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MEETING ROMA -FRASCATI "AFTER GENEVA" (VERBALE DELLA RIUNIONE - ROMA, FRASCATI 9-10/7/1958)

MEETING ROMA - FRASCATI 'AFTER GENEVA'

Parte I

(Verbale della riunione del 9 Luglio 1958.)

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ISTITUTO NAZIONALE DI FISICA NUCLEARE
Laboratori di Frascati

MEETING ROMA - FRASCATI 'AFTER GENEVA'

Verbale della riunione del giorno 9.7.1958, presso l'Istituto di Fisica dell'Università di Roma. Presiede la riunione il Prof. G. Bernardini.

(Trascrizione a cura di C. Infante e A. Turrin).

Bernardini: We wanted to have here the Electron Synchrotron groups of Caltech and Cornell, in view of the exciting work now being done there.

This idea that Salvini had, to have a stay of the Geneva Conference to reconsider the program of these machines, was the most appropriate essentially because of a mistake in the organization of the Geneva Conference: a mistake for which I was mainly responsible. This mistake was the program: when the organizing Committee discussed the program (of the Geneva Conference), I had the feeling that the pion-nucleon physics wouldn't be represented by so many new contributions, as instead turned out to be the case. The mistake was a bad one: as a consequence, the session of pion physics were even crowded and essentially the only ones which were not so good as the others: in fact most speakers did not have time to present their material with an adeguate extension and depth.

I think that it's a good idea to repeat those sessions for these parts of pion physics particulary related to the synchrotrons, and to have time to go into the details of the experimental information and of the discussions. That's all I have to say.

Recent experimental activities at Cornell

Wilson: As you know, the Cornell Synchrotron, has been in operation for nearly two years now, with quite a good intensity and has been working almost all of the time. That is we work nearly 24 hours a day. We have difficulties now and then, but we seem to have gotten, over the worst ones.

The kind of experiments you do when you first have a machine are obvious. One wants to extend the measurements to higher energies, and you want to see the new particles which you can get at these high energies. In our case we can see to K, Λ particles and we can also see the $K-\Sigma$ production. First I'd like to talk about the $\gamma+p \to n+\pi^+$

production, and later about the π^0 production (at large an gles) while Cocconi will talk about the multiple production.

I also plan to say something about the K production, measurement and decay.

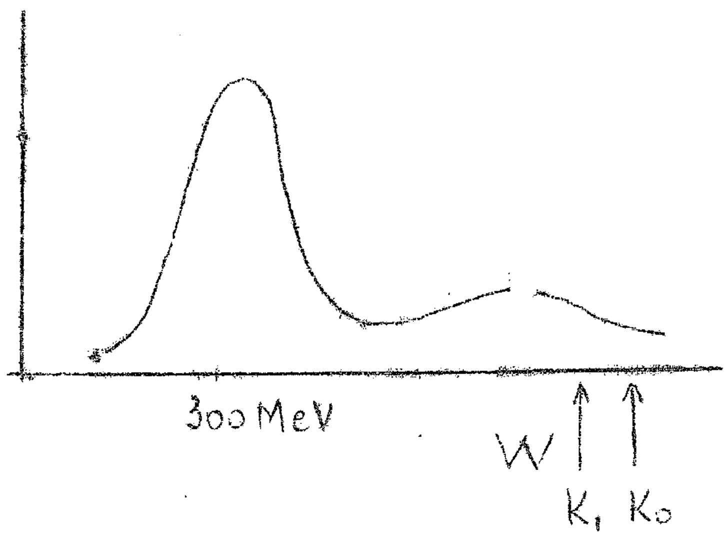
Bernardini: Please remember these are no stringent limits on time.

W.: About the $\gamma + p \longrightarrow n + \pi^+$ I'm sure you have all had an introduction to this, and know about the first peak at low energies. We were anxious to see if there was a high intensity of π^+ and to see how the cross-sections go.

I believe that this is one of our most productive sche dules, because even when we studied the γ - rays incident e.g. on Carbon, the first measurements we made were on π^+. The first study we did was on the reaction

$$\gamma + C \longrightarrow \pi^+ + ?$$

we observed a curve that had this character:



we were looking at the 80 MeV π^+. This was the very first curve we took, and it took us only one afternoon.

The curve shows the usual peak at an energy of about 300 MeV, and then it showed a small secondary peak.

When we first saw it, it rather mystified us: we thought it would be due to some degredation of high energy photons, but later we have come to think that there is a second peak: I will to try to give you the evidence we have for this. We think this second peak corresponds to a new excited sta te of the proton.

If this is true, it is the first important result we obtained in this energy region.

For studying this reaction, the equipment is just a continuation of the old equipment: that is a magnet to iden tify and measure the momentum of the π^+, a counter-telesco pe to discriminate the π^+ from the proton[1] (figure number 1 at the end).

It's necessary to measure the energy and the angle of the π^+ because it's a two-body interaction. But you must be sure that there 's not multiple production of π^+; in all cases the upper limit of the energy of the synchrotron is 150 MeV above the energy of the photon that we're looking at as bremsstrahlung. Thus there's not enough energy to make a multiple meson and have it come through our magnet too. Since we have a strong focusing synchrotron we had to have a strong focusing magnet: this is a two - lens magnet with a 2 m radius of curvature, it is 25 cm from the liquid hydrogen target (I might add we use liquid hydrogen in all our measurements). The cup of liquid hydrogen is about 8 cm long. The magnet has the property that it's double focussing, it focusses the mesons from the cup to a symmetrical point also 25 cm from the magnet. The aperture of the magnet is rather well defined by 15 cm-long lens slits. We have placed a counter at the entrance as a precaution: the counter also helps to define the geometry, but it did not make much difference. In other words scattering or penetration at the slits was not important at these energies. Then we have a Cerenkov counter; a counter telescope that consist of two small (1 ÷ 2 cm wide) scintillation counters, and this gives us two counting channels that count adjacent momentum channels. The magnet has a momentum resolution, such that 1 cm in the direction of the width of the counters represents 6% momentum change. When we want the make it 12% we use 2 cm counters. Depending on the counting rates, we can change things around.

Bernardini: How much was the solid angle?

W.: The solid angle is about 6'; I don't know the figure exactly: what we talk about is the

$$\int w \frac{dp}{p} = m \simeq 0,48 \times 10^{-3}$$

as I remember. This is a number that is independent of the excitation until we reach saturation.

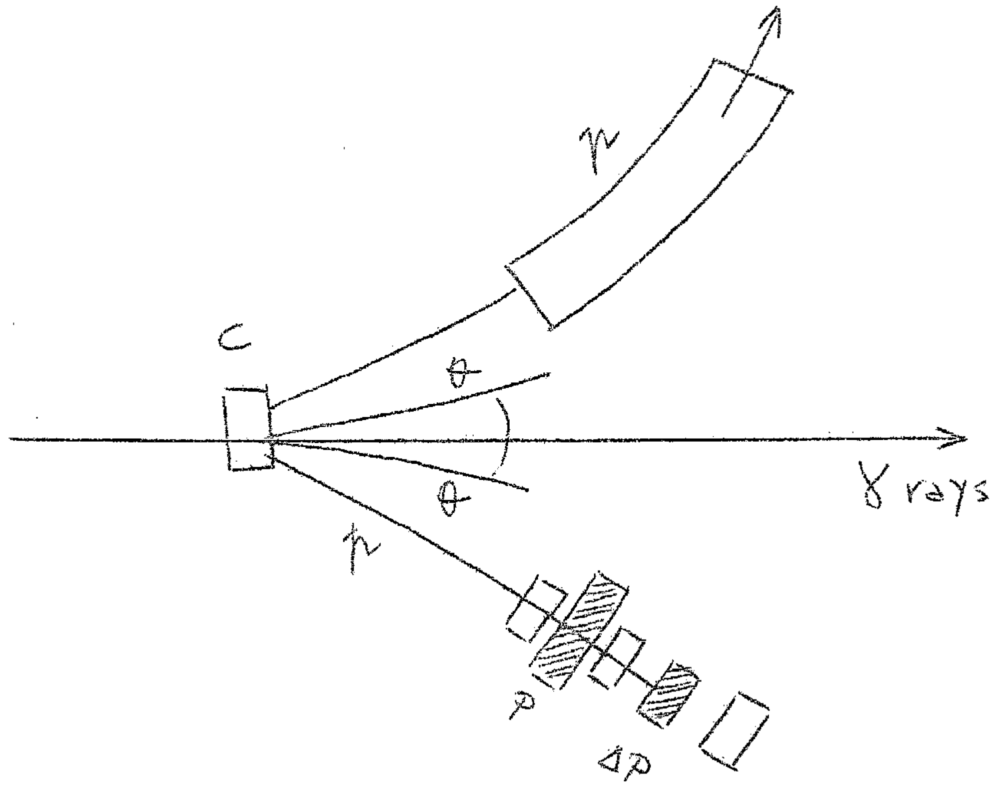
Budini: Over what range do you extend the integral?

W.: As you know, the acceptance is a function of momentum. That means that the integral extends over the momentum range you can detect: i.e. over the 6% range with a 1 cm counter. To get this number for our old magnet (that made measurements up to 300 MeV) we used the wire metod, in which a wire automatically plots the trajectories. In this way we can plot the radial angle as a function of excitation. It took us about two years and tree graduate students: we didn't have the heart to do it again for the new magnet. Instead we calibrated it to the old magnet at the energy where the two magnets corresponded: we measured the energy

1) M. Heinberg, W.M. McClelland F. Turkot W.M. Woodward, R.R. Wilson and D.M. Zipoy. Phys. Rev. 110, 1211 (1958).

of a particle at a given angle with the old magnet, and then at the same angle we did the same experiment with the new magnet and just cross-correlated the two. That was the first way to get this number.

The second method was with a counter telescope. The first method could onlytake us up to 80 MeV. To get up 600 MeV, we used protons for the calibration:



The γ -rays have wery good geometry; you might be 3 m away, and the size of the γ -rays beam might be the size of your finger. Because of the high-energy, there is very good definition of this beam. One can use a calorimeter to trim off the sides, so your beam is usually the size of your thumb or your finger, depending on how far away you are. Then we place a piece of C as a source of protons, on one side we put a magnet. On the other side we have a counter telescope which would define proton energies, with absorbers, a number of scintillation counters, some Cu absorbers, and an anticoincidence to show where the proton stopped. One of the absorbers defines the ΔP , another the momentum P: from the size of the counters we could determine the solid angle very well. So that we could determine the number m. The only uncertainty is the amount of absorption of the protons in the absorbers by the principle of uncertainty....

Bernardini: Excuse me: I have a source of protons of known momentum to make the calibration of the magnet: you consider two identical angles. The counter telescope gives the direct information on the momentum of the protons, and you use this to calibrate the magnet?

W.: Right: the same number of protons goes on both sides; one then calibrates a rather complicated magnet.

B.: Could you have put the magnet in the beam to calibrate it by the electrons (well defined in energy) in order to have a more intrinsic calibration?

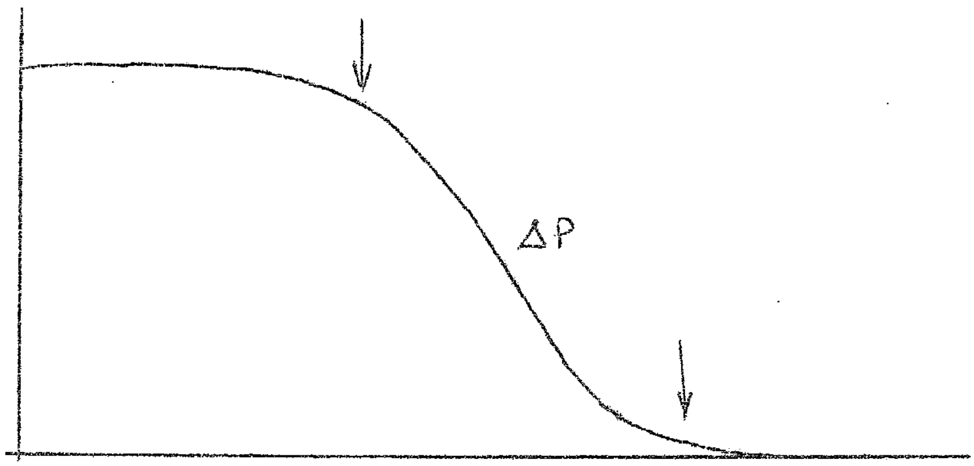
W.: Yes, but had we done that, because the angle of acceptance of the magnet would be large (a few degrees) compared to the angle at which the electrons are radiating out (10^{-3} rad).

B.: Could you have rotated the magnet?

W.: That would have given us too much information. We set a current in the magnet that defines the P. We set a range in the counter telescope that defined the same P: then we make the count, and we have the data to compute m.

Tollestrup: You must use a very accurate way of setting the energy, because the proton energy dependence is very steep.

W.: Right; behind the magnet we put a second counter telescope, very like the first one, with the same absorbers, and we make sure the counter cuts off. We do this on every measurement. We measure a curve of the range in Cu, like this:



The falling - off is Gaussian-like. Except that it's not a Gaussian, the width is greater than a Gaussian would have, and is determined by the ΔP. In fact, we get experimentally what the ΔP is for the magnet. When we get into saturation the ΔP is not determined completely by the relation

$$\int \omega \frac{dp}{p} = m = const$$

but it gets worse: but we always check this very carefully and make sure we determine the two points and the mean energy, and that that corresponds to our calibration: we're very careful about this, because we found that when we didn't, we got into trouble. I might say that the numbers we got in this way were the same, to about 10%, with the number that we got with our old measurements: furthermore we used ano-

ther check with the measurements at Caltech, as you'll see.
Cocconi: Do you know the geometry of the magnet well enough to calculate the $\frac{\Delta P}{P}$ and compare it with your results?
W.: It's about the same. You see the trouble with this magnet is that it's rather hard to get into it to make measurements. They're quite consistent, but it's rather hard to tell wich way the counts are going. I mean, you can tell when you're at the right momentum but when you're not then you're wild.

Let me show you the results of π^+ production:
(see fig. number 2 at the end)

Here we have the cross sections plotted as a function of the energy of the machine.

Incidentally I should talk about two other things that come into the measurements before talking about the results; one of these is the hydrogen target; but I won't waste your time by discussing this: the second thing is how to determine the quanta, but I'd like to leave that for a later discussion on collaboration. I'll simply say that we use a quanta-meter for measuring the equivalent quanta for each experiment.

The most spectacular result is that at 45° c.m. we see that after the first peak we've always regarded as very large (the low-energy part of the curve contains old measurements made at Caltech and Cornell) we see a second peak about as high as the first one. I believe you hardly have to argue about the presence of a second peak.

Especially if you keep in mind something like the De Broglie wavelength of the proton or the wave-length of the photon, a curve that goes down like $1/\lambda^2$. This curve could be dropping down very rapidly, and then in your imagination this becomes a larger effect. We'll see the implications later on; just now I want to say that this is a large effect. At 90° in the center of mass there is still some evidence for a peak especially as regards the dropping as $1/\lambda^2$. There's not much evidence of a peak in the back-ward direction. This summarized the results. Another curve shows how the data fits older data at lower energies (old Cornell, new Cornell).

I show these to show the momentum width involved; as you go to the higer energies the momentum width gets larger: as you see, the curves fit exactly: this was no accident because we used the same magnet.

I'll go on now on the π^0 experiments: I'm reporting on somebody else's measurements: there were two groups: De Wire, Littauer, Jackson[2] in the first and Stein and Rogers[3] in the second group.

I'm going to talk about De Wire and Littauer because their experiments is more complete. I will not spend too much time

2) - J.W. DeWire, H.E. Jackson and Littauer R.; Phys. Rev. 110, 1208 (1958).
3) - P.C. Stein and K.C. Rogers; Phys. Rev. 110, 1209 (1958)

on Stein and Rogers: they got the same results anyway. DeWire and Littauer have used the experimental arrangement that is pratical for this work: (figure number 3 at the end)

Here is the γ-rays; the synchrotron is off to one side.

There is a wall of concrete 1/2 meter thick, many meters high and wide: a lead slit inside; usually, there's a further collimator 1 m in front of this one. That is, the beam is fairly well cleaned of electrons and other things befor it gets into the experimental area. The liquid Hydrogen was placed behind this (At some stage I'd like to talk about our target, maybe later, because I think we've had a new development in this at Cornell; this was due mainly to DrLittauer who introduced a lot of simplicity into this business). In any case there's a cup containing liquid Hydrogen, and again the cup is about 7 cm long.

Here's the quantameter that monitors the number of photons. In this case, instead of looking at the π^+ (as we did before) one looks at the recoil proton to define the

$$\gamma + p \longrightarrow \pi^o + P$$

We have a counter telescope to define the energy and the angle of the proton: with this they define this two-body process.

They used a large glass Cerenkov Counter, just to make sure that indeed they are looking at protons coming from the process: you'll notice that same of the adsorbers are tapered. This is due to the kinematics of the process: you know that the protons go forward fairly well: but depending on the angle of emission of the π^o (or the angle of the proton) there's a very big change in energy over a very small angle range. So protons coming from a small angle have a very high momentum and thus a large range. This was taken into account by tapering these absorbers, so that one is always looking at the same energy of the protons, no matter at what angle the protons come out. In other words you're looking at the same energy of the protons as determined by the Bremsstrahlung spectrum. Then we have a Cerenkov counter that measure the π^o.

Bernardini: Let's say we have a monochromatic photon: thus if you take one angle or another, then you have a different proton energy and you can taper it. But you have a continuous spectrum, and then you also have low energy protons.

Wilson: No. You do have a continuous distribution of photon energy of course; as you said, if you had a single photon energy, every thing would be all right.

But they work back wards: they pick out a single proton energy, and go back to a single photon energy.

If we did not do that, we would pick out a band of energies, wich would be wider than if we tapered the absorbers.

All this if we were sure that any proton coming out of the liquid Hydrogen was due to the production of π^o.

But that's not true of course. At these very high energies you can run into multiple production, i.e. you can produce $\gamma + p \longrightarrow \pi^+ + \pi^- + p$.

This has a big cross section, and you can get into trouble if you don't take any precautions against multiple production. Here we do this by looking at the γ - rays resulting from the decay of the π^o. There's a Cerenkov counter ($\sim$20 cm diameter; 30 cm long), there's also a small counter in anticoincidence, so that a electron or charged meson will not come in and give rise to a back ground. All this is well shielded. This counter has been calibrated by putting monoenergetic electrons into it; one knows the width that is rougly 30 - 40% of it. De Wire and Littauer have made exhausting studies by looking at a given energy of the protons (defined by the photon energy) then they varied the energy of the machine so that these protons could not be made, but leaving all of the geometry invaried.

Then they saw that the counts disappeared; they can also study distribution of these pulses. You see, they should correspond to a certain γ - ray energy in general; when they were above the threshold they found a curve of number of counts versus pulse height. When they dropped the energy of the photons below the energy you're looking at, there should be no count at all, and the curve they find does not correspond to monoenergetic protons at all. What I want to say is that when they change the energy of the machine (maximum energy of the Bremsstrahlung spectrum) to bring it <u>below</u> the band of proton energies they were looking at, they saw no pulses in the Cerenkov counter: so that all the details of the experiment seem to be well under control. Furthemore we never look at a proton energy more than 150 MeV below the upper electron energy, so that there would be no possibility of counting a proton due to a multiple production.

When we didn't do that we did see the effects of multiple pion production These effects are large, but one can reduce them to 0 by working close to the upper limit of the Bremsstrahlung spectrum.

Now let me show you the results of their work (figure number 4 on the end).

The agreement with Stein and Rogers is very good, and the curves lit very well at low energies. It's not so evident this time but there seems to be a peak, at about 750 MeV, which is essentially the same energy of the peak in the π^+ production (fig. number 2 at the end). While the π^+ had a preference to forward angles, this seems to be more symmetrical.

<u>Bernardini:</u> The angular distribution of the π^o seems to be in the reverse direction of the π^+.

<u>W.:</u> Yes. Let me show another curve (Fig. number 5 at the end). This contains the same data you've just seen plotted, together with some Caltech data. This curve is one of the things

you might expect if you believe that there's a resonant state of the proton. On the other hand, this is an S wave, which is one of the simplest waves that depends on the multipolarity of the absorption of the γ-rays, with an angular momentum of 5/2, and this would be the simplest curve you can get, an S wave. This is just to show an intercomparison: in other words the data are not inconsistent with a $2 + 3 \sin^2 \theta$ angular distribution in vicinity of a peack, at 750, 650, 550, 850, 950 MeV (fig. number 6 at the end). The data are due to Stein and Rogers, together with De Wire and Littauer.

The triangles are the data from Caltech. The results are more or less the same, and not inconsistent with a $2 + 3 \sin^2 \theta$ distribution; this is an important point to keep in mind in view of later discussions. My only interpretation is that the distribution is a $2 + 3 \sin^2 \theta$ law at low energies, changing as you go up to higher energies.

Budini: How many anticoincidences did you register?

W.: I'm quite sure there were a lot of anticoincidences due to charged mesons, coming out from multiple processes. I didn't work on the experiment myself, so I can't state the exact number.

Conversi: Charged mesons could not influence the results because of energy considerations.

W.: Yes, although a continuous energy spectrum is present. I'm sure though that the $\gamma + p \longrightarrow p + \pi^- + \pi^+$ did not affect the results too much. I mean we looked for this reaction in counter experiments, but these experiments are extremely hard to make. I think most of the counts will be accidentals of some sorts, as from electrons.

