the experimental results reported earlier there had been a peculiar behavior at 70 degrees, an angle at which one has great difficulty doing measurements, as at all small angles. That difficulty has been removed by increasing the beam rates, and now there is a very flat behavior over all angles.

**DISCUSSION**

**BERNARDINI:** If this is right, you are again getting into troubles. Our interpretation of the photon scattering via the photo-production of neutral pions is based on the fact that in absorbing the photon, the nucleon is almost a rigid structure. That is, the electromagnetic properties of the nucleon are practically given by their low energy limits. Now in the Powell formula you have energy dependent terms. If you suppress them, you have to add a form factor. With such a form factor you no longer have agreement with the neutral pions!

**WEISSKOPF:** I would like to ask somebody from the Berkeley group to tell us more about the difference between the dispersion formulas of Capps which Bernardini presented and the new one by Zachariason et al.

**CHEW:** It is only a matter of a more careful calculation. The Capps calculation kept just electric dipole and magnetic dipole terms and the other calculation kept many more things. Also the calculation just finished was not based on the dispersion relations but just on the cut-off model.

**BERNARDINI:** How does the cut-off model handle S-wave pions? The cut-off model is essentially non-relativistic and the S-waves involve nucleon pairs.
CHEW: I think they have neglected the S-wave secondary scattering. There is no difficulty in handling S-wave photo-production terms.

WICK: Neutron-proton mass difference

My talk is very definitely on the structure of the nucleon, but it is not very definite. Several years ago Feynman and Speisman (Phys. Rev. 94, 500 (1954)) revived the hypothesis that the mass difference between the neutron and proton is of electromagnetic origin. They showed that the possibility of obtaining not only the correct order of magnitude, but also the right sign, was not as hopeless as it was thought to be. We have tried to look into the question of evaluating this mass difference a little more precisely. I must ask you to consider this attempt with some forebearance because it was made, not with the hope of finding a satisfactory number, but more to find out what the difficulties really are.

The idea is the following: First of all, we assume that electrodynamics does not break down at distances of the order of the Compton wave length of the nucleon as might be suggested by some of the results in Stanford. It seems to me that if one wants to discuss the assumption that the mass difference is entirely electromagnetic, it is more consistent to take this standpoint; for if there is a breakdown of electrodynamics at these distances it is likely that pure electromagnetic and non electromagnetic effects get scrambled up so that it may not be meaningful to ask if the mass difference is entirely electromagnetic. Anyhow this is the assumption we make. Then we ask the question: Can it be decided in some way whether the mass difference is entirely electromagnetic or not (without, of course, waiting for the time that will come some day when one knows everything and understands everything)? The idea is to try to combine some theoretical formula with empirical data which we are now obtaining from experiments on charge and current distributions around nucleons, to make an estimate of what the mass difference ought to be.

We assume that electrodynamic interactions can be treated as small perturbations. This is certainly reasonable, of course. So we write down a standard formula for the mass shift of a particle in terms of perturbation theory with respect to the electromagnetic interaction. Then it is easy to see that the mass shift for a nucleon is given by a simple extension of the classical formula which tells
you that the electrostatic energy of a charge distribution is obtained by multiplying the charge at one point by the charge at another point, dividing by the distance, and by two, and integrating over the two points. The generalized formula involves something slightly more complicated. It involves the electromagnetic current density at some point in space time and the current density at some other point in space time, where it is necessary to consider two points which are not only different in space but also in time. If you knew the expectation value of this object

$$\langle j_\mu(x) j_\mu(y) \rangle$$

for a nucleon, you could, by means of a simple integral calculate the mass shift. You might point out that in the case of meson interaction there is another term which I have not written down. That term is a little confusing to me, but one thing I am quite sure of: it cancels out when you take the neutron proton mass difference. So I will not worry about it.

Now the question is: Do we have information to evaluate such a thing? The answer, of course, is: we don't have enough information. This expression is directly connected to the Compton effect on the nucleon. I understand Dr. Goldberger had the idea to use some such connection, and also the connection which I will mention later, and Mr. Riesenfeld did some calculations; but I'm not well informed about what his standpoint really is. I don't think that the Compton effect can be used with great advantage to evaluate this.

The only possibility you're left with is to try to evaluate it in the usual manner, by introducing intermediate states and summing over all possible intermediate states which you know something about. Now that is of course the unfortunate part of the story. We don't know enough about the matrix elements of the current between the nucleon state and the intermediate states that you want to consider. You want to consider, for example, the single nucleon intermediate states and you would like to have intermediate states involving nucleon, anti-nucleon pairs. These two types of states play equally important roles in perturbation theory. But we don't know anything about the matrix elements for nucleon pair states. Then you have states with a nucleon and one meson or two mesons, and so on.