Bernardini: From the numbers of these counts you know how much the set-up has been silent, so that you can correct for this. Even though these anticoincidence experiments may take up a lot of time, you can get a cross-section for the effect, and make a correction.

To make a definite point on Budini's question, can you expect any difficulty from the double pion production? No, because you look at the decay photon, and with a pulse height check you can rule out any complication due to this effect.

W.: Double pions cannot be made, because we always make the difference between the maximum energy of the electrons E_0 and the proton energy W less than 150 MeV, so that there's not enough energy to make a multiple meson.

B.: Otherwise you'd have a lot of trouble with these tapered adsorbers.

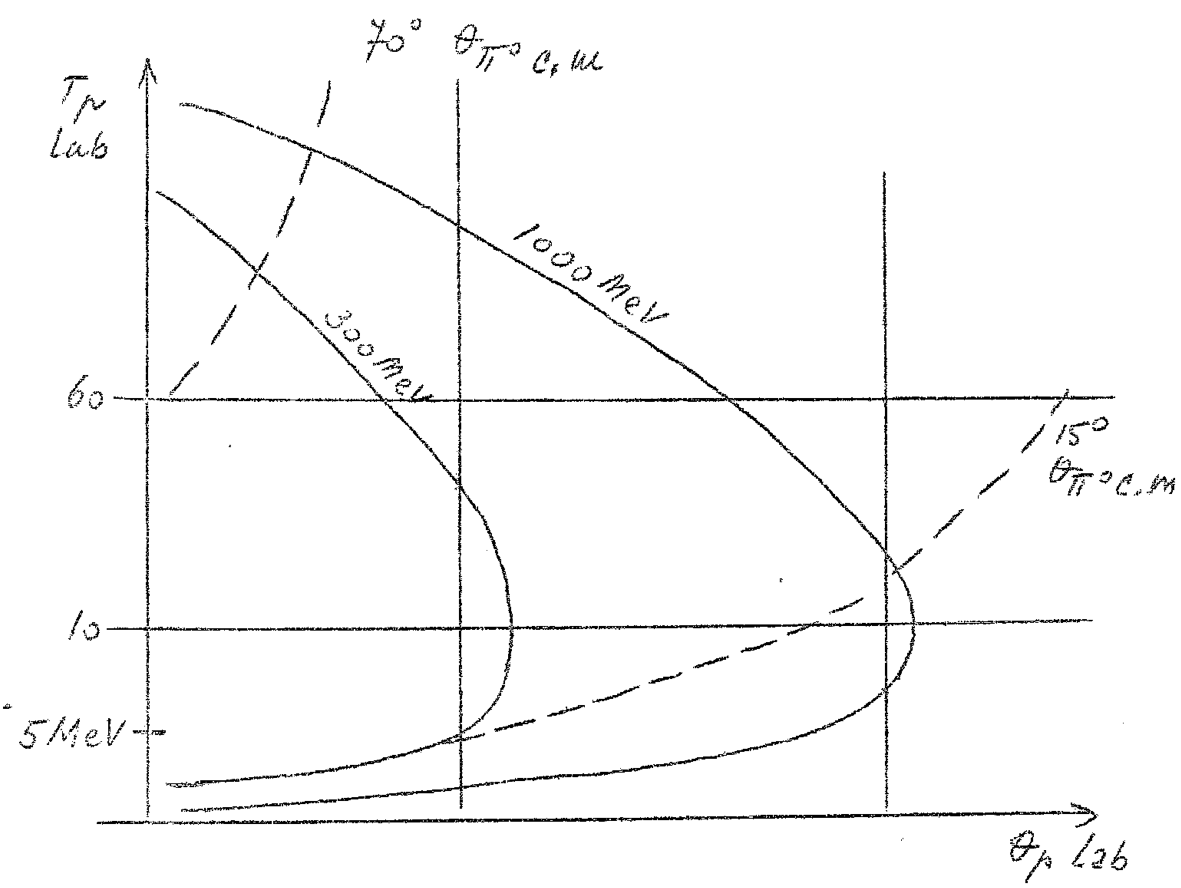
W.: Yes, you see they made a check. Keeping W the same, they let E_0 increase, and the counts did increase, coming perhaps from multiple production. You can have

$$\gamma + p \longrightarrow p + \pi^0 + \pi^0$$

for example.

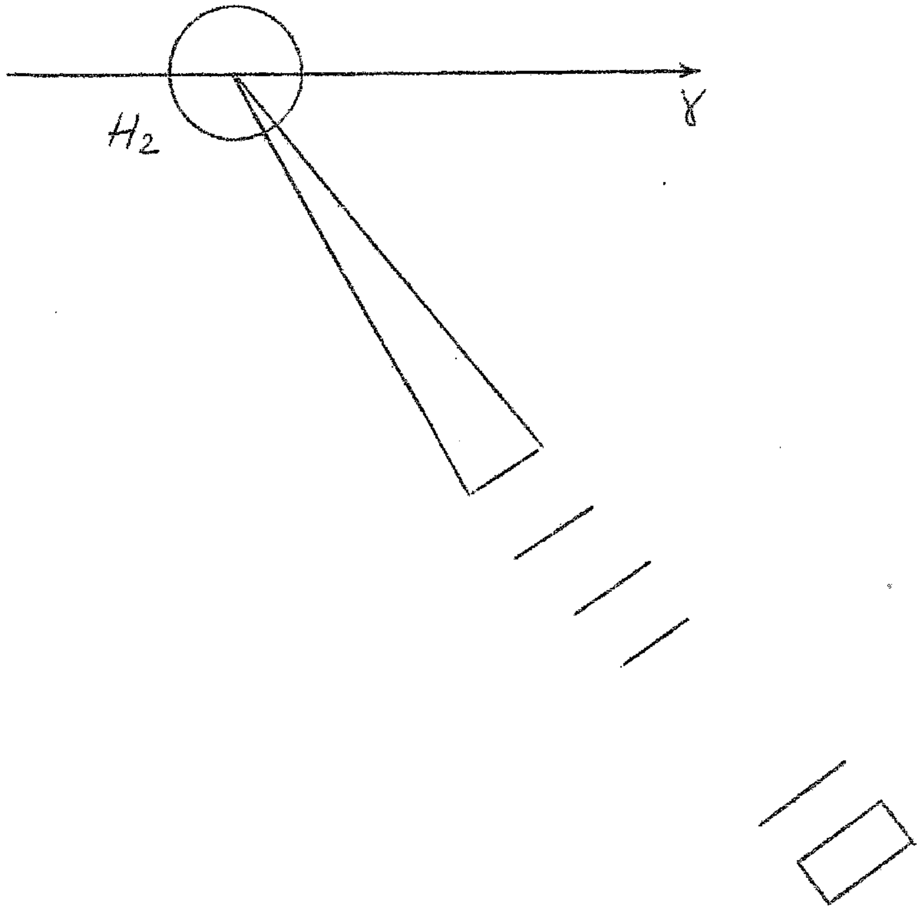
Bernardini: Now we hear from Professor Corson.

Corson: I'm very happy to be here. I'll talk about an experiment to try to improve the angular distribution in the π^0 process, which Professor Wilson has just been talking about. The trouble in measuring good angular distributions is the fact that when one detects the protons, for very forward π^0 angles, the protons come out backward (c.m. system) and have very low energy in the laboratory. I can show this if I sketch some kinematics for the reaction. If I plot the proton kinetic energy in the laboratory versus the laboratory angle of the proton, then there are curves that look something like this, for a certain photon energy:



Let's say the first is for 300 MeV, and the second for 1000 MeV. I'll indicate a π^0 c.m. angle, say 70° and 15° in the c.m. If you want to measure your forward π^0 angles, in order to get angular distributions with data near 0°, then we have to measure very low-energy protons and the peaks of these curves all come at about 10 MeV. So, the experiment that I'm concerned with, takes out a piece of this kinematics diagram, that goes from 5 to 60 MeV, while the laboratory angles are from about 50° to 70°. I tried to do this in the past with the Caltech people: Peterson, McDonald and I did it with photographic emulsions, and it's a very difficult job to compile statistically significant results with photographic emulsions.

So here we're trying to do an experiment with counters; this is easy to do statistically but very, hard counterwise. The people who have worked on this are primarily Mr. Berkelman and Mr. Waggoner, with some help from me. The basic arrangement is the following:

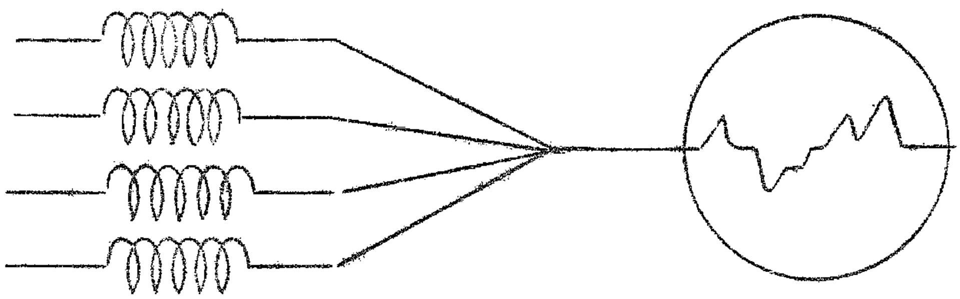


You have a γ-rays beam, there's a liquid Hydrogen target (we have three different kinds of Hydrogen targets at Cornell: this is modelled after the Illinois target).

It's about 1 cm in diameter, made of Mylar, and it's walls are 0,25 mm. I might give a warning about these targets. We had a lot of trouble with background we couldn't understand: so we put a transparent window, so that we could look at the Hydrogen while it was in the target. What we saw was very dismaying. It was full of dirt; in the first place, when the target was filling there was a lot of stuff that looked like snow, presumably contamination from air, exygen, nytrogen, still left in the chamber after it was evacuated, or from contaminations from the helium which was used to flush the target. So we had to be very careful to get a very high vacuum in the chamber before filling it, and then not flush it with dirty He.

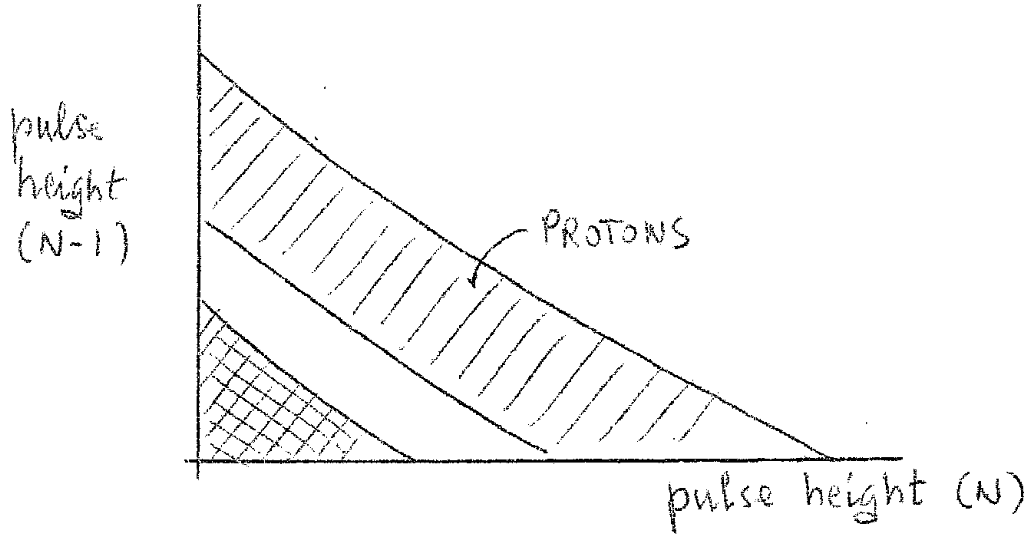
We arranged the device at various angles in the laboratory, from 50° to about 70°, with a counter telescope of eight counters in a row of varying thickness. The first one defines the angle and is just thick enough so that it will stop a 5 MeV proton (0,3 mm thick). The details on how to arran ge this in order to get the light out, and to have uniform sensitivity over the whole area in the counter I won't go into here. If anyone 's interested I'll discuss it with you. It took a lot of effort to get a good light collecting system that was successful in this score. The counters are about 2 cm wide and about 15÷20cm long. The angle in this direction is defined to about 2° (in the other it doesn't matter). The second counter is thick enough to stop a 10 MeV proton (going through both counters); the next one stops at 20 up to the last one that stops a 60 MeV proton. There's an anticoincidence counter behind. There can be no adsorber between the target and the counter telescope, so that it's all in vacuum. The light reflectors in this counter arran gement are in evaporated Al on the surface of the plastic scintillator and then, after the thinnest counters are pas sed there is a thin Al diagonal reflector.

Each of these is filled with light-pipes that take the light out to the sides alternatively on one side and then the other. These counters then come through delay lines of varying length. The delay in each is about 30 ÷ 40 mμsec successively.



In other words zero for the first; 40,80 and so on. Then they're all mixed together in a suitable mixer and then go to an oscilloscope, where they're displayed on a trace and the trace is photographed. And some counters are displayed with one sign of pulse, and others with the opposite sign.

By observing which pulses are present one determines the range of the particle and thereby its energy: by observing the pulse height one discriminates protons from mesons or electrons, wich get in the telescope in a great number. If one plots, in just the last two counters, the pulse height in the N - 1 counter versus the pulse height in the N coun ter one gets a curve that looks like something like this:



There 's a band of pulses of course, as in these very thin counters there's big statistical fluctuation in pulse heights, the worst of course is the 5 MeV counter. The width of the pulse height curve for a particle that just stops in that counter is about 20%. Great deal of effort has been made in selecting F.M. tubes to get ones with big gains and lots of photo-electrons from the first surface; this band will be the protons, while the mesons and the electrons will fall down in the small region. One attempts to set the biases on these tubes so that these particles don't count at all.

I have oscillograph photos but I don't think I should go into any more details. These have been two successful runs of this apparatus. The results are uncertain because of the background difficulties I mentioned: not with standing our efforts the background is still 30 ÷ 40% of the counting rate at least for the low energy protons. We've put in a very thin Mylar target, and tried to remove other sources of background so as to have a cleaner beam. I should say this is not an absolute determination of the cross section, as the beam was larger than the target. What we did was to measure the ratio of cross sections at the peak of the resonance curve at 300 MeV. This is arranged so that we tie on to the previous Caltech and Cornell data. The preliminary results are in rather good agreement, so far, in the E_γ region 400 ÷ 500 MeV, with the combined Cornell-Caltech results previously found and summarized in the work of Peterson and McDonald, except at the most forward angles where we did not find as many counts as predicted from the earlier data: but it's too early to say if it's significant or not.

Amaldi: How long did it take you to work out an event?

Corson: Too long: one projects on a microfilm projector, and one has a paper scale, and you read two numbers off: one can read a thousand events a day. It's far easier than working with emulsions, but it's not as easy as reading a dial, when you work with counters.

Bernardini: Excuse me, Corson, but I undertand you've had to make a lot of corrections even for a relative measurement: because you said the first counter defines the solid angle, and you have to take into account effects like straggling, nuclear adsorptions, scattering etc. For the very last counter the correction should amount to something like 30%.

Corson: I don't know how large the correctinn is, and we haven't had time to make these calculations yet, but these are sure to be difficulties.

B.: Because at least once we tried to make an estimate e.g. for protons, taking all sort of processes into account, of the amount of correction needed: these calculations were made by a digital computer: the corrections were rather appreciable after, say the fifth counter.

C.: Scattering is not very serious, because of the low Z material.

B.: I guess the thickness of each counter is about 5 mm, so five counters are 2,5 cm; now taking into account the nuclear interactions the protons might suffer on the way, I seem to remember that the corrections come up to 20 - 30%, after about 3 cm of scintillators.

C.: The effect for scattering angle becomes appreciable after the protons slows down to a few MeV, in order to be at the last counter, since the other counters are much wider than the first one.

I'm sorry if it wasn't apparent from the drawing.

Cocconi: The measurements I'm going to speak about have been made by a group of people whom I represent. The are Sellen, Vanda Cocconi, Hart and myself.

The measurements were dose in a diffusion chamber, and they concerned the reactions

$$\gamma + p \longrightarrow p + \pi^+ + \pi^-$$
$$\gamma + p \longrightarrow p + \pi^+ + \pi^- + \pi^o$$
$$\gamma + p \longrightarrow n + \pi^+ + \pi^+ + \pi^-.$$

(The exposition follows the lines of the Letter to the Editor: Cross Sections for Double and Triple Meson Production in Hydrogen by Photons with Energies up to 1 Bev.

J.M. Sellen, G. Cocconi, V.T. Cocconi and E.L. Hart; Phys. Rev. 110, 779 (1958) Slides (fig. 7 - 12 at the end) are shown on this experiment)

I will now turn to the analysis of the experimental data. As we know pratically every thing for each event (i.e. energy, momentum, charge of each particle) we can analyze each event by several ways. Now, it's easy to understand that we're essentially flat people: in other words we see a two-body process very well, but as soon as you turn to space everything becomes complicated because something is always going in the wrong direction. Accordingly, we analysed the events as if they were two-body processes. What we did was to assume, just for the sake of argument, that the reaction $\gamma + p \rightarrow p + \pi^+ + \pi^-$ goes through <u>two</u> two-body processes.

Assume e.g. that the following reaction takes place

$$\gamma + p \rightarrow (p+\pi^+) + \pi^-$$
$$(p+\pi^+) \rightarrow p + \pi^+$$

where $(p+\pi^+)$ denotes the mass of the intermediate-state particle. We have thus broken up a three-body process in two two-body processes. The reaction $(p + \pi^+) \rightarrow p + \pi^+$ for a certain Q, we shall call $Q_{\text{proton}\ \pi^+}$; now, the reaction under study can be analyzed in different ways; we have thus:

$$\gamma + p \rightarrow (p + \pi^+) + \pi^-$$
$$(p + \pi^+) \rightarrow p + \pi^+$$

$$\gamma + p \rightarrow (p + \pi^-) + \pi^+$$
$$(p + \pi^-) \rightarrow p + \pi^-$$

$$\gamma + p \rightarrow (\pi^+ + \pi^-) + p$$
$$(\pi^+ + \pi^-) \rightarrow \pi^+ + \pi^-$$

In other words, each event can be analyzed in three different ways; for each there'll be a certain Q; i.e.

$$Q_{\text{proton}\ \pi^+},\ Q_{\text{proton}\ \pi^-},\ Q_{\pi^+\ \pi^-}$$

From now on I will concern myself with the 77 events due to photo energies between 500 and 700 MeV which is the region of the so called maximum. Now, we plotted the Q of these processes: from pure statistics one should expect to find no preference for any single process. What we plotted is the number of events <u>versus</u> cos $\theta_{c.m.}$ ($\theta_{c.m.}$ is the angle of emission); and the number of events <u>versus</u> Q in MeV, for the various processes. The results are (fig. number 13 and 14 at the end).

As you see, the proton - π^+ shows a preference for high momenta, while the proton - π^- prefer <u>low</u> momenta.

The statistics are poor of course, but I have the feeling that these results are meaningful; I mean it's rather difficult to decide. This is only true because the momentum distribution finds the same state: as far as these 20 events can say something, they say this; I'm not ready to commit myself on this point. The $\pi^+ - \pi^-$ doesn't contribute or show very much. When we discussed the interaction energy of the $\pi^+ - \pi^-$ in Geneva, may be I could have said that, if I believe these curves, the π^+ and π^- try come out.

<u>Touschek</u>: If I may make a remark, I think an answer may come

from the τ decay.

Cocconi: It might be a good idea.

Bernardini: Supposing we don't have an isobaric state, than these curves would be flat?

C.: Yes: you see if I had a better energy definition, I could see the isobaric state near the resonance: still, my energy definition was not good enough.

If in this energy region the transistinn is helped by a $\frac{3}{2}, \frac{3}{2}$ state, this state is certainly not a pure $\frac{3}{2}, \frac{3}{2}$ state, since there are certainly some second-order effects that contribute to the interaction, so as to create some asimmetries. You see that the interaction is quite complex, and cannot be described in simple terms, since more than one state is responsible for the transition, if you believe our curves. Let me say that Prof. Morpurgo suggested that some more information could be had from our results: in the sense that in this two-body representation we measure the distribution of the plane of decay of this intermediate body. With some information on this plane of information of this body, we could measure the spin. This we haven't done yet, but Vanda will probablywork on it. After a conference at Brookhaven, in which a lot of emphasis was placed on digital computers, Vanda Cocconi came back very enthusiastic, and proposed to change our program completely so as to do everything with one of these computers. Now I think she has completed the programming of the machine: while it used to take us about two days for each event, it can now be done in 5 minutes. I suggest you try to work with these things, because do seem to savoà lot of time. The other thing I want to say is this: I have been asked by a lot of people, whether a diffusion chamber is better for this type of experiments than a bubble chamber. My answer is that if one had a bubble chamber with a depth of about 20 cm, then a bubble chamber would be definitely superior; no doubt: this is due to the fact that the tracks are sometimes very short, so that you can measure the angles well, but not the curvature, so that you can't tell whether you have a π^+ or a π^-. If you have a good magnetic field it can be done, of course, but the device becomes expensive, and that's why we didn't use such a chamber. Furthemore the diffusion chamber is still useful, I'd say superior to anything else, in electromagnetic work because of its simplicity and because it takes everything in. As far as intensity goes, the bubble may be better, because the electron pair have to be separated and that entails a loss in intensity.

At Cornell we have a group of people working with photographic emulsions. I thought I'd tell you about their work so that you can think about it.

One group is thinking of measuring an e.m. quadruplet, i.e., e.m. production of two pairs of electrons in the field of a proton. Now the theory gives

$$\frac{\sigma_{quadr.}}{\sigma_{pair}} \simeq \frac{k}{\alpha^2} \ln \frac{E_\gamma}{mc^2}, \text{ where k is a constant.}$$

Now Mr Connelly (?) who's doing this work, so far has found zero quadruplets against 20,000 pairs, while the theory predicts about one quadruplet against 3,000 pairs. On the other hand, Russian working on a shower which had produced a great number of pairs, found 3 quadruplets in only one shower.

Bernardini: Are there any remarks or questions concerning the reports of Wilson, Corson and Cocconi?

Wilson: I'd like to make some remarks about our second peak. From the peaks, that are so evident in the π^+ and the π^o photo production, it seemed to us at Cornell that this was perhaps a manifestation of a new excited state of the proton. So where as before I was trying to present the data objectively, I'm going to be prejudiced and say that these do represent a new excited state of the proton and then see what one can say about that excited state. Now then, from the position of the peak we can say that this state comes at about 600 MeV. So we begin to draw an energy level diagram for the proton and try to make an assignment of the various quantum numbers of these states:

E		T	J	P
600	————————	$\frac{1}{2}$	$\frac{3}{2}$	?
300	————————	$\frac{3}{2}$	$\frac{3}{2}$	+
n p	————————	$\frac{1}{2}$	$\frac{1}{2}$	+

The first one is the n p state, then the 300 MeV excited state. This is of course in the c.m. system: we know that the first peak has $\frac{3}{2}$ isotopic spin and $\frac{3}{2}$ total angular momentum, and plus parity, while the n p has $T = \frac{1}{2}$, $J = \frac{1}{2}$ and by definition P = +. We interpret thus the data we've seen this morning as E = 600 MeV, and I'd like to state it has a $T = \frac{1}{2}$, an angular momentum $J = \frac{3}{2}$* and we'll have to see what the parity is. We know the isospin of this state; there are at least two bits of evidence for this.

In the case of the $\frac{3}{2}$ you'll remember you have

$\gamma + p \longrightarrow \pi^+ + n$,

$\gamma + p \longrightarrow \pi^o + p$

In this case the Clebsch - Gordan coefficients for decay, which tell whether it'll decay into a π^+ or a π^o gave the ratio as 1 : 2. In this new state we saw that the total cross section for π^+ was about 80 μ barns, and the total cross section for the π^o was about 40 μ barns: the ratio has just flip

---

* - This is a weaker statement

ped, instead of 1 : 2 they're more nearly 1 : 2. The ratio does actually come out to be 2 : 1. That's a bit embarassing because you'll be asking me well how about the direct production. On the other hand it's not at all inconsistent with say 40 : 80; there's factor 4 involved depending on whether the isospin is $\frac{1}{2}$ or $\frac{3}{2}$.

It's much more nearly 2 : 1 than it was 1 : 2. This incidentally is the reason why the second peak in the π^+ production is nearly as high as the first one at 45°: infact the isospin happens to work against the first peak and <u>for</u> the second one; this explains the rather dramatic result in the π^+ production at 45° c.m. The second bit of evidence that the isospin is $\frac{1}{2}$ is the following:

If we look at the π^+ scattering, as measured by Cool, Piccioni and Clark (Phys. Rev. 103, 1082 (1956)).

(see fig. number 15 on the end)

the curve comes down to a very low minimum and then goes up to a peak at higer energy.

If you now take our data and ask where the 600 MeV peak comes, it would come at an energy of about 450 MeV: and - 450 MeV for the scattering of the π^+ is right at the minimum of this curve.

So, had there been a state $\frac{3}{2}$, we would expect to see the second peak occurring in the π^+ scattering. But of course the π^+ beam is a pure $\frac{3}{2}$ state, there is no $\frac{1}{2}$ state, therefore the isospin is $\frac{1}{2}$, we would not excite it all, and not see anything peculiar: i.e. the π^+ scattering is quite consistent with our assignment of isospin $\frac{1}{2}$.

Turning to the π^- scattering, where the situation is a bit more complicated:

(see fig. number 15 at the end).

The graph shows the total cross section of the proton $-\pi^-$ scattering, due to the work of Cool, Piccioni and Clark.

The energy scale is the equivalent photon energy, i.e. W is just the pion energy plus 150 MeV. Since we are interested in photons, this is the proper energy to compare it with. If we say that there is a resonance here, and if we make an assignment of the reduced width of that resonance, then we're forced to make an <u>absolute</u> calculation of the π^- scattering there are no arbitrary parameters; once you've given the reduced width and the isospin, you have to make an absolute calculation of the scattering from the resonance formula. This is the curve that one obtains if one assigns a 60 MeV reduced width, the maximum is of course $\frac{2}{3}\, 8\, \pi \lambda\!\!\!^{-2}$. You see that this is not completely inconsistent with this data, <u>assuming</u> that this peak, which was originally interpreted as a single peak (in this scale it comes at about 1000 MeV) consists of <u>two</u> peaks which were not resolved before and that in photoproduction we see the first peak, and the energy is not quite high enough for us to see the second peak.

So we're forced to make a reinterpretation of that data saying that there are indeed two peaks, each of isospin $\frac{1}{2}$, because each of them are reached by the π^- scattering and not by the π^+. So both the cross-section ratio and the scattering curves show that the isospin is $\frac{1}{2}$. If we assign an angular momentum of $\frac{3}{2}$ that seems to be consistent with the data, while an angular moment of $\frac{1}{2}$ seems to be quite inconsistent with the data, not giving us enough cross section.

This is part of the reason of making an assignment of $\frac{3}{2}$ for J, although one might quarrel with that. Thus I think it's fairly definite that the isospin is $\frac{1}{2}$, what one can argue about is the angular momentum: i.e. what is the evidence that is a $\frac{3}{2}$ state? Well, partly this total cross section here; the other evidence is the angular distribution seen for this curve. Now, if this process goes through this state, you would not expect much direct electric production of the π^0 and if the angular momentum were $\frac{3}{2}$, you'd expect an angular distribution of $2 + 3 \sin^2 \theta$, and I'd like to remember that the data I showed you were, I think, quite consistent with that distribution, although I don't think that any of us could argue that the data can <u>prove</u> this distribution. Still it was quite consistent with the $2 + 3 \sin^2 \theta$ distribution.

This then is the second reason for assuming this angular momentum.

But now there 's some trouble with the π^+: we'll see this particularly with the Caltech data: (see fig. number 2 at the end), i.e. the π^+ distributions are strongly peaked forward throughout the photon energy interval 500 ÷ 850 MeV. We will have to explain this.

To give some idea to see how big the direct production might be consider this slide:

(see figure number 16 at the end).

You see the total cross section for the excitation of both π^0 and π^+ of the first $\frac{3}{2}$, $\frac{3}{2}$ state and it is just hit by the usual one - level resonance formula (M. Gell - Mann and K. Watson; Annual Review Nuclear Science Vol. 4, p. 238). We see that the effect of this is consequential even at the highest energies. You then have the S - wave production, as computed by the usual formula one uses for direct production that has been investigated at Illinois. If you extrapolate to high energies not knowing much about how the recoils come in, that would give us something of the kind of the curve labelled S, which as you see is rather large. You then have the total excitation of the new state: as you see it 's a very strong state because you should take this curve and imagine a curve which decreases as $1/\lambda^2$. So this is a very large peak, even compared to the $\frac{3}{2}$, $\frac{3}{2}$ peak which was always considered a very large peak any way.

In this case there 's a difficulty then due to the size of the peak and to the lack of symmetry in the forward - to-back angular distribution. There are perhaps two explanations for that: one of these is the one the people at Caltech have devised: i.e. that this is a $\frac{1}{2}$ angular momentum state, in which case you could have it decay into a P wave. I believe they have make some calculation of that at Caltech, and under special conditions they have shown that you can explain why the distribution stays forward. Perhaps Prof. Tollestrup will talk to us about this.

Another possibility is that this is a negative parity state: if this is so, then the interference with direct production will be quite different, it will thus decay in a D wave instead of a P wave. The D wave would have the same angular distribution (viz. $3+2 \sin^2 \theta$); so that will be consistent with our π^o production. On the other hand, the interference terms will be different so that the direct production will be less symmetric than in the $\frac{3}{2}$ case.

In any case it might happen thus a $\frac{3}{2}$, negative parity decays into a D wave. That would be much more consistent with what we see.

That's about all I'd like to say on this subject; I think that this question is stile quite open. Let me say one more remarks, if I can: one of the reasons for assuming that the state is a negative parity D state is the following:
This came about because of magnetic dipole absorption.

If this is a negative parity state, we can get excited by an electric dipole absorption, and that can explain why we get a stronger excitation of this level than we had of the previous level. Let me give you a model: we could assume that this state is a nucleon with just a pion in a P state; that would account for the angular momentum, the isospin. One thing we could do is to add another pion in an S state: with a pion - pion interaction you might have another meson in an S state, and then you'd have negative parity.

Now I want to go one step farther and say that this state does exist, and take a look on the possible implications: while in the case of the first state we just had two modes od decay. That was easy, we just assigned the Clebsch-Gordan coefficients, and got the 2 : 1 ratio.

This is no longer so simple, because now we can write down the following modes of decay, say:

$$\gamma + p \longrightarrow p^{**} ,$$

while

$$
\begin{aligned}
p^{**} \longrightarrow\ & \pi^+ + n \\
& \pi^o + p \\
& \pi^+ + p^* \\
& \pi^o + p^* \\
& \pi^- + p^* \\
& \pi^+ + \pi^- + p \\
& \pi^o + \pi^+ + n \\
& \pi^o + \pi^o + p.
\end{aligned}
$$

Finally, if we have enough energy on the high side of this state, we can get $p^{**} \longrightarrow K^+ + \Lambda_0$; here p^* indicates the $\frac{3}{2}, \frac{3}{2}$ isobaric state of the proton.

You can work out the statistical weight in this isospin space on the basic of Clebsch-Gordan coefficients for all of these states: I did that for the first two, but I was not smart enough to do them all; Dr. Salpeter had to do that for me. You can put in numbers that are proportional to the Clebsch Gordan coefficients: for the $\frac{3}{2}, \frac{3}{2}$ you had 2 : 1; now it 's a bit more complicated, all of these states are possible, so that we have to calculate the statistical factors for all of these. Furthemore, we can now take a Fermi picture of this, and say: 'Well, we have a state: how will it decay?' It'll decay proportionately to the Clebsch - Gordan coefficients, to the momentum space: thus make a Fermi computation, i.e. calculate what's going to happen just from phase space arguments.

On this basis, I fitted the experimental data with these phase-space calculations.

Phase-space of course changes violently with the energy as you cross the various thresholds concerned: anyway assuming these figures you have a fairly complete prediction of what you would see.

Touschek: If I may make a semi-theoretical remarks, I'd say this: I think that a very remarkable situation has come up. Because if one tried to make a phase analysis of the data for electron ionization of Hydrogen atom, in the corresponding energy region in atomic physics, we would all say that this could not be possibly right and have nothing to do with the facts, whereas here we use exactly the same technique to analyze the data. On the other hand there is a mitigating factor that seems to make this situation not so completely hopeless, and that is the fact that the angular distributions that come out from most of the measurements seem to be simple.

I mean there are not these violent kinks and ups and downs which one encounters in atomic physics. Now, for the experimental part of my question: how much is this due to the fact that we only have a few points? I mean, are measurement really good enough to tell us that there isn't some sort of very queer phase? Because if this is not so, we clearly don't have any-rule of the game: i.e. we can certainly find among the theoretical curves one that hits a certain number of experimental data.

W.: In the first place, I'd say an experimentalist is inclined to write down the simplest of the theoretically possible curves: that's the only thing an experimentalist can write down, to see deviations from.

I think the Caltech results have looked at more details both for the π^+ and the π^0, none of which (except maybe at the higest energy) are inconsistent with a $2 + 3 \sin^2 \theta$ angular distribution. I mean, this sort of thing is just one year old, and we do not have the full story, by any means.

Touschek: Maybe I wasn't clear. What I meant was this: I think it would be best to have one single curve at one single energy, but very densly populated by experimental points with small error: we could thus realize immediately if this picture of simplicity is as general as it appears to be.

W.: If we do, you probably would start talking about retardation effects, or some other complicated thing, even on a simplified theory.

Corson: This is a very difficult thing for an experimentalist, because the region where you can measure easily is just in the vicinity of 90°, you can measure say 150° to 75°, and then you have statistical uncertainties on the points. Where you'd really like to measure is at 180° and 0°.

Bernardini: In this sense, at Geneva we heard the preliminary research by Panofsky, but I don't think it's quite comprehensible yet. His work concernes the π^+ up to about 600 MeV in the forward direction.

The question I want to ask as a preparation for this afternoon is the question of parity. If we consider the π^0 experiments, that show the simpler $2 + 3 \sin^2 \theta$, then this is the final state. (Let's consider the π^0 because the π^+ is a bit more complicated). This has a definite parity, and is a P wave.

W.: Or a D wave.

Bernardini: I looks very much like a P wave. Because you pointed out with cross-section arguments that this was a $\frac{3}{2}$ state. If we stick to this interpretation, then it's parity is even, there es no doubt about it. The final state is a D state: you have a pion and a nucleon in the final state, thus even parity: there can be no question about it.

W.: You've cought me in my own trap. It's true I did use that as a reason for assigning the P wave. It would here been complicated had you gone on to ask what would I have gotten in the case of the D wave.

I'm not sure that that would be inconsistent with a D wave. Because a D wave, with a sort of a magnetic moment of $\frac{3}{2}$, might not be inconsistent with that.

I don't know, I haven't looked into it. There might be enough cross section, it's quite complicated.

B.: That was just a matter of clarification: if we agree, that this is a state of disparity then it's practically a prohibition of this state $\alpha/_{13}$. Then we should expect, e.g. in the scattering experiments, a very significantbehaviour of this state.

W.: Right

B.: Another question: do you consider the interaction to be attractive or repulsive?

W.: I would be have to be attractive.

B.: In that case the measurements by Piccioni et al. should be corrected, because it might be a very strong interference effect. The Piccioni's measurement was a measurement of total cross section. They took special care to distinguish among angles differing by a few degrees, thus it was actually also an angular distribution measurement, that was strongly influenced by a negative interference between these states: i.e. the agreement with the Piccioni numbers should be corrected. It might not be as good as it looks, it might be even better, I don't know.

W.: Since you're written down α_{13}, let me say this: If you take the best assignments of α_{13} for low energies up to the peak, then you can say that there is a resonance at 600 MeV, and from the value of α_{13} you can determine what the reduced width is. I did that and that also gives 60 MeV, which is consistent with what you get if you just make an eye - fit with the curve. But I don't think it proves very much.

B.: Furthemore, I missed your point when you plotted all these curves for the several cross sections. I don't know if you considered the direct interaction terms, i.e. the photon finds the proton in a state in which the latter is essentially a neutron with a pion cloud and acts directly on it. This gives the retardation term, and has an increasing importance as the photon energy decreases, involving higher and higher momenta. This is so true that in spite of the fact that the angular momentum becomes more peaked in the forward direction, and in spite of the fact that this section of the solid angle is fairly small, it contributes appreciably to the total cross section. Did you consider this term a part from the S wave?

W.: No. This might help to explain the angular distribution though.

B.: Let's say that pion momentum rises with the photon momentum: you are thus entering the nucleon more and more: it might be that the perturbation method used for calculating this in formula is not valid anymore. In other words, it may be that the pion production is not representable in this way anymore.

Tollestrup: I have the impression that there 's a bit of confusion in terms. This direct photoelectric effect is not the total S - wave production.

B.: No, it isn't. It's the Kroll-Ruderman S - waves; they are due to electric dipoles, while the others account for all sort of dipoles. The only problem is to define the pion current problem; you can then account for it by conventional perturbation theory.

W.: This is probably an important factor, I haven't considered.

B.: There's an important theoretical point I hope will be discussed later: i.e. how much and how do we conceive the pion-nucleon state in the physical nucleon. That is, considering the nucleon as a superposition of many states, how much does the pion + nucleon state contribute to the physical nucleon state? But as far as you apply the perturbation calculation, it's there, has to be there, and remains there.

W.: Perhaps the predominance of one state is not a proper picture. But if it is a proper picture it would bring about a great deal of simplification in meson physics because you can stick a donain around 300 MeV, and at 600 MeV. In each of these youhave just one state that is predominat: you could go on and finish the story and say there's a state of perhaps $\frac{1}{2}$, $\frac{5}{2}$ predominant at 800 MeV, and another maybe $\frac{3}{2}$, $\frac{5}{2}$ that's predominat at 1000 MeV. There are nearly equally spaced energy levels, each one predominant over a given energy region. If there 's anything at all in this kind of picture it would be a great simplification in this field of physics.

Touschek: I would like to ask if this sort of picture is realistic enough. As long as one doesn't have a particular rule for the choice of these states, one can always work out a pretty picture. You see, the rules of the game aren't stringent enough. I mean if you go high enough and work hard enough you can find a variety of states, but I don't know if the physical picture thus presented is an accurate one.

W.: It seems to me that for each energy region you'd assign special isospin states, special angular momenta and special parities; had the theorists predicted these things ahead of time we could here tested these theories: just as in the sense that the strong-coupling theory predicted that there would be a $\frac{3}{2}$, $\frac{3}{2}$ resonance at 300 MeV. You could imagine that the theory had predicted the existence of two other resonances that would dominate the experiments in a given energy-region.

I think that what comes out of this can be calculated from the point of view of theory. I mean you could immagine completely different angular momenta and iso-spins any get completely different experimental results.

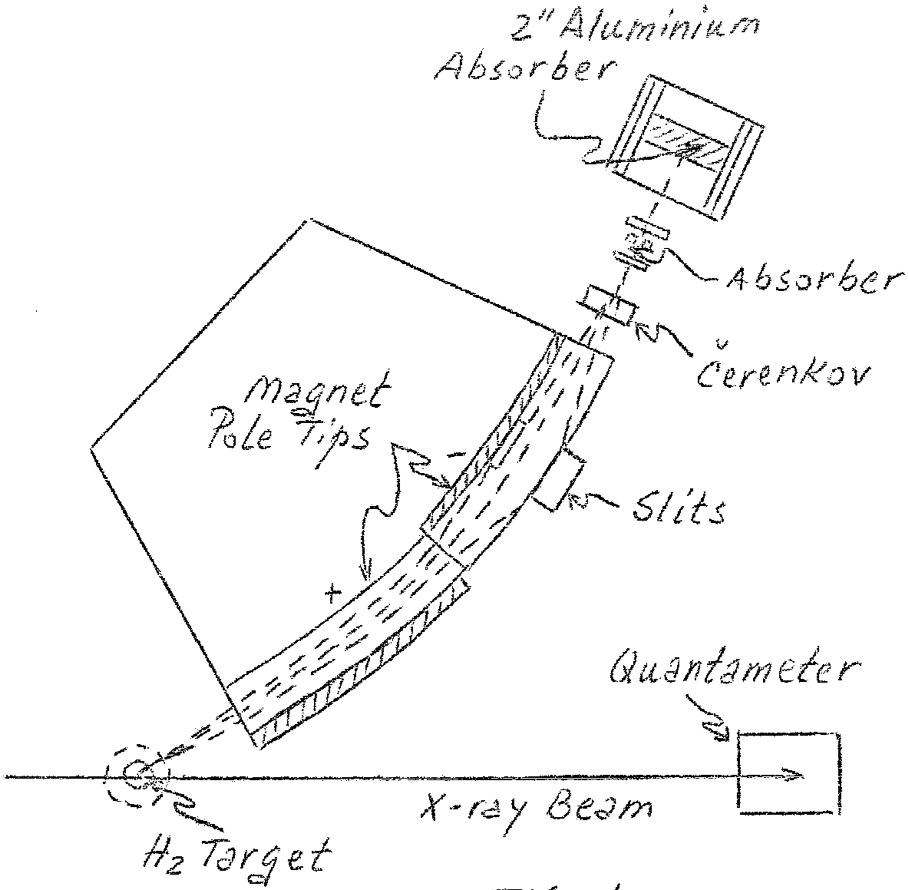

2" Aluminium Absorber
Absorber
Čerenkov
Magnet Pole Tips
Slits
Quantameter
X-ray Beam
H_2 Target