What we have done is very naive; namely, we have looked at the case of single nucleon intermediate states. That is rather simple because we have information on nucleon to nucleon matrix elements of the current from the Stanford results. They contain form factors, depending on the momentum transfer, which cause the matrix
elements to decrease rather rapidly. This yields a completely convergent result. However, not to our great surprise, we get something of almost the right value but with the wrong sign.

Then we tried to add something about the meson states. The one nucleon plus one meson intermediate states involve matrix elements which are rather well connected to the photo-meson cross section. However, the connection is a little more complicated than we would like, because what we need is something off the energy shell. The most unfortunate part is that here you cannot make the usual assumption that, owing to the resonant character of the photo-meson cross section, you can estimate things rather well, even though your information doesn't go to high energy. The resonant part of the effect for the mesons cancels out when you take the mass difference between neutron and proton. So what you really have to evaluate is the remaining small terms, which are not known too well and to sufficiently high energy to carry out the calculation. The values that we know, up to the energies to which we know them, give a very small contribution. But that doesn't mean that the one meson state will not ultimately give a large contribution.

Experiments not with protons but with electrons on meson production, if sufficiently detailed and subject to a careful analysis, would give more information about the matrix elements that we need. It would be very interesting to have these matrix elements. This is the very incomplete state of the question now.

DISCUSSION

KLEIN: I was a little surprised by your statement about the contribution from the single nucleon intermediate states. I would have guessed that this would correspond roughly to the Feynman-Speisman calculations.

WICK: No, because, you see, the cut-off is quite different. I do not assume I have the right to change the cut-off as I like. I take it from Stanford.

KLEIN: I see. Except for the fact that the cut-off is different, in principle this should be the same contribution.

WICK: No, not exactly. The Feynman cut-off was on the photon propagator. This is different. The fact that the neutron has such a small electric charge radius, makes it almost impossible to get the right sign from this term only. But there is no reason
why this term should give the complete answer.

**GOLDBERGER:** This one nucleon contribution should not be the same as the Feynman-Speisman calculation. Feynman and Speisman were using ordinary perturbation theory. So they also have the pairs.

**FELD:** If the neutron has such a small charge radius, and you have charge independence, then the proton core must be spread out to about the same size as the meson cloud. If that is so then I don't understand how you get much of a contribution, even of the same magnitude.

**WICK:** The core itself does not enter the calculation. I just use a form factor.

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**MATTHEWS:** Pion-nucleon scattering

I want to report on work done in collaboration with Dr. Edwards who will be the next speaker. We have developed a relativistic equation for the meson-nucleon scattering problem. The most familiar treatment of this problem is, of course, the perturbation theory treatment. One writes the T matrix as a power series in the coupling constant, symbolically,

\[ t = S^{(2)} + S^{(4)} + \ldots. \]

The equations which we have derived can be put in terms of Feynman diagrams symbolically, this way:

![Diagram](attachment:image.png)
The last term is rather a mess, but it can be written in closed form if one uses the functional derivatives with respect to the field. In the low energy limit that term can be shown to give almost entirely self energy effects; so in the first approximation one drops it.

You will notice that if one takes the first two terms only, then one has the so called ladder approximation to the Bethe-Salpeter equation. It doesn't look like a ladder in this problem. What is suggested by this equation is that one should take also the third term, which is mathematically not very much more complicated. The advantage of this equation is that it satisfies crossing symmetry.

We have solved this equation in an extremely crude form. The approximate solution we have can be expressed in terms of the two lowest order terms in the perturbation approximation, in the rather well known form

$$t = \frac{S^{(2)}}{1 - S^{(\omega)}S^{(2)}}$$

If you take the no recoil limit at the start of the problem, that is, if you work with the no recoil Hamiltonian right to begin with, then this formula was first produced, I think, by Cini and Fubini and was calculated with by Sartori and Wataghin. It then gives rise to the Chew-Low effective range formula for the P-wave phase shifts.

What we have done is to take the $\phi_5$ theory to start with. Then one can write down the perturbation expressions and renormalize them before taking the no recoil limit. In the other treatment, as was stressed in his introductory talk by Professor Chew, if you take the no recoil limit first, then one gets two parameters, $G^2$ and the cut-off, $\omega_{\text{max}}$. If you renormalize before taking the no recoil limit, then you get a theory which depends only on the coupling constant. Very much to our surprise we came out with the effective range formula. You have only one parameter now to fit the thing, and to our surprise it still gives correctly the $3, 3$ resonance in the correct place. This is the main result. The $G^2$ needed is the one which you want, of the order of 14.

Just before I came up here to talk, Dr. Low informed me of some work done by Dr. Wyld, Phys. Rev. 96, 1661 (1954), who has calculated $S^{(2)}$ and $S^{(4)}$ but apparently does not agree with us. Since no details are given it is hard to compare Wyld's calculation with ours. However it is evident from his Table 2 that he has not
satisfied crossing symmetry and his approximations are hence clearly different from ours.

There are also S-waves in our approximation, and they come out to be of the right order of magnitude, but no light whatever is thrown on the strong isotopic spin dependence of the S-wave. Dr. Edwards will be discussing this from a different point of view, but I will say that we did look at this from the point of view of crossing symmetry. If you put the recoil in as we've done in the rest of this work, then the crossing symmetry not only relates the different P-wave phases but it expresses relations between the S-waves and the P-waves. We thought this might throw some light on the isotopic spin dependence of the S-waves, but we haven't been able to find this. We have found, however, that due to this coupling the splitting between the $\delta_{33}$ phase and the $\delta_{31}$ phase, which are taken to be equal in the no recoil limit, can be linked to the S-wave phases. One finds that the difference between these two phases is of the order of one degree. The phases themselves are less than about five degrees, so that the splitting between these phases would appear to be of the same order of magnitude as the phases themselves. Any theory that attempts to deal with the small phase shifts must therefore take the S-waves and P-waves all into account, and it must take account of the correlations between the S and P-waves which are due to crossing symmetry. These effects are of the same order of magnitude as the effects you are trying to calculate.

EDWARDS: S-wave pion-nucleon scattering

This is the work of J. S. Langer and myself at Birmingham. In an attempt to get somewhere with the S-wave problem, we considered the effect of the virtual hyperon cloud which will be around the nucleon. There are many different interactions and the paper which follows mine will give a very similar kind of idea but not quite the same interaction as we've chosen.

If one introduces the hyperons then one would think that one has amplitudes where the $K$ and $\Sigma$ scatter, and also an amplitude in which a $\Xi$ interacts with a nucleon, but instead of coming out as a $\Xi$ and a nucleon, they come out as a $\Sigma$ and a $K$. Then you get, instead of the usual equations, a set of coupled equations. One eliminates them all except the first one and uses the value of the coupling constant required. The actual calculation of the equivalent term which enters when you introduce the hyperons involves graphs of the type...
with a virtual hyperon appearing in the meson-nucleon scattering, or one can have the virtual hyperon straddle the vertex part, or straddle the whole graph.

Even though you may not like this way of setting the problem up, what has been explicitly calculated is the first radiative corrections coming from virtual hyperons, sort of built into the first order scattering. We have used only the $\Sigma$ and K and no other hyperons. The coupling constant used for the $\Sigma - \pi$ interactions may be taken the same as for the pion-nucleon interaction, and it turns out that the effect of putting the hyperons in is of the opposite sign to the leading term in the previous T matrix, providing that one uses a pseudo-scalar coupling. That is, this kind of equation has the form

$$t_{rs} = A \delta_{rs} + B \frac{\omega}{m} [\tau_r, \tau_s]$$

The first term in ordinary pseudo-scalar pion-nucleon scattering comes out to be large; the second one, small. Experimentally it is the other way around: A is very small, and B predominates. If you put in the K and $\Sigma$ effects you find that there is an additional term which involves the coupling constants for the hyperon and the $\Sigma$, K and $\pi$ interactions. It has the opposite sign in A, but it agrees with the old sign in B. This is really no more than an indication. I'm not saying that this is a solution of the S-wave problem. But it shows that the hyperon interactions can come in with the opposite sign from the normal one. It is therefore possible that the strongly $\tau$-dependent behavior of the S-wave found experimentally is due to hyperon effects coming in even at the low energies where the experiments are done.

We have looked very briefly whether these things will effect the P-wave. Of course, it is not much good patching up the S-wave if one wrecks the P-wave. But as far as one can see it does not effect the resonance state at all.
DISCUSSION

PEIERLS: Would you say what the coupling constant is, that is required for the hyperon coupling?

WENTZEL: What are the types of vertex parts you put into the interactions?

EDWARDS: In answer to both questions, there are two types of vertices here:

The new coupling constant is the one for the sigma, K, nucleon interaction. I think it comes out to be about 1.8. Incidentally, if one drops the $\gamma_5$, one finds that the term changes sign. That is the point. The presence or absence of $\gamma_5$ determines the sign of the new term. Of course we have taken the choice which would cancel the $\hat{A}$ term.

APPENDIX

DALLAPORTA: Pion-Nucleon S-waves and K-mesons (Budini, Dallaporta, Fonda), abstract

Supposing that the pion-nucleon interaction goes through the K-meson field, (i.e. with parity doublets) one finds that the lowest orders of the S-Matrix for pion-nucleon elastic scattering are of fourth and sixth order in the $\pi$ - K, K-N, K-Y, coupling constants. For PS coupling at low energy, the fourth order term contributes principally to S-waves, the sixth principally to P-waves and S-waves. We find that the S-wave scattering amplitude dependencies on isotopic spin coming from the fourth and sixth order terms have opposite signs. For reasonable values of the coupling constants their sum can go practically to zero, in accordance with experimental results.