FIG. 1

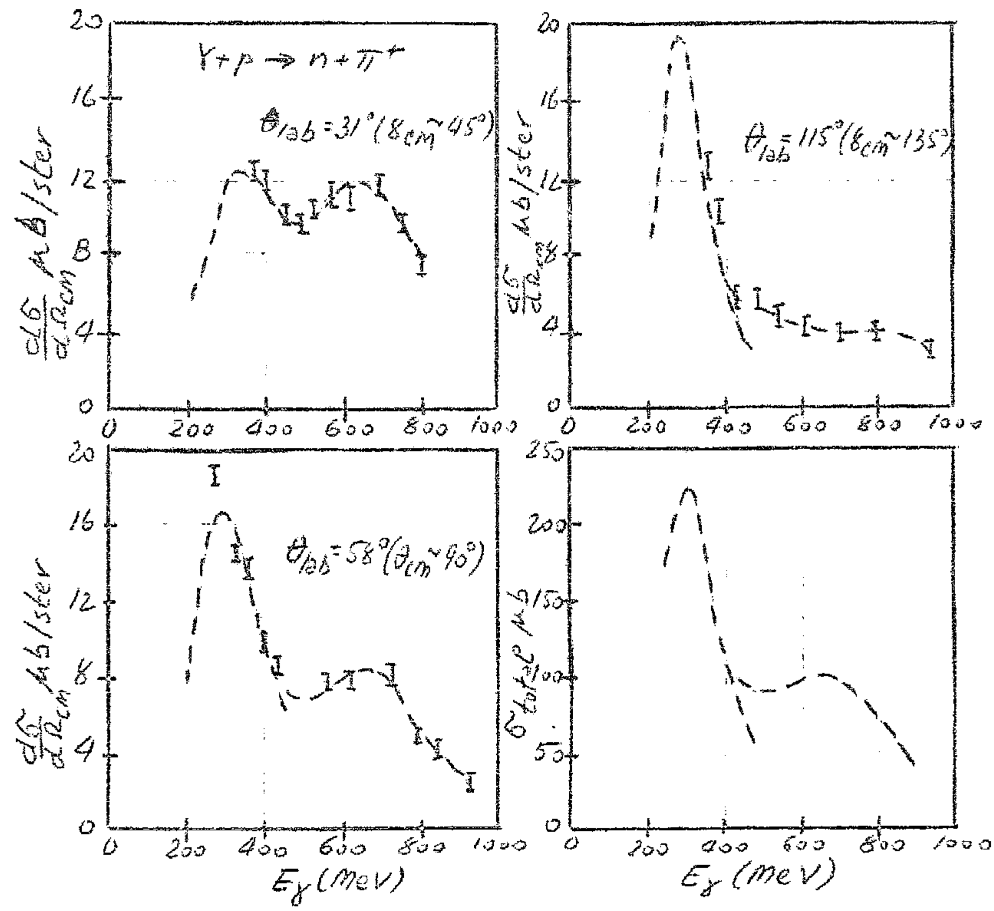

$\gamma + p \rightarrow n + \pi^+$
$\theta_{lab} = 31°$ ($\theta_{cm} \sim 45°$)
$\theta_{lab} = 115°$ ($\theta_{cm} \sim 135°$)
$\theta_{lab} = 58°$ ($\theta_{cm} \sim 90°$)
$\frac{d\sigma}{d\Omega_{cm}}$ μb/ster
σ_{total} μb
E_γ (mev)

FIG. 2

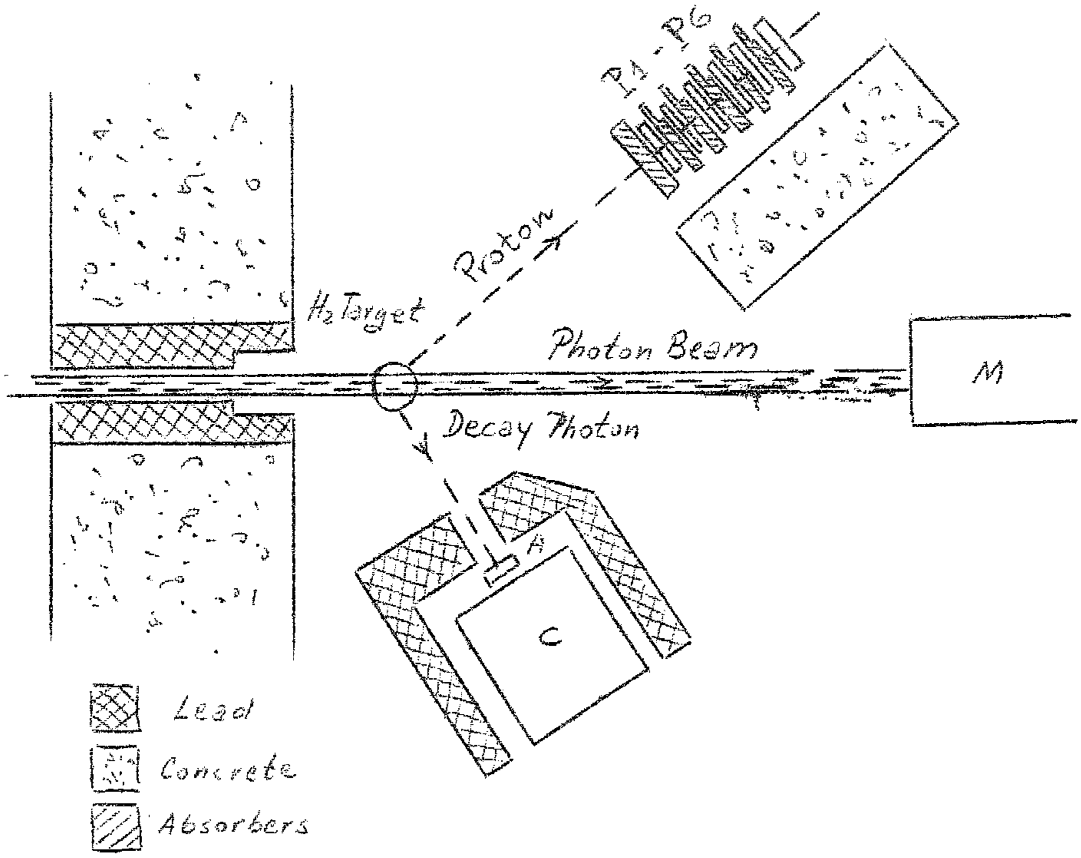

P1 - P6
Proton
H_2 Target
Photon Beam
M
Decay Photon
A
C
Lead
Concrete
Absorbers

FIG. 3

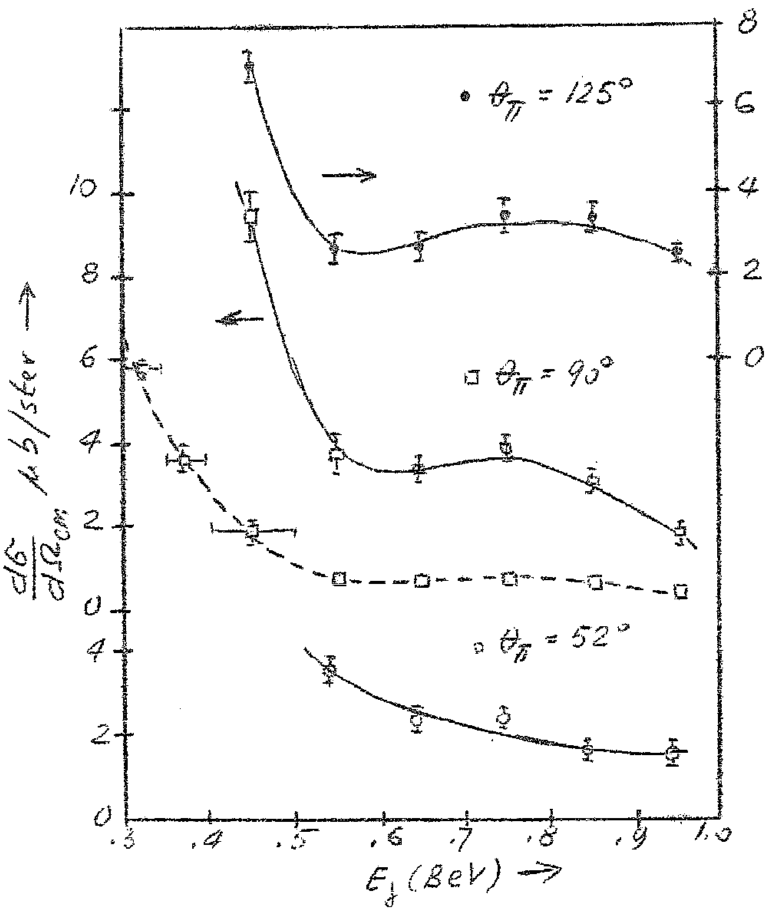

$\theta_\pi = 125°$
$\theta_\pi = 90°$
$\theta_\pi = 52°$
$\frac{d\sigma}{d\Omega_{cm}}$ μb/ster →
E_γ (BeV) →

FIG. 4

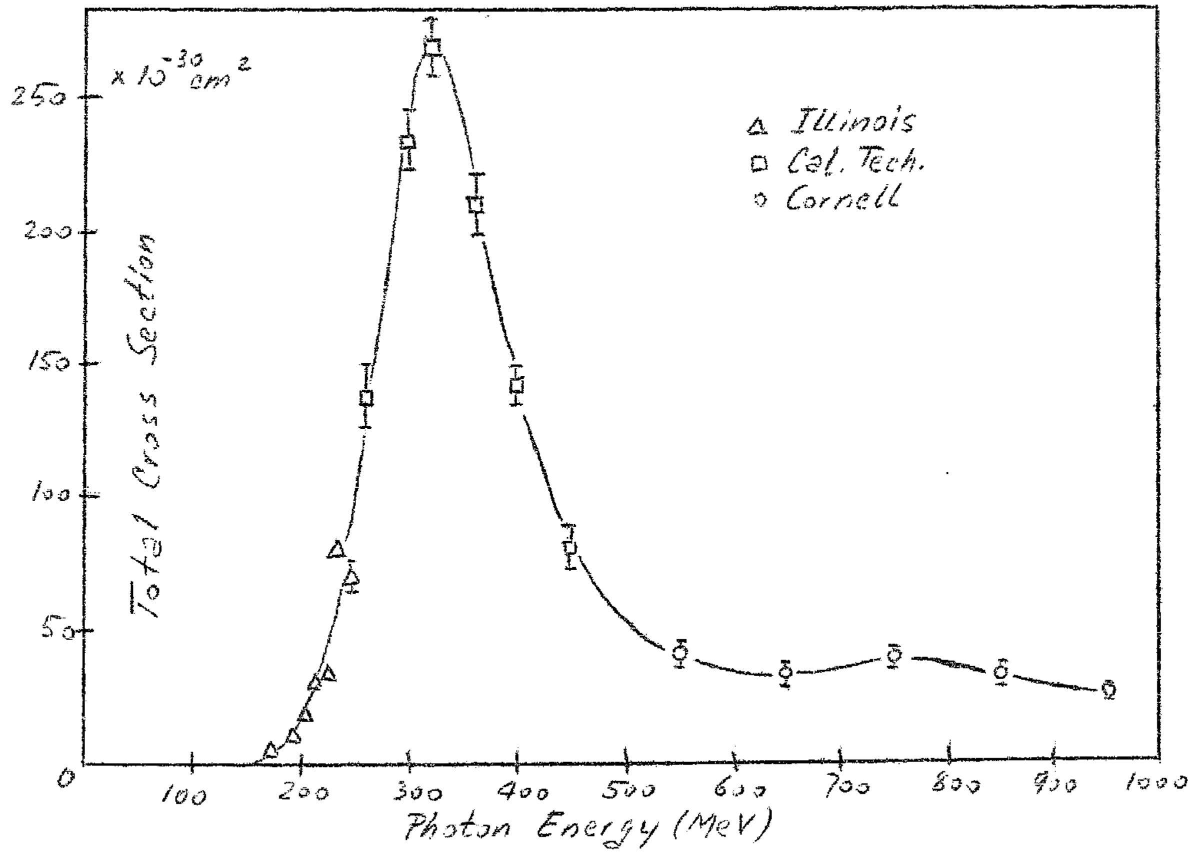

x 10^-30 cm^2
Total Cross Section
Photon Energy (MeV)
Illinois
Cal. Tech.
Cornell

FIG. 5

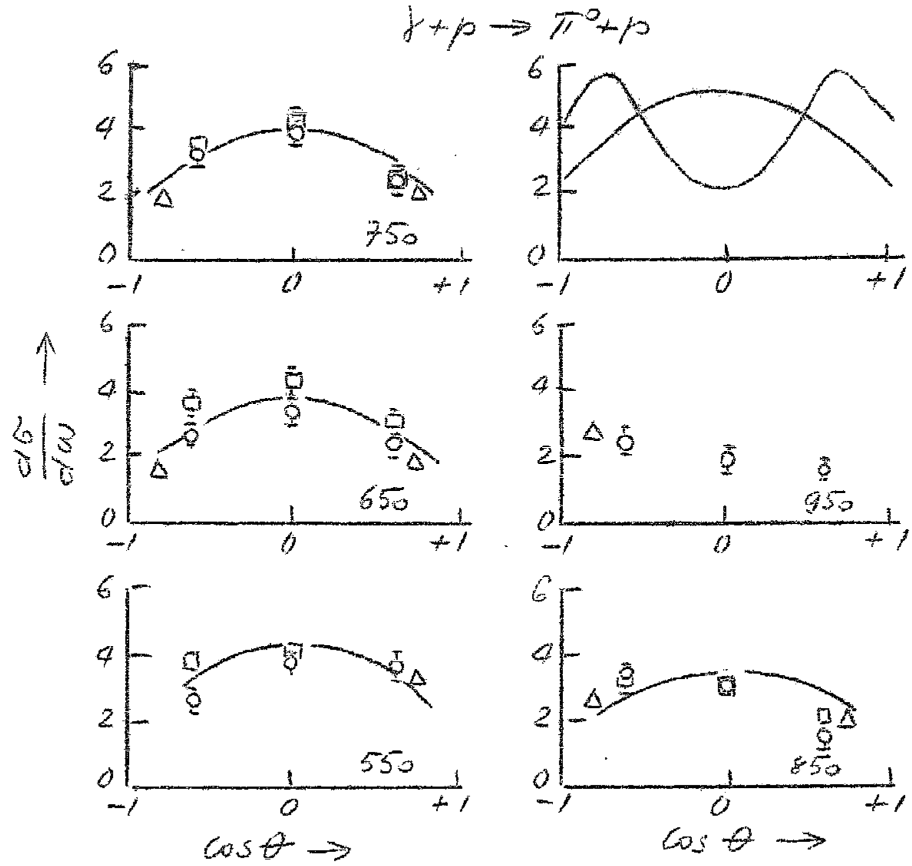

γ + p → π° + p
dσ/dω
750
650
550
950
850
cos θ →

FIG. 6

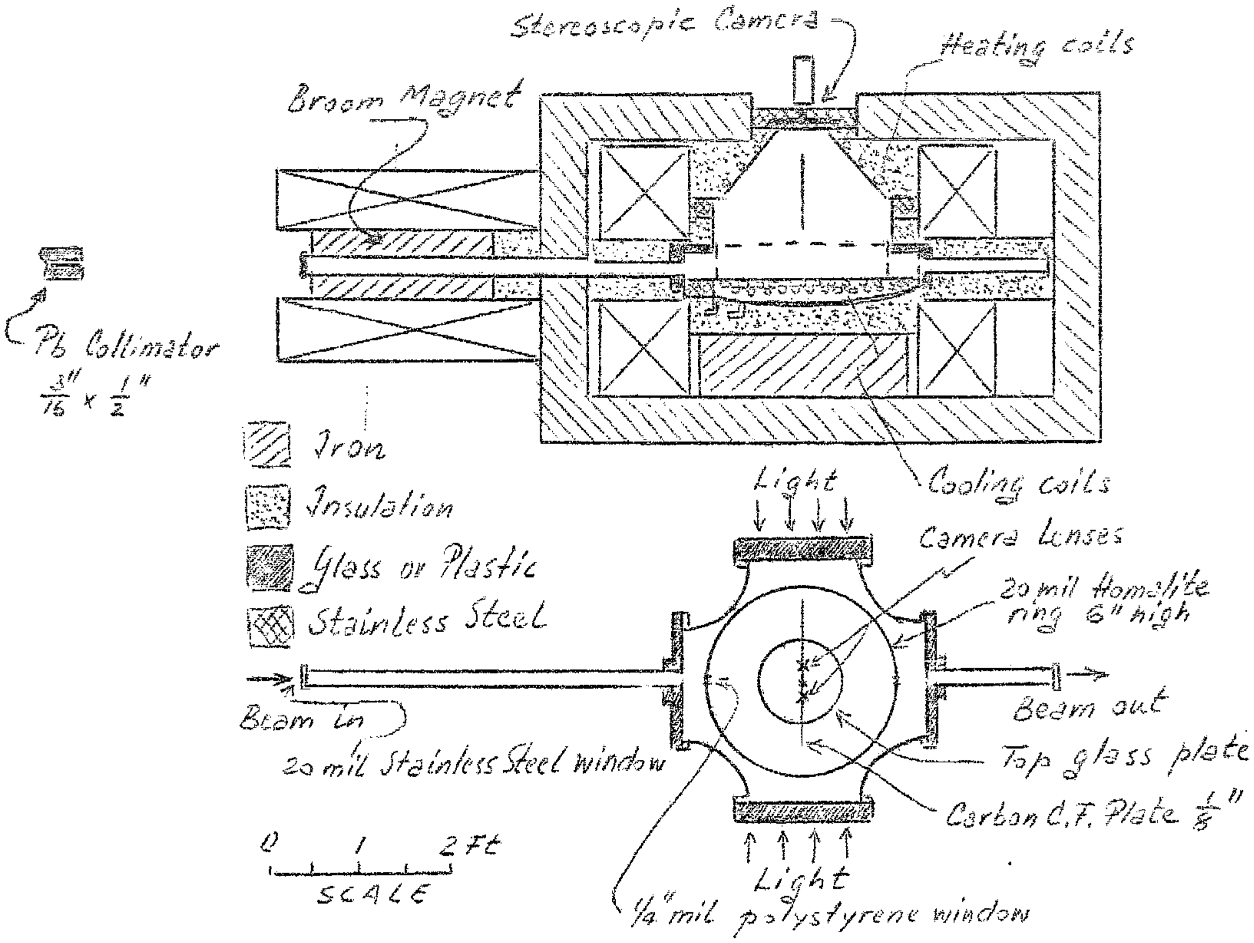

Stereoscopic Camera
Heating coils
Broom Magnet
Pb Collimator
$\frac{3}{16}'' \times \frac{1}{2}''$
Iron
Insulation
Glass or Plastic
Stainless Steel
Cooling coils
Light
Camera lenses
20 mil Homalite ring 6" high
Beam in
Beam out
20 mil Stainless Steel window
Top glass plate
Carbon C.F. Plate $\frac{1}{8}''$
0 1 2 Ft
SCALE
Light
1/4" mil polystyrene window

FIG. 7

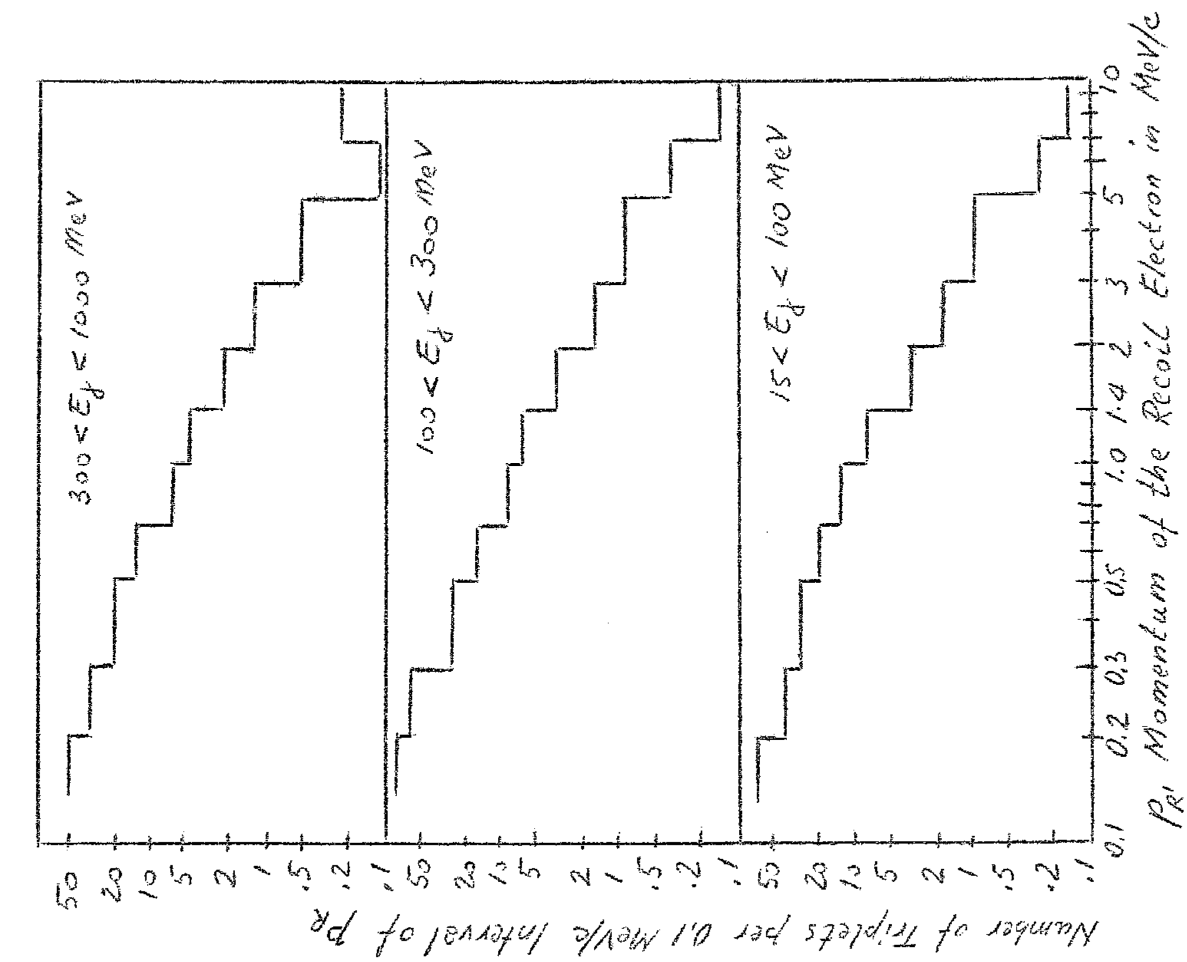

$300 < E_\gamma < 1000$ MeV
$100 < E_\gamma < 300$ MeV
$15 < E_\gamma < 100$ MeV
0.1 0.2 0.3 0.5 1.0 1.4 2 3 5 10
p_R: Momentum of the Recoil Electron in MeV/c
Number of Triplets per 0.1 MeV/c Interval of p_R

FIG. 8

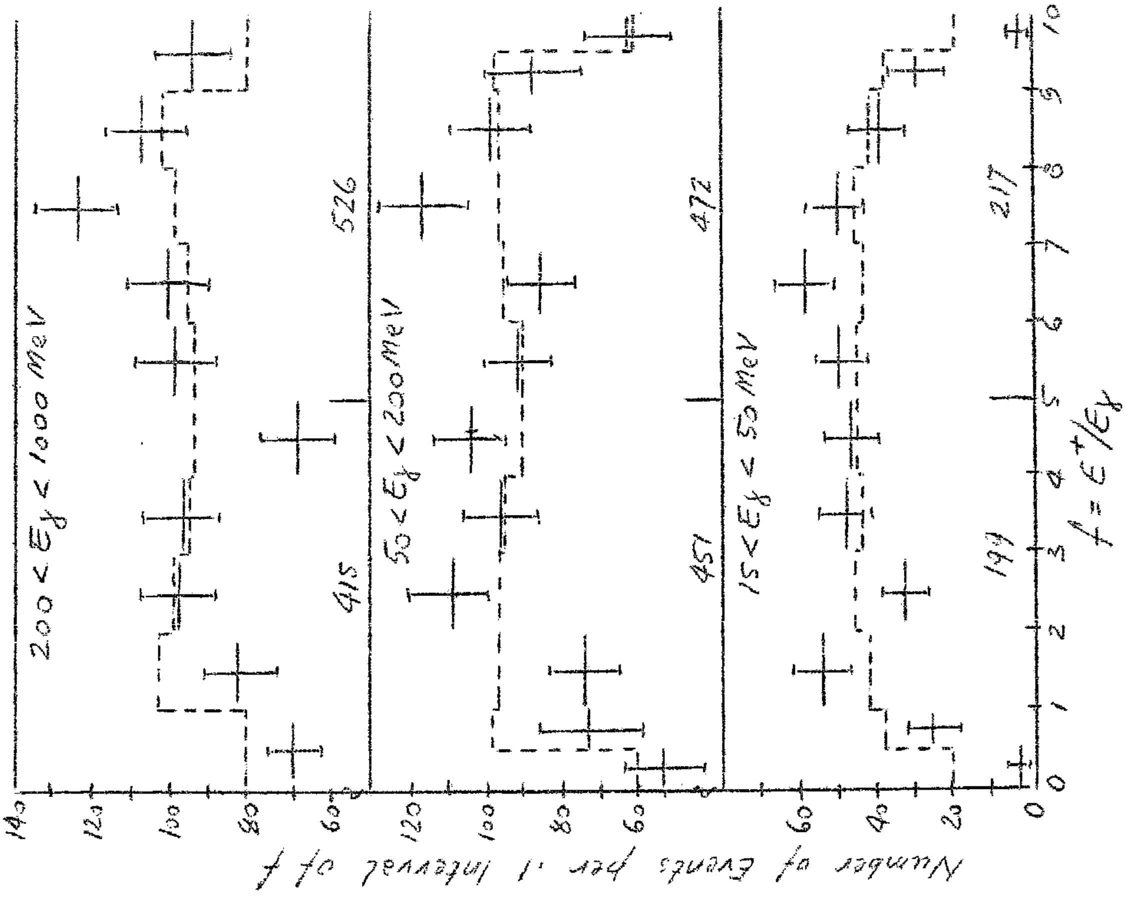

200 < Eγ < 1000 MeV
50 < Eγ < 200 MeV
15 < Eγ < 50 MeV
f = E+/Eγ
Number of Events per .1 Interval of f

FIG. 9

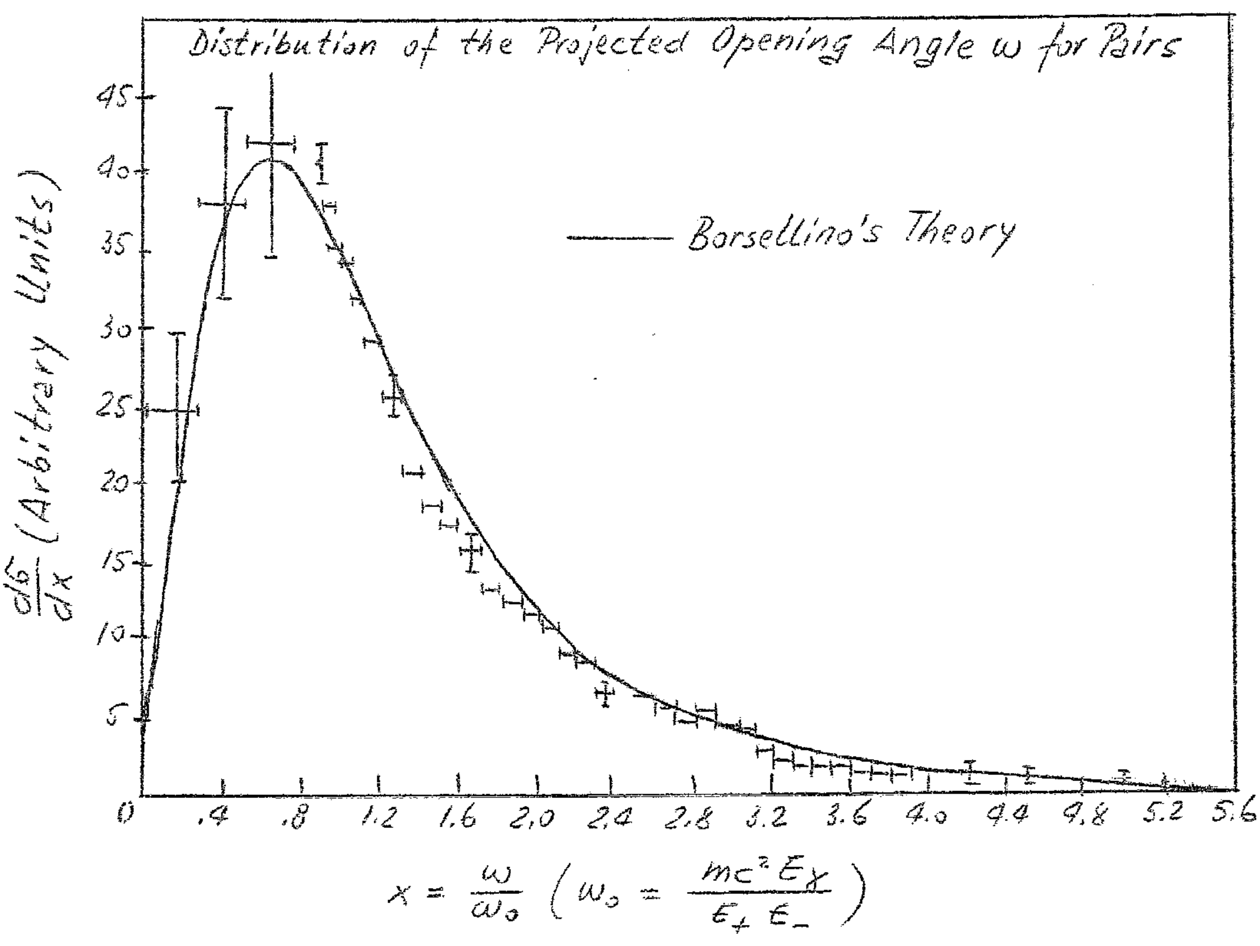

Distribution of the Projected Opening Angle ω for Pairs
Borsellino's Theory
dσ/dx (Arbitrary Units)
x = ω/ω₀ (ω₀ = mc² Eγ / E+ E−)

FIG. 10

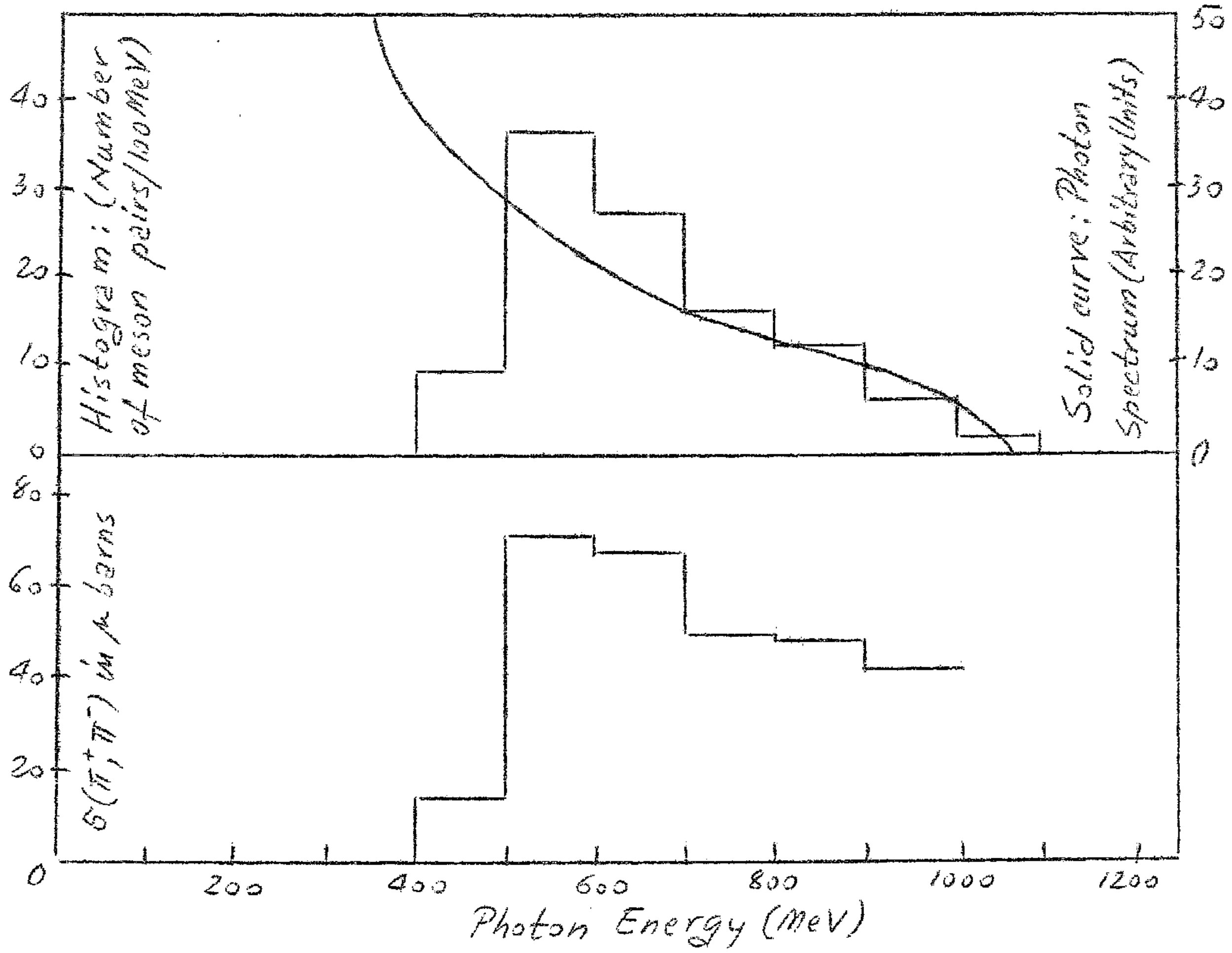

Histogram: (Number of meson pairs/100 MeV)
Solid curve: Photon Spectrum (Arbitrary Units)
$\sigma(\pi^+,\pi^-)$ in μ barns
Photon Energy (MeV)

FIG. 11

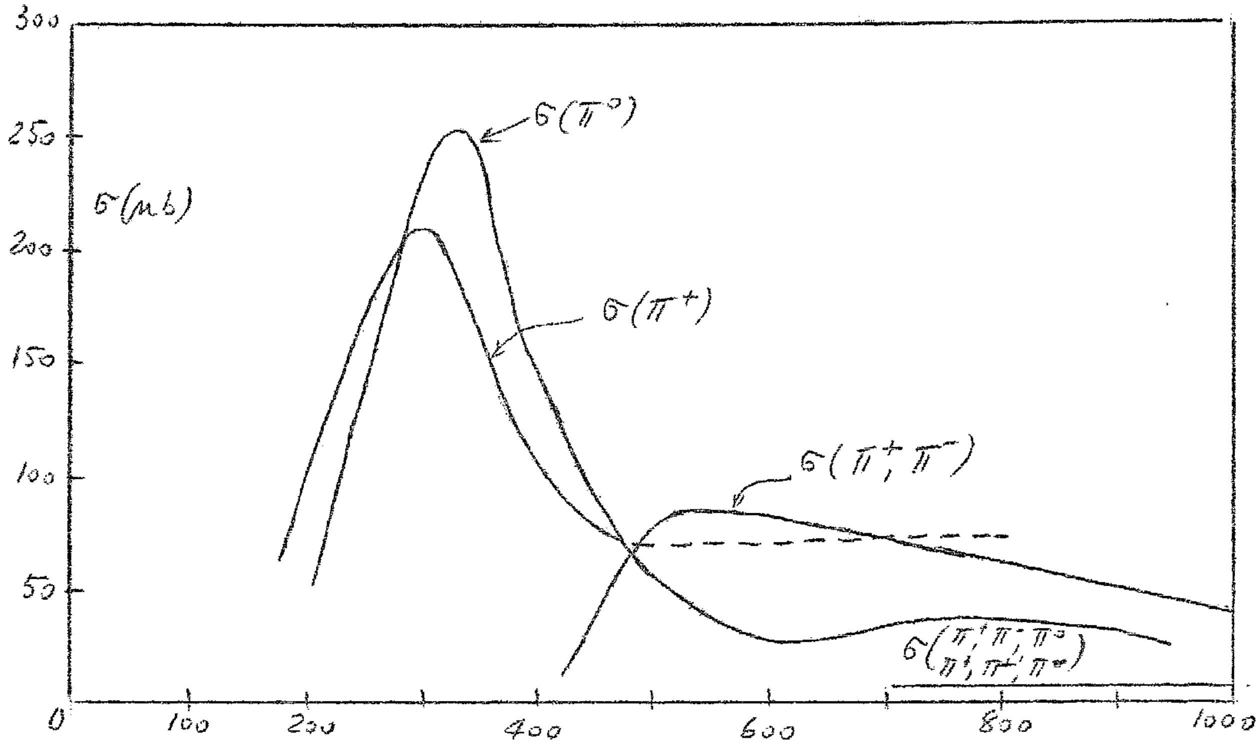

$\sigma(\mu b)$
$\sigma(\pi^o)$
$\sigma(\pi^+)$
$\sigma(\pi^+,\pi^-)$

FIG. 12

CM Angle of Emission (500 < hν < 700 Mev)

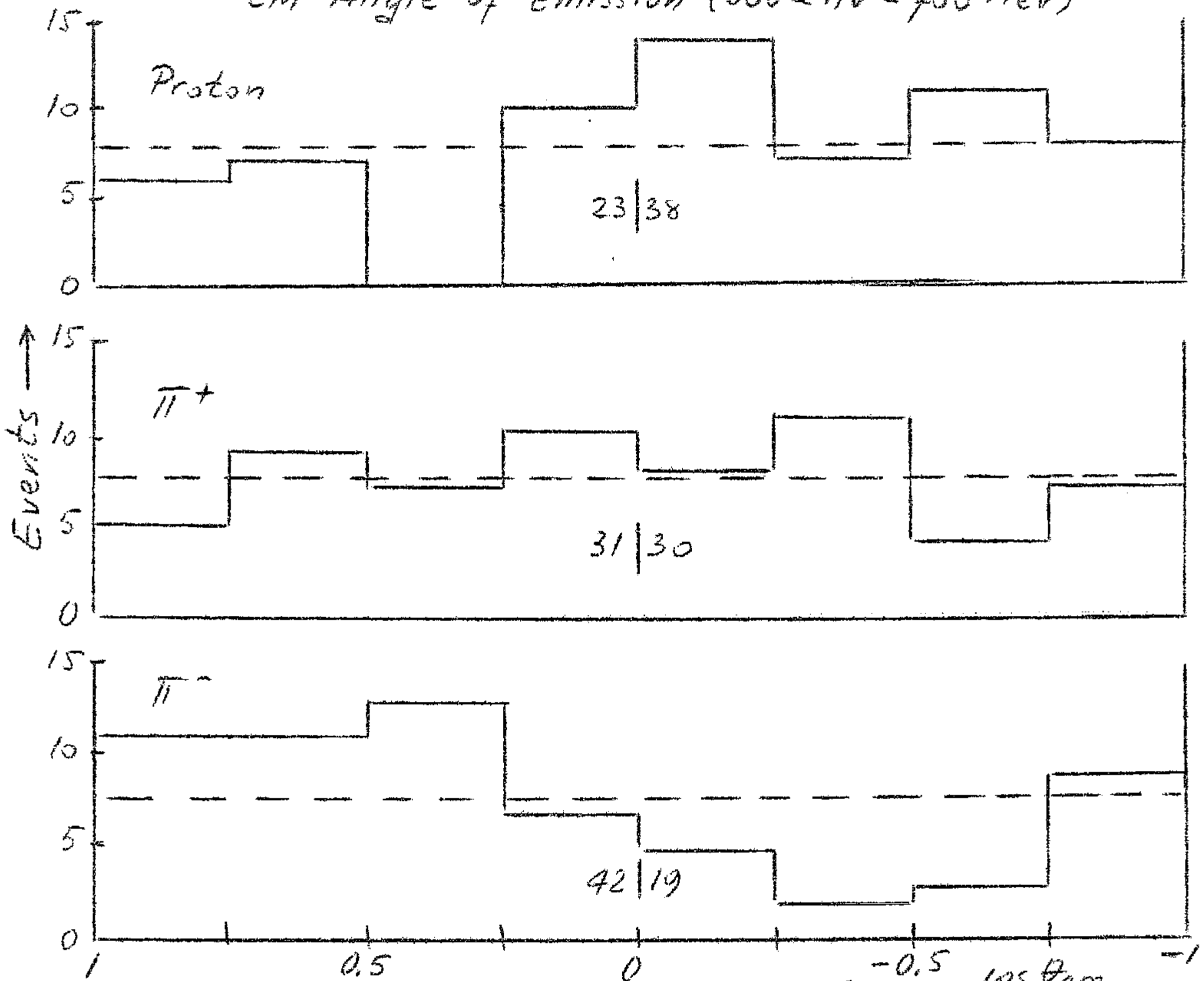


FIG. 13

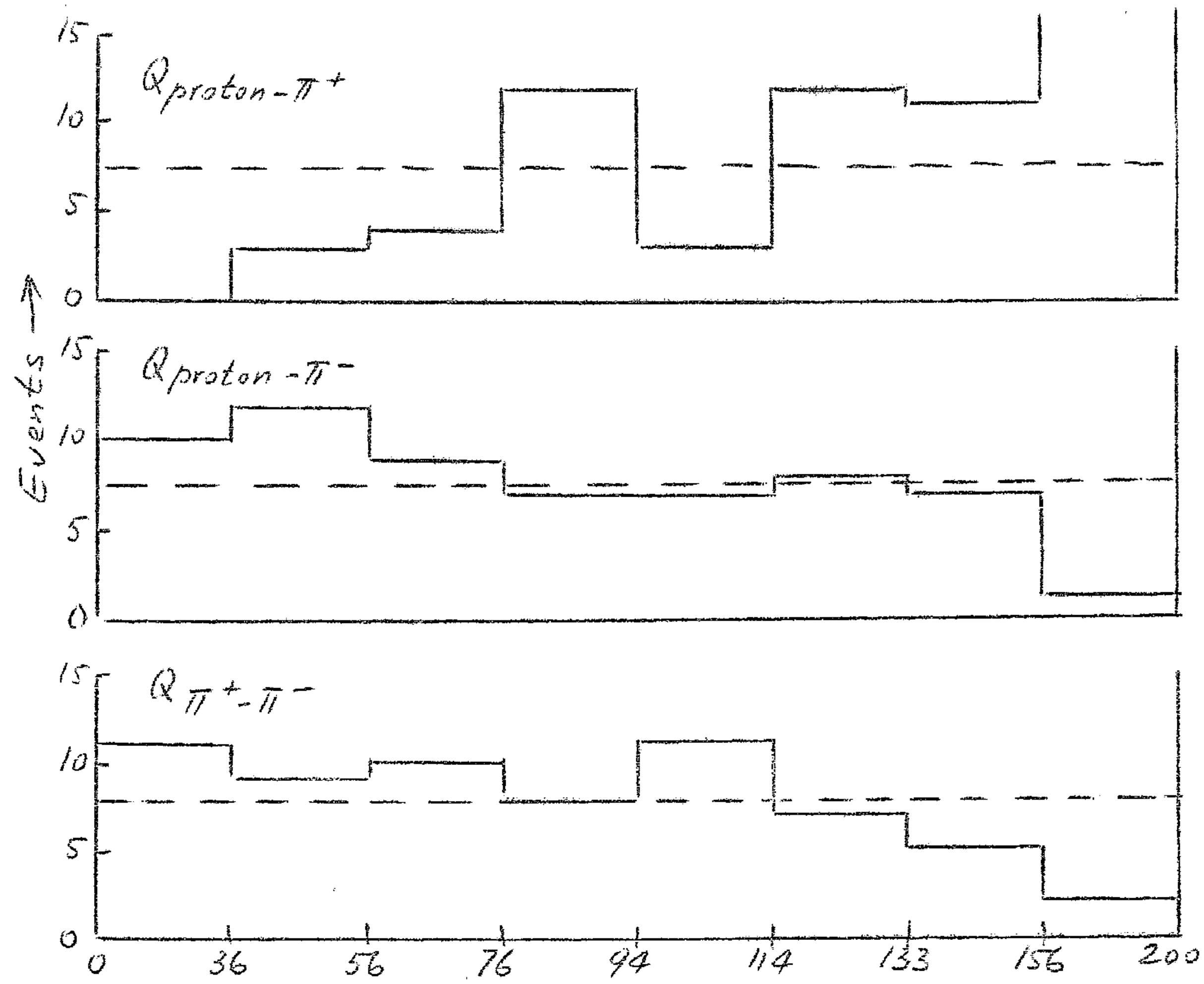
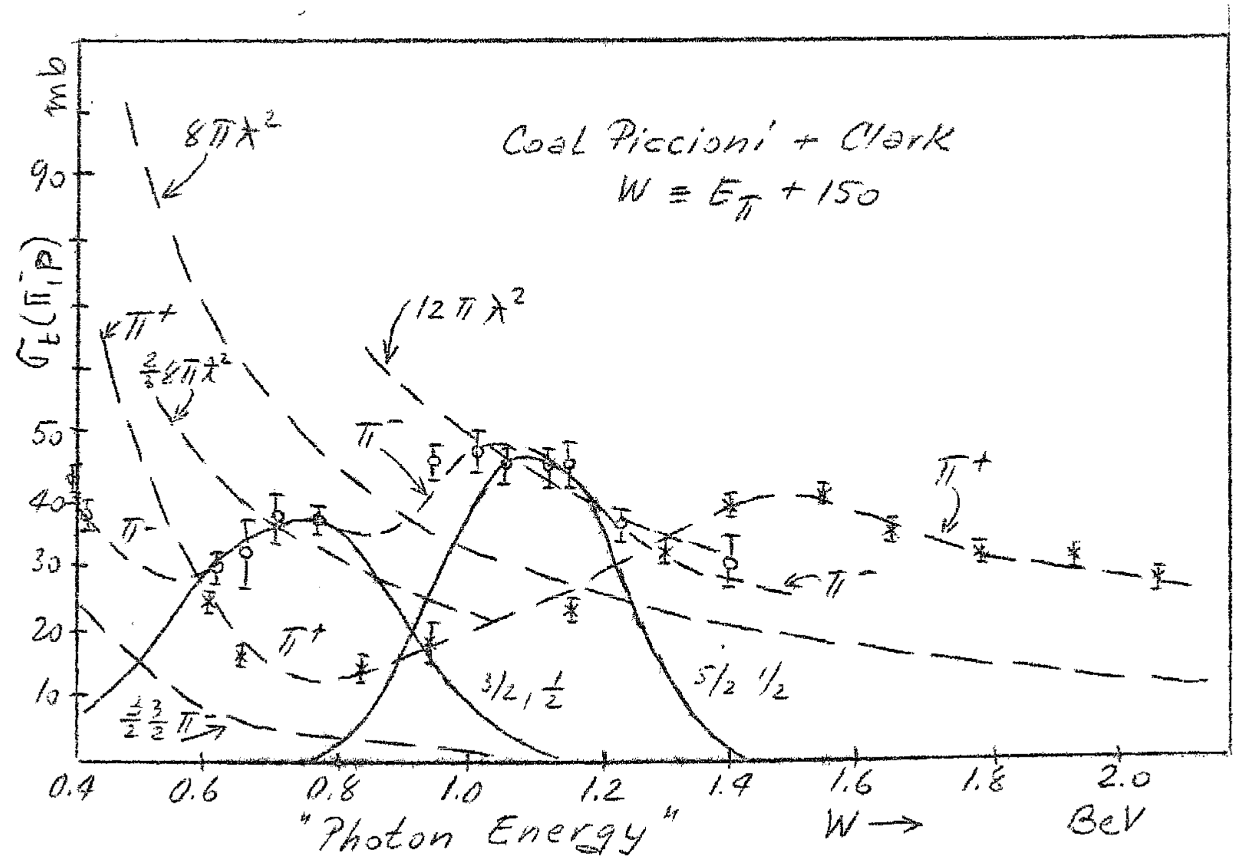
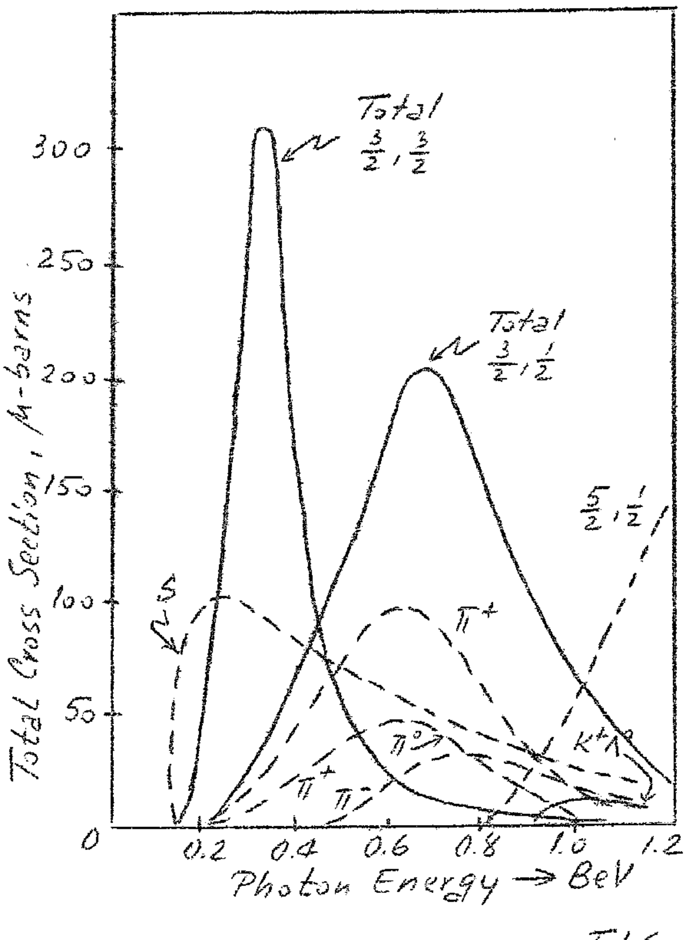


FIG. 14



Coal Piccioni + Clark
W ≡ E_π + 150
mb
$\sigma_t(\pi, p)$
90
50
40
30
20
10
8πƛ²
12πƛ²
π⁺
π⁻
3/2, 1/2
5/2 1/2
0.4
0.6
0.8
1.0
1.2
1.4
1.6
1.8
2.0
"Photon Energy"
W →
BeV

FIG. 15



Total 3/2, 3/2
Total 3/2, 1/2
5/2, 1/2
S
π⁺
π⁰
π⁺ π⁻
K⁺Λ⁰
300
250
200
150
100
50
0
0.2
0.4
0.6
0.8
1.0
1.2
Total Cross Section, μ-barns
Photon Energy → BeV

FIG. 16

Pomeriggio del giorno 9.7.1958

ESPOSIZIONE DI TOLLESTRUP

This slide (fig. 1) shows the experimental arrangement for all the work that I'm going to talk about today. There's a high pressure hydrogen target that sits here, the one that works at 2000 pounds, and liquid nitrogen temperature, and this is the target that all of the π^+ and π^o exitation work was done with in our so-called Phase 1. I should say that at present, when I was there, M. Bloch was using this target, and a small magnet to measure the π - pair production cross-section.The beam goes through this target,it goes through a second liquid hydrogen target that has two large magnets set up to pivot about it as a centre. This magnet here has a vacuum chamber for measuring the energy of recoil protons, for istance, for π^o production, and will go up to something of the order of $\frac{600 MeV}{c}$ in momentum, I believe. This large magnet here has two positions. The particles get bent out of the plane of the beam, and our beam is about 7' high off the floor, so that the trajectories come out of the plane of the beam and are focused in counters down. Then we have the magnet that Bloch was using, that bends the particles in the plane of the beam. It's been somewhat useful in our case to bend particles out of the plane of the beam for two reasons: one, there's less background to get off the beam plane, and secondly, for istance, as you noticed in the experiment that Corson was talking about this morning, the π^o production dynamics change very rapidly with angle.

If you want a large solid angle, the nicest way to get that large solid angle is with an extent perpendicular to the plane of the drawing here. This is just a large uniform field here. Now, we have in addition, a small table here that counter telescopes can sit on, and the way the machine has normally been run is to try and have at least three experiments going at the same time. Now generally, two of these experiments are running in earnest and trying to get better, and the third experiment is just sitting there waiting to get in, so that they can make a change and adjust this experimental apparatus, so that when he gets on the machine, in a little while he'll be able to have all this equipment adjusted for a run. There's not as much interference as one would think, and I think it's a very satisfactory way to run a synchrotron, particularly if there's some planning goes into it, so you try to select experiments that want the same Bremsstrahlung, and let them have the same type of burning characteristics. Finally, there's the beam under here, the beam-catcher away back here, and the electronic apparatus goes along here, and all the cables for the electronics.

Now we had two experiments done on the π^o production. We have one that's been done with this arrangement of magnets. The two magnets together cover the complete momentum spectrum from a very low momentum up to about $\frac{1200\ \mathrm{MeV}}{c}$ and that'sbeen done by Vette.[1] And a second one has been done by measuring the recoil proton energy by means of a counter-telescope, and that's been done by Worlock with some help from myself. Well, we've done almost all experiments in the laboratory by at least two different ways, I think, right from the start of the laboratory, and this has had the interesting consequence that, although the counting statistics in general on experiments are very small if you do them by these two completely different techniques, you soon realise that the troubles are not counting statistics in experiments, but sources of error that you don't understand. These two experiments will not agree with each other, but at least I think you can decide what kinds of limits of confidence one can have, by looking at these two experiments. The magnet experiment,[1] and the counter-experiment have both been done by measuring the recoil proton alone. Now this means that the experiment in some respects is not nearly as clean as the one we heard about from Cornell this morning, in the sense that multiple production constitutes a serious background, and we have to worry about it. This shows (fig. 2) you the dynamics for the process for three different γ-ray energies here. This is the recoil proton energy, plotted against the laboratory proton angle, so this is a kinematics diagram for the process in question. Now if one sits, for instance, at 30° in a laboratory system, and looks at 400 MeV or 450 MeV protons with some small rectangle here that's established by the counter: namely, these co-ordinates here are counter co-ordinates, there's a certain energy interval that one is sensitive to, and there's a certain angular interval that one is sensitive to, so your counter defines a small rectangle on this diagram. Either your counter or your magnet, or whatever system you have, appears as a rectangle on this form of system. So you can see, and the thing that discouraged us from adopting the technique that Wilson mentioned of keeping the Bremsstrahlung limit just 150 MeV above the part we were measuring so that we didn't have to worry about the π^0 production, was the fact that at the time we had not measured our Bremsstrahlung spectrum, so we didn't know the details of this end-point. Now, as you know, the end-point of the Bremsstrahlung spectrum is very sensitive to how thick your target is; whether you have a thick target spectrum, a thin target spectrum, or just exactl. what-have-you. So, as a consequence, although if one looks here, you would think, 'Allright, it would be very easy to operate in this small region with the counter so that the pairs which come in at from 1000 MeV, photons which come in from this limit

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1) - J.I. Vette; Phys. ReV. - 111, 622 (1958).

could not influence your one BEV measurement", it turns out that one has to know the details of the Bremsstrahlung spectrum very accurately in this region if you're going to do that. What we did do was always keep the machine empty, or as low as we could, but that is, - for instance on this diagram here we might well have operated with a rectangle like this. What we hoped was - and I hope it's justified by the later measurements that were made - is that phase space, for instance, that one has 1000 MeV photons, and that the two π - mesons go off parallel. They may make a recoil proton at this end, for instance, at 30° you can get 420 MeV protons if the two π's go off as a unit together. Now phasespace prohibits this, and so, if I plot the numbers of π - mesons in a third directioncoming out of here, I'd expect to have a thing that slopes up, a sort of hill this way, and if I get close to this line (but I'm not very far down in this region), phase-space should make it quite safe for me, even so. Now there's a region where one can operate - where we did operate - that was completely safe; namely, Vette, withthis low energy magnet set-up that I showed you, with the vacuum system, operated out in this region here, this very low energy region, not as low as Corson was talking about this morning, but still, photons in the order of 35 MeV - when found out in this region, and here there's no trouble with pairs at all, you're completely safe in this region. On the other hand, our target was much too thick for these measurements - our liquid hydrogen target was a three-inch diameter container of liquid hydrogen. It was not designed for this measurement, and the energy loss of the protons in the target itself made it rather difficult to measure the cross-section that we finally got out of here. So, although we believe the numbers that come out of this, there is the problem that the numbers you get are not directly related to the cross-section that you got for measuring, and it's necessary to unfold the target thickness from the result that you get. The two experiments were more or less divided into two sections. The magnet experiment was originally going to measure the energies 800,900 and 1000, and the telescope was going to measure 600,700, and 800 MeV. this took care of two graduate students that had been waiting around for more than five years to get a Ph.D. In actual fact, these things overlapped somewhat more than was intended. Originally, we thought they would overlap only at 800, but it turns out that there's some magnet data at the lower energies and there's some telescope data at the higher energies.

Well, the counter - telescope experiment was set up with a 4-counter telescope. (fig. 3) Where this is veto, this is the Δ R counter, and there's two $\frac{dE}{dX}$ counters. Now this kind of counter-telescope is necessary for the following reason: if you are going to measure protons, it's important to eliminate, for instance, π-mesons that come in, and some place in the absorber stack make a star with the proton, a

π changing to a proton. So the first thing we did before any absorbes in the experiment was to measure $\frac{dE}{dX}$ with the counter, and that's what this first counter is doing. Now the absorber can be placed in here, like so, and in this region, like so, in order to slow the particles down, and the fact that you'vemeasured $\frac{dE}{dX}$ and verified that it's a proton at this point eliminates the background that you get from π - meson stars. There's another rather nice thing that you can do with counter telescopes, and that is, you'll find that when you work with these things around synchrotrons in the rather forward directions that electrons coming in are building up showers in here which then scatter out or stop. They can look very much like protons, or particularly π-mesons, but they have a tail. If you look at the pulse height spectrum in one of these counters, you'll see the π's and the protons should look like this (fig. 4). In actual fact, what happens is, you get this kind of thing: you have some difficulty deciding just how much of this tail here should be subtracted out, and this is purely due to electrons that come in this region. By placing a Cerenkov counter right back of the $\frac{dE}{dX}$ counter that you're looking at, right in this region here, it's possible to make this pulse height, and you place this Cerenkov counter in anticoincidence. It's possible to eliminate all of the electrons and get a pulse height spectrum that looks like this. And this has been a very useful technique - we've been able to measure as far forward as 15° with countertelescopes set up in this fashion. Now the corrections that one has to make are for absorption and scattering in the telescope, and in the magnet one does not have these two corrections to make, so in a sense the telescope is subject to how well you can make these absorption errors. On the other hand, the magnet has the problem of not having as well-defined a solid angle: - the countertelescope is very strong in this respect, and, as we heard this morning, Wilson even calibrated his magnet this way. When we started to get two different answers on our π^0 experiment,we got worried about these calibrations, so we set up in the γ-ray beam a piece of carbon, (fig. 5) here, and put the magnet on one side, and the counter telescope on the other side at the same angle, and measured over the whole energy region that was important for these experiments the photoproduced protons from carbon, and these two curves agreed withing two or three percent of each other. So intrinsically, I think we both know very well our scattering corrections and absorption corrections in the solid x angle, and the Integral of this solid angle times $\Delta P/P$ of the magnet, I think are known quite well. And I think that this means, when we see differences in our two different experiments, it means that there's backgrounds from the hydrogen target and things like this that are causing the differences. When one has carbon in here, the secondary processes, that is, the protons so overwhelm everything else that you can think of, that all these backgrounds are unimportant. On the other hand, when you put in hydrogen, the details of the background become very much more important in relationship to the protons from secondary sources, and so I think the differences between the two experiments are background difficulties more than anything else. The pair corrections and multiple meson production corrections are made the same way for each case.

This, what we're going to do now is, look through three or four different angular distributions at different energies as one goes up (fig. 6). This angular distribution here has been obtained from the magnet, in fact this whole series except one has been obtained from the magnet data available, and I think it illustrates fairly well the overall propeties of the angular distribution. Corson and Peterson[2] in their early work at 500 MeV found a cross-section in angle that roughly looks like this, and I would interpret this differently than Wilson does, namely, I think there's somewhat of an isotopic part in it here with somewhat of isotropic a peaking in the forward direction - not peaking, but higher on this side than there is on this side, which implies a cos θ term in the angular distribution, but more or less peaked around 90°. Now we'll see that this characteristic ofangular distribution remains more or less constant till we get up to the highest energy. (Next slide fig. 7). This is at 785. The last slide was at 580 which is just on the tail of the 500 MeV resonance; this is at 785 which is close to the position of the second resonance maximum. The triangles are the points from the counter telescope, and the rest of the data - the circles - are the points from the magnet.

Well, you can see part of the differences that's going to show up between these two experiments is that consistently the counter thinks there's more counts for backwardmoving π^0's than the magnet does. In the forward direction, I think, perhaps, that you can say that there's fairly reasonable agreement. Now this (fig. 8) is the change - you remember that up until this point all of the curves were looking more or less like this. All of a sudden, at 940 MeV, one obtains a curve with a dip in it at 90° instead of a peak in it. Now Wilson draws these curves like this; he draws a straight line through these points. I think that our data is good enough to exclude this. Now this particular magnet data which is - this is at 940 so there's only magnet data available here - this particular data was made with three different arrangements of the magnet. That means the difference on this big magnet is just essentially what the focal lengths are; as you go up in energy you

2) - McDonal, Peterson and Corson; Phys. Rev. 107,577(1957)

can then be given momentum through less of an angle and so there's two different positions on the big magnet. And as one moves backward on the π^o, that means that one's trying to measure more forward protons and hence higher energy protons. Now since the details od this - I think one can argue as follows - these points were all taken with the same magnet so there's certainly no troubles with changing magnets about the slope of this curve. It may be that the target thickness or something like this was corrected wrong, but you certainly can't say that these points here are due to changing magnets, because this is the same magnet. Similarly the jump-up here - this also depends on the medium focus magnet, so I think it's reasonable to assume that there is a dip in this curve unless the corrections were made long in reducing the doubt somewhat. (Another slide shows the coefficients in the angular distribution of various parts of cos θ).

$$\sigma = A_0 + A_1 \cos\theta + \dots + A_n \cos^n \theta$$

That A_0 is the constant term - like this - this is our old resonance. The points that have been taken now match on very well. Now the situation is this, that on A_0 and A_2 these depend the symmetrical coefficients but are, I think, fairly well determined. The two different sets of points here are not the two different experiments, but an attempt to analyse the magnet experiment with coefficients up to $\cos^2\theta$ and coefficients up to $\cos^4\theta$. As far as A_0 is concerned, there's almost no difference whether one uses up to $\cos^4\theta$ or up to $\cos^2\theta$. So I think we know A_0 quite well. On A_2 there's some difference depending to how one analyses the data.

Now in the rest of the coefficients on the next slide, one will see large differences. Here's the coeffident A_1, the thing that measures the symmetry, the cos θ, the most important of these terms; and you'll notice that if he just uses up to $\cos^2\theta$, this curve might possibly match on to the old data. But it still looks a bit funny and id he tries to use higher terms than the cosθ distribution, the agreement becomes somewhat worse. Now this is reflected upon what's in the cross-section. You remember, in the backward direction, the magnet and the telescope were differing by very large amounts, namely, the magnet thinks that the cross-section falls off very much steeper in the backward direction, and that shows up in the differences on these coefficients. Some preliminary points from the counter telescope are these two points that are drawn in here. The coefficient A_3 now does not do what one would expect, namely, one would hope that A_3 would start off from 0 down to around 500 MeV, for we know it's not necessary from our previous work, and gradually grow in importance, instead, one can see that it starts off with a jump here. A_4 has more or less the kind of behaviour at least that one would hope for, it vanishing in the more lower energy regions here. So I think these experiments mean that one still has to do further experiments on measuring the value of these coefficients. We still can't assume that we know them. (Last slide showing total cross section) (fig.9). This is the total cross section, again from the magnet experiment. This curve through it is the dispersion formula, and one can see how much the total cross-section differs from the tail of the 3,3 resonance up here.

Now, there's the problem with the corrections to the pairs. You look at the following it's this thing that Wilson was mentioning again. Normally, if this is the Bremsstrahlung spectrum, (fig. 10) we tried to operate on a region out near this last flat part before it started to curve gradually, but it's not hard to extend the Bremsstrahlung limit like thi and again measure at the same photon energy. Now if the pair corrections are important, all of these photons here can make protons by means of the pair production that fall in this, that work like protons coming from π^{o} production from this energy here. So if one sees the cross-section vary rapidly as you change the Bremsstrahlung limit of the beam, then one ascribes this to pair production. Now to get an idea of how big a correction we had made to this effect, I'll give you same numbers. It was corrected by using the data of Bloch which you're going to be told about in a minute, so I'll omit mentioning it now, and we'll assume that we have a measurement for the π for the process $+ p \longrightarrow \pi^{-} + \pi^{+} + p$. Now, by essentially putting in the phase-space factors on this, the correction for the number of protons coming into this experiment that I'm talking about now, were made, and the size of the errors, the size of the corrections, in the worst case was about 30%. Now, in the worst case we could also get a situation where the cross-section was twice as large, and in the worst case, the way we normally work we have a 30% correction. By increasing the Bremsstrahlung limit, as I've been talking about, we could make that correction go up to a factor of 2. If one then corrected the data and got the same answer both ways, that is, whether the correction was 30% or whether it was a factor of 2, one would conclude that one was making the correction correctly. Well, this, tentatively then, is the situation, that sometimes the pair corrections are only a few percent, and in the case where you look at low energy π^{o} production in the forward direction, the pair correction can amount to as much as 30% or so.

Well, I think this probably conclude the experimental side of the π^{o} production. When Walker was here, he mentioned that Cohen has been trying to fit some of these angular distributions by not assuming the same state that Wilson does, assuming P-waves, but assuming that they come from the 1,1 state. Now you can get into the 1,1 state with

P - waves by means of the magnetic dipole absorption, and apparently Walker thought that there was some means of fitting the angular distributions and so forth by means of this assumption, but the work had just been started when Walker left, and he didn't know very many of the details, and I've now told you about everything that I know about it. Essentially what one will have to do to decide the question, I think, is look both at the scattering data with this assumption, and also to look at see what happens when one puts this assumption into the excitation curves with the appropriate interferences between the waves. There's one other interesting point - I won't go into the details of this, but the Bremsstrahlung spectrum, after the π^0 experiment was done, was measured, and I've got the results here, and I don't know whether you can see it or not.....

Well, our target, in spite of the fact that the radiator is 2/10 of a radiation length in thickness, looks very much like a thin target Bremsstrahlung spectrum, and it's quite sharp and flat. Now this is a nice thing for making subtraction experiments. We could certainly have worked much closer to the end-point than we did without any trouble, but we didn't know this at the time the experiment was done.

Walker and Dixon have also made some of their measurements on π^+ production. These measurements are not in as nearly complete a state as the π^0 production. Since they've only been measured once, there's no disagreement between this and any other experiment particularly. I'll pass the curve round. This curve (fig. 11) shows the second resonance coming in at around 700 MeV quite nicely. This is at 90°, and on the second sheet you can see the pieces of angular distribution that have been measured.

The details of these angular distributions are essentially as have been mentioned by Bob Wilson, namely, at 600 MeV they show the same characteristic that was observed at lower energies before, namely the peak forward and fall-off in the bachward direction. At the higher energies - there's not nearly as complete data about it - the measurements only go up to about 90° in the forward direction, so we'll have to wait for more detail on those things. Well, I think I'm through now.

Here we go. The curves that have been done most completely are 600 and 700 MeV, the curves down here which we all have fractional information on are at 900 and 1000 MeV. The reason for this is that, as one goes forward, the momentum of the pions becomes higher, and this essentially is the limit, the momentum limit set by the magnet arrangement that was used. In order to complete these curves, the magnet will have to be changed to the long focal arrangement - long focus-point arrangement - and then these curves can be continued on in the more forward direction.

M. Bloch: Experiments about double meson production.

The exposition follows the lines of Bloch's Ph. D. Thesis. This work has been submitted to the Physical Review (end of August) to be published.

A note on this experiment may also appear in the Proceedings of the Geneva Conference.

Experimental Methods for the determination of the Validity of Electrodynamics at Short Distances.

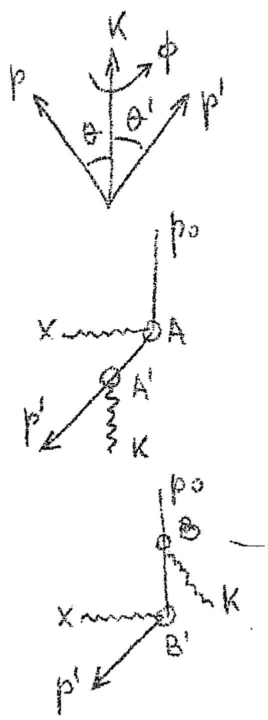
P. Budini - G. Poiani - I. Reina
Presented by P. Budini

One method which may be used to separate the effect of nuclear charge from an eventual breakdown of electrodynamics at short distances is that of comparing results from the elastic scattering electron-nucleon (or electron-nucleus) with those obtained from inelastic electrodynamic processes.

In fact, suppose for example that the Lagrange density for electrodynamic interaction can be represented with an expression of the following type:

$$L_i(x) = e\int \bar{\Psi}(x)\, \gamma_\mu\, \Psi(x_i)\, A_\mu(x_1)\, f(x_1 x_2 x)\, d^4x_1\, d^4x_2$$

where $f(x_1 x_2 x)$ is a suitable function of $x_1\, x_2\, x$, one finds that the cross-section for bremsstrahlung at high energy can be represented with the expression:



$$d\sigma = \frac{\alpha Z r_0^2}{(2\pi)^2}\frac{m^2}{q^4}\frac{\varepsilon'}{\varepsilon}\frac{d\omega}{\omega}\sin\theta\, d\theta\, \sin\theta'\, d\theta'\, d\phi\, |F(q)|^2 \cdot$$

$$\cdot \frac{1}{1+\frac{q^2}{2p_0 mc}}\left\{ \frac{A^2 A'^2 \sin^2\theta'}{(1-\beta'\cos\theta')^2}(4\varepsilon^2 - q^2) + \right.$$

$$+ \frac{B^2 B'^2 \sin^2\theta}{(1-\beta\cos\theta)^2}(4\varepsilon'^2 - q^2) - \frac{2AA'BB'\sin\theta\sin\theta'\cos\phi}{(1-\beta\cos\theta)(1-\beta'\cos\theta')} \cdot$$

$$\cdot (4\varepsilon\varepsilon' - q^2) - \frac{2\omega^2}{\varepsilon\varepsilon'(1-\beta\cos\theta)(1-\beta'\cos\theta')} \cdot$$

$$\cdot \left[\left[AA'\varepsilon(1-\beta\cos\theta) - BB'\varepsilon'(1-\beta'\cos\theta)\right]^2 - q^2 AA'BB'\right] +$$

$$+ 2\omega\left[\frac{1}{\varepsilon(1-\beta\cos\theta)}(\varepsilon\varepsilon'\sin\theta\sin\theta'\cos\phi - \varepsilon^2\sin^2\theta - \right.$$

$$- 2\varepsilon'\omega)BB' + \frac{1}{\varepsilon'(1-\beta'\cos\theta')}(\varepsilon\varepsilon'\sin\theta\sin\theta'\cos\phi -$$

$$\left.\left. - \varepsilon'^2\sin^2\theta' + 2\varepsilon\omega)AA'\right](AA' - BB')\right\}$$

where $q = p - p'$ and A A' B B' are the Fourier Transforms of the form factors relative to the electrodynamic vertices represented in the figure. The ratio between the non-local and local cross-sections for bremsstrahlung then takes the form

$$V = \frac{\sigma_{BR.}}{\sigma_{BR.Local}} = |F(q)|^2 X(A, A', B, B')$$

where the function X assumes the value of unity wherever one has that A = A' = B = B' = 1.

In elastic scattering one has instead, for the corresponding ratio:

$$U = \frac{\sigma_{EL.}}{\sigma_{EL.POINT}} = |F(q)|^2 |C(q)|^2$$

where C is the electrodynamic form factor relative to the scattering process. One therefore has that if q is the same in the two cases:

$$R = \frac{V}{U} = C^{-2}(q) X(A, A', B, B')$$

If R is different from 1, this signifies that of necessity one of the form factors at the electrodynamics vertices is different from one. The ratio R can be determined, at least theoretically, directly from experiment.

Similar formulae can be obtained for the process of pair-production.

The limits of these calculations are the following:

1) The first Born approximation is used. This signifies that the calculations are valid with an approximation of a few percent only for Z=1. For Z>1 it would be necessary to take into account hyperbolic wave functions or further terms in the S matrix.

2) The contribution of radiative corrections must be added. These corrections are calculable; anyway it in to be expected that they act in the same way on both the numerator and the denominator of the formulae. It can therefore be maintained that the magnitude of the effects for Z = 1 can be obtained to within a few percent from the uncorrected formulae.

Let us now consider the experimental possibilities for determining sensible deviations of R from 1.

Obviously everything depends on the form factor which is chosen. At the base of the argument lies the admission of the validity of the Special Theory of relativity. This implies that the Fourier transforms of the form factors must be invariant functions of the momenta of the incoming and outgoing particles. The particular choice of these functions corresponds to different points of view about the eventual deviations of electrodynamics in the local theory at short distances. Following the terminology used by Feynmann, there are two points of view:

a) the optimistic mathematical point of view:
the form factors result only from the modification of the virtual propagators in the Dyson exposition of the S matrix. This is equivalent to taking for each of the product AA' and BB' a product of two factors of which one depends only on $q = p - p'$ and the other only on $(p' + k)$ and on $(p - k)$, i.e. $AA' = C\,(p - p')\,D\,(p' + k)$ and $BB' = C\,(p - p')\,D\,(p' - k)$ respectively. In this case the factor which depends on $p - p'$ will be identical with $C\,(p - p')$ so that R will depend only on the modification of the virtual electrodynamic lines.

b) The pessimistic physical point of view:
AA' BB' are covariant functions in arguments of the type:

$$A(p,p'+k),\ B(p,k),\ A'(p',k),\ B'(p-k,p')$$

In this case the deviation is present at each vertex rather than in the virtual tract joining two vertices. This is the same as considering that the modification comes from a sort of structure of the electron, which one would have, for example, if the electron could interact with fields other than the electric field. An argument in favour of this point of view is that at high energies processes of the following types become possible:

$$\gamma + e = e + \mu^+ + \mu^-$$

$$\gamma + e = e + \pi^+ + \pi^-$$

and that therefore at those energies the purely electrodynamic S matrix cannot be unitary, Starting from completely general considerations (relativistic covariance and gauge invariance) it may be shown that the form factors at the vertices, when the two electron lines leaving the vertices are real, must be of the form:

$$J_\mu(p',p_0) = \bar{U}(p')\left(F_1(q^2)\gamma_\mu + \sigma_{\mu\nu}\, q_\nu F_2(q^2)\right) U(p)$$

where $q = p - p'$ is the difference between the energy-momentum fourvectors of the initial and final states and with the condition $F_1(0) = F_2(0) = 1$. This theorem, however, is not applicable when one of the electron lines is virtual.

The optimistic point of view has been studied in detail by S.D. Drell who shows that, if one puts:

$$C(q) = \frac{\Lambda_f^2}{\Lambda_f^2 + q^2} \qquad D(q) = \frac{\Lambda_e^2}{\Lambda_e^2 + m^2 + q^2}$$

with m = mass of the electron, the limits of Λ_f and Λ_e given from a study of known experiments are

$$\frac{1}{\Lambda_f} < 0,3 \cdot 10^{-13}\ cm = \frac{1}{\Lambda_{f_0}}$$ (from elastic scattering electron-nucleus)

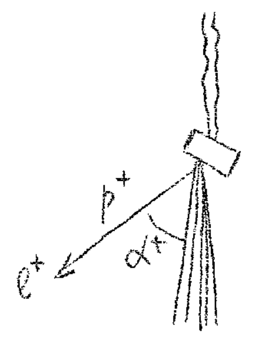
$$\frac{1}{\Lambda_e} < 5 \cdot 10^{-13}\ cm = \frac{1}{\Lambda_{e_0}}$$ (from Lamb shift)

Experimental Methods.

Let us now examine the experimental possibilities for lowering the limit of $1/\Lambda_e$ when one accepts the optimistic point of view.

a) Pair Production.

a 1) Detection of one member of the pair.



In this case when α is sufficiently large one can arrive at measurable values of $\frac{R-1}{R}$ when $\frac{1}{\Lambda_e} < \frac{1}{\Lambda_{e_0}}$. For example, Drell calculates that, for k_{max} = 140 MeV, p^+ = 90 MeV/c,

$$\alpha = 90^\circ; \quad \frac{R-1}{R} \simeq 0.05 \ \ for \ \ \frac{1}{\Lambda_e} = 0,7 \cdot 10^{-13}$$

In this case one can not go beyond k_{max} > 140 MeV since the production of pions becomes important. The ratio pion electron at 500 MeV is

$$H \begin{cases} 30^\circ & 3,5 \cdot 10^4 \\ 90^\circ & 5 \cdot 10^5 \end{cases} \qquad Pb \begin{cases} 30^\circ & 1,2 \cdot 10 \\ 90^\circ & 3,4 \cdot 10^3 \end{cases}$$

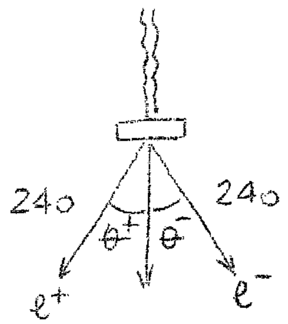
It is not possible to measure the recoil of the nucleus since the negative electrons which provide the maximum contribution to the deviation of R from 1 are those with $\alpha^- \neq$ angle of maximum frequency $\simeq$ m/k.

a 2) Both members of the pair are detected.

This is an experiment of the type already proposed by us (see 'Esperienze con l'Elettrosincrotrone, verbale della riunione tenutasi a Roma presso l'Istituto di Fisica dell'Università nei giorni 1 e 2 ottobre 1956', page 43 and following) in which one condition was that the momentum transferrend to the nucleus was a minimum. The experiment will be carried out by Panofsky with $\theta^+ = \theta^- = 30°$; $p^+ = p^- = 240$ MeV/c. With a beam of 10^{13} electrons/ second and a thichness of liquid hydrogen of 10 cm., and with

$$d\Omega^+ = 0{,}007; \quad d\Omega^- = 0{,}08; \quad dE^+ = ?$$

one obtains 15 events per hour.



A determination of R with a precision of 10% would allow $1/\Lambda_e$ to be taken to 0,3 . 10^{-13}. Extending k to 1000 MeV in the same conditions one would have, with a measurement carried out to a precision of 10%, $1/\Lambda_e \sim 0{,}08 \cdot 10^{-13}$. We have calculated that for $d\Omega^+ d\Omega^- dE^+ = 10^{-3}$ there would be about 3 events per hour (10^{10} electrons per pulse). In this case, however, the solid angles could be increased owing to the longer duration of the pulse from the machine with respect to that from the linear accelerator used by Panofsky. It is to be born in mind however, that notable increases in the energy lead to increases in the cross-section for the production of pairs of $\pi^+ \pi^-$.

One might hope to distinguish electrons from pions by kinematic considerations: it turns out that this is possible, above all when one detects pairs which are not symmetrical in energy.

b) Bremsstrahlung:
Both schemes a) and b) shown in the figure can be used. Scheme a) could perhaps be realized with an internal beam. It presents the following advantages:

1) For $\theta_K = \theta_p$' one has a frequency maximum in a zone of low background. One can therefore carry out relative measurements of R, which would be equal to one if $\theta_k = \theta_p$'.
2) The production of pions is presumably reduced by a factor of 100.
3) It might be possible to work with Be since the angles can

be varied while keeping q fixed.

On the other hand the disadvantages would be:

1) the nucleon recoils with high momentum and therefore there is the possibility of gamma emission by the nucleon (calculations of this effect are in progress).
2) The difficulty of working within the vacuum chamber with a target of liquid hydrogen.

The predicted frequencies for a target of 5 cm liquid hydrogen are:

$$\left.\begin{array}{l}\theta^{+} = \theta^{-} = 90^{\circ} \\ d\Omega_{\varepsilon} \cdot d\Omega_{\omega} \cdot d\omega = 10^{-3}\end{array}\right\} \nu \sim 10^{3} \text{ events/hour}$$

$$\left.\begin{array}{l}\theta^{+} = 90^{\circ}\ ;\quad \theta^{-} = 75^{\circ} \\ d\Omega_{\varepsilon}\ d\Omega_{\omega} \cdot d\omega = 10^{-3}\end{array}\right\} \nu \sim 0{,}5 \text{ events/hour}$$

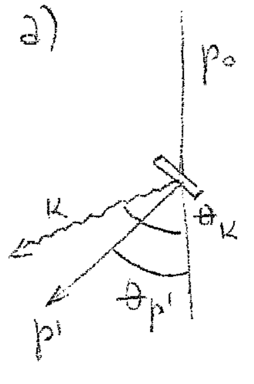
Scheme b)

Advantages:

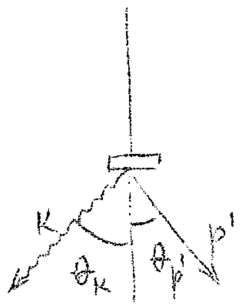
The momentum transferred to the nucleus is small and therefore the possibility that the nucleon radiates is also small. A hydrogen target may be used.

The fundamental disadvantage (for Frascati) is that one must work with the external beam.

a)



b)



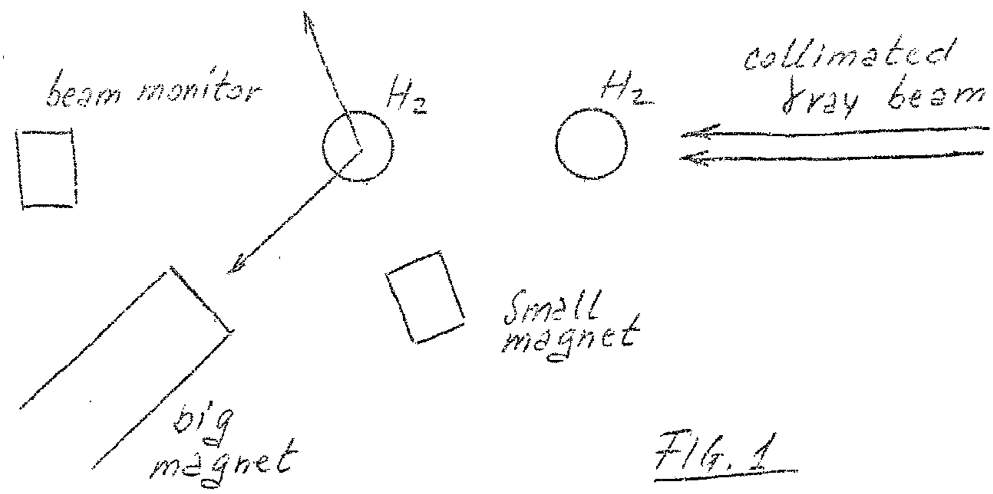


FIG. 1

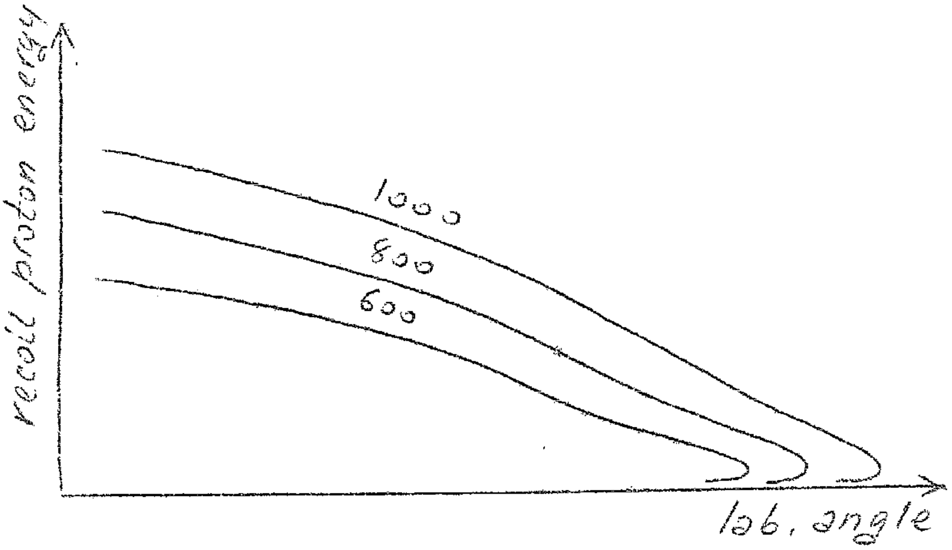


FIG. 2

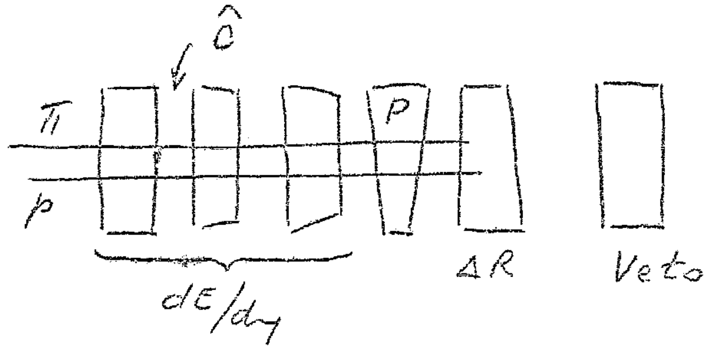


FIG. 3

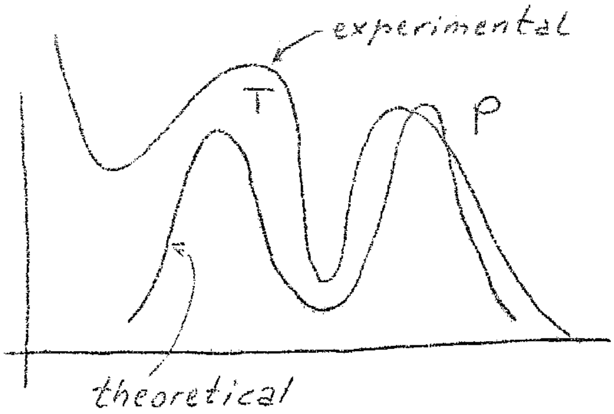


FIG. 4

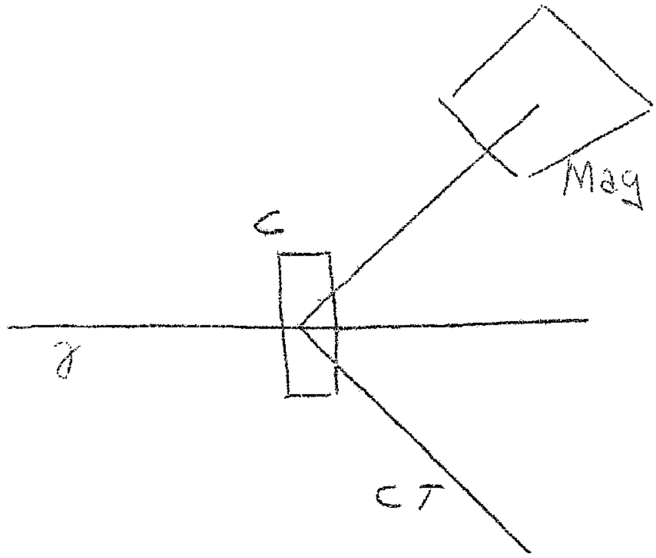


FIG. 5

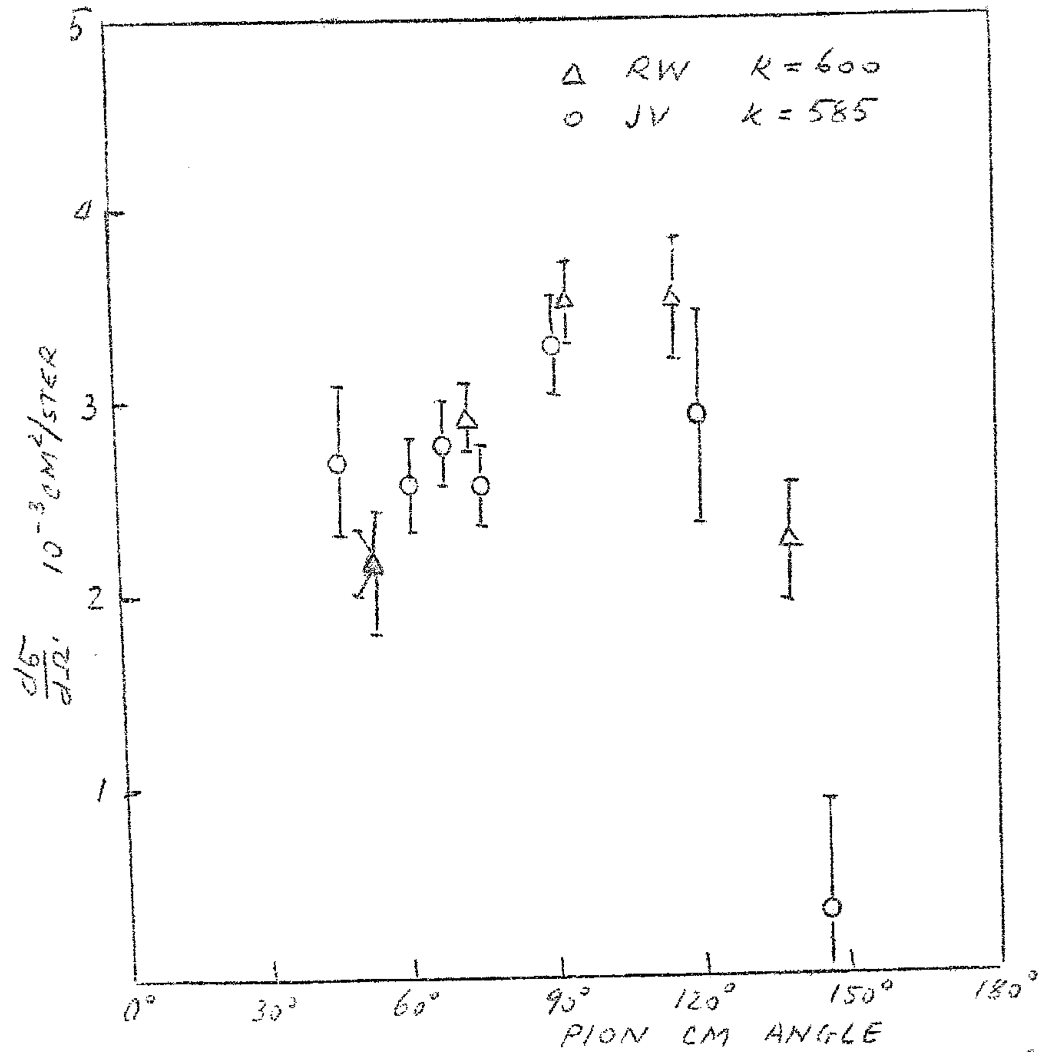
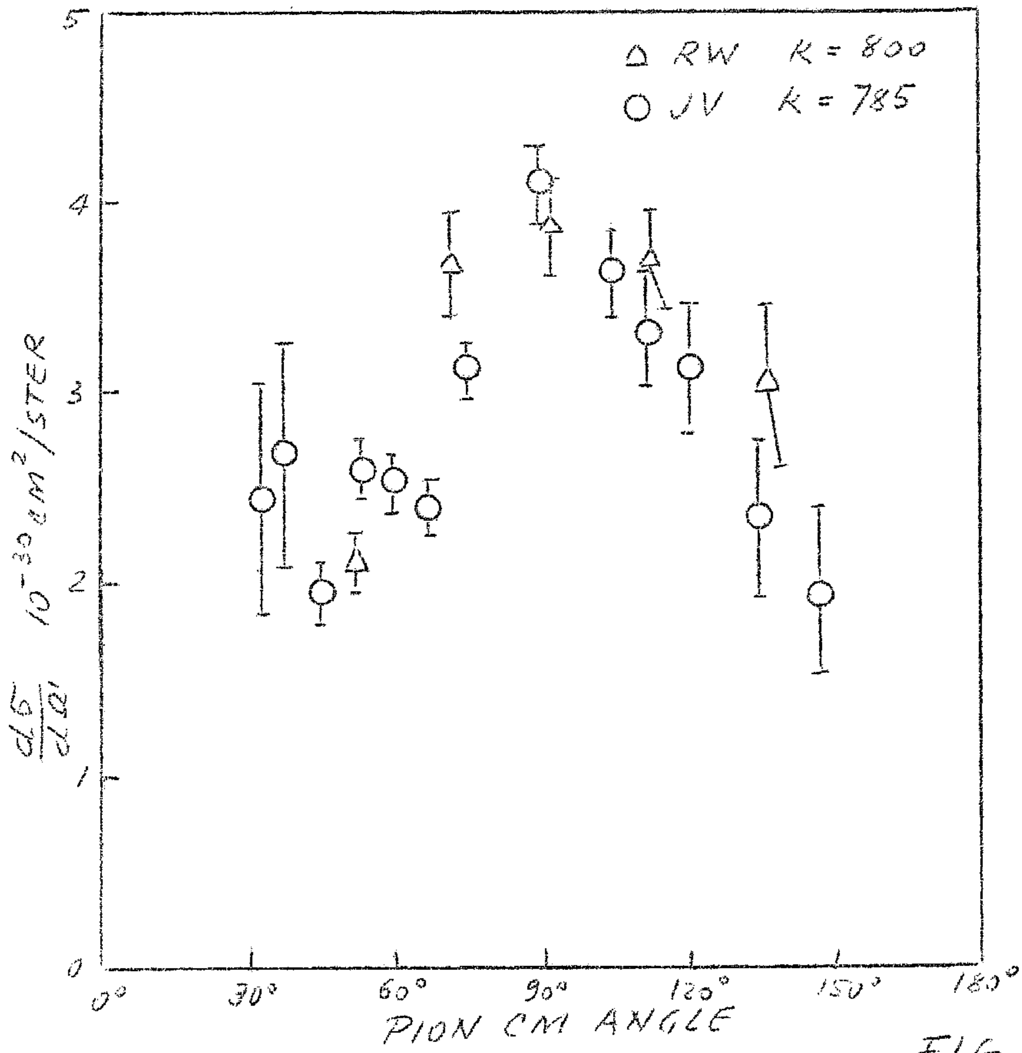
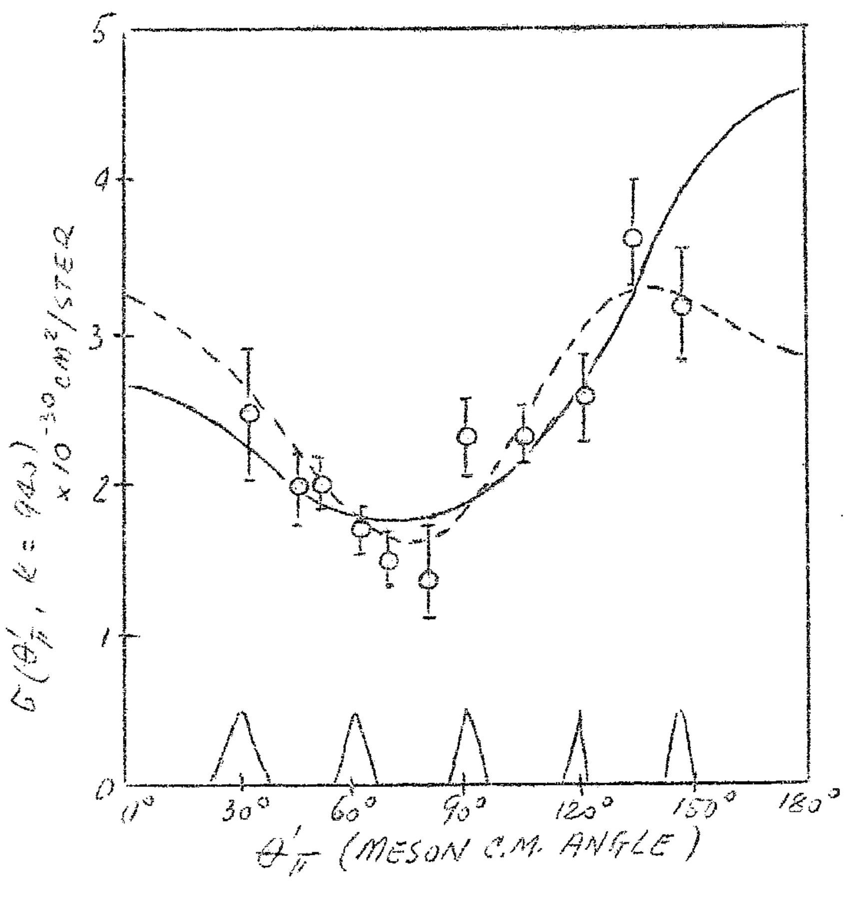


FIG. 6



△ RW k = 800
○ JV k = 785
$\frac{d\sigma}{d\Omega'}$ $10^{-30}cm^2/STER$
0
1
2
3
4
5
0°
30°
60°
90°
120°
150°
180°
PION CM ANGLE

FIG. 7



$\sigma(\theta'_\pi, k = 925)$ $\times 10^{-30} cm^2/STER$
0
1
2
3
4
5
0°
30°
60°
90°
120°
150°
180°
θ'_π (MESON C.M. ANGLE)

FIG. 8

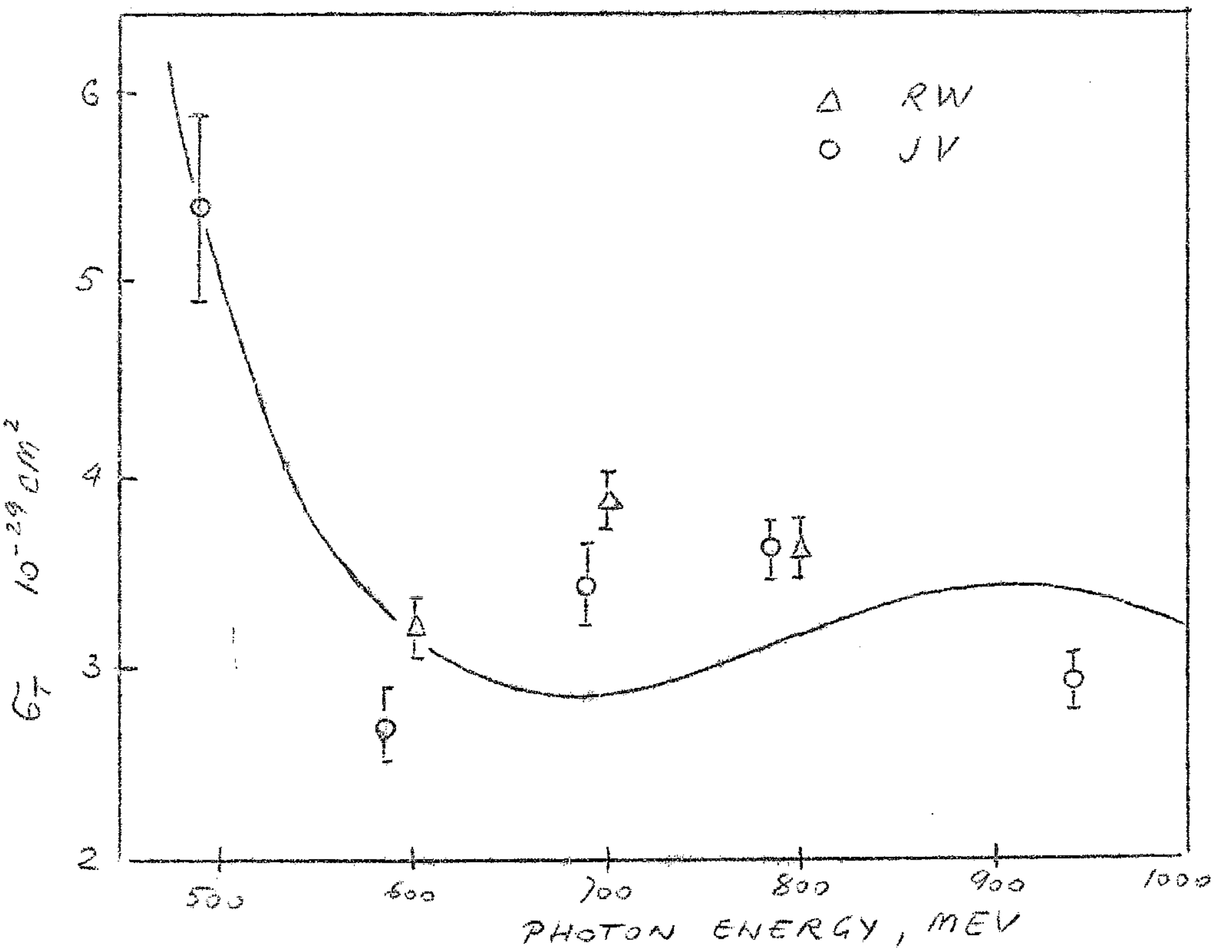


FIG. 9

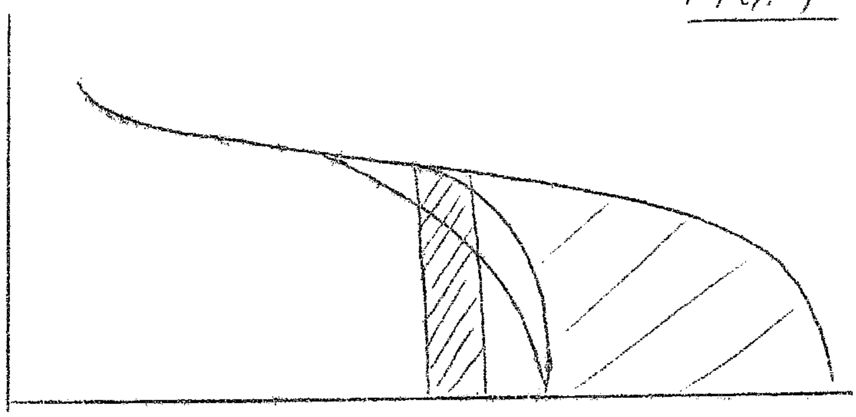


FIG. 10

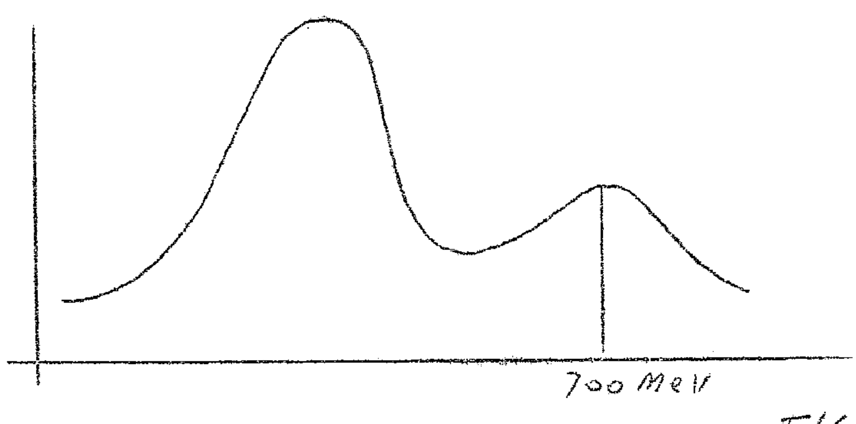


FIG. 11

I N D I C E

MEETING ROMA - FRASCATI 'AFTER GENEVA'

(Verbale della riunione del 10 Luglio 1958.)

Parte II

A cura del Servizio Documentazione
dei Laboratori di Frascati dell'I.N.F.N.
Frascati, dicembre 1958.

ISTITUTO NAZIONALE DI FISICA NUCLEARE
Laboratori di Frascati

MEETING ROMA - FRASCATI " AFTER GENEVA "

Verbale della riunione del 10.7.1958, presso i Laboratori di Frascati dell'I.N.F.N.

R.R. Wilson: Future exchange of experimental information among Cornell, Caltech, Bonn, Frascati.

(Trascrizione a cura di G. Cortellessa, C. Infante, A. Turrin)

I was asked to talk about collaboration between the Laboratories in this new field.

There are some obvious points to agree on:

The first of those is clearly calibration. We must use the same calibration, so that there is no trouble after a few years to compare experiments not using the same monitoring device.

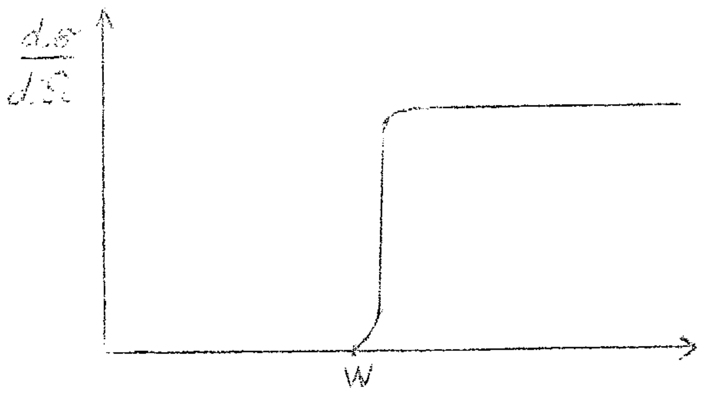
Experiments on γ - rays discredit proton physics; no comparison could be made because one couldn't determine how to measure the γ -rays. About the calibration we should all speak exactly the same language.

The 1-st thing is you want to know the energy of the electrons. This is a fairly simple problem, in fact not much discussion is necessary, as you can measure it by many methods.

By energy of the electrons one usually means the energy of the electrons on the equilibrium orbit; in fact you put a target in the doughnut on the inside or on the outside. If on the inside, you turn off the R.F. (slowly of course) and allow the electrons to spiralize until they collide with the target; you have thus a loss in mean energy.

You have to be careful because the nominal energy is not the energy of the experiment. The best way (there are two) used at Cornell, tells the energy of the electrons striking the target.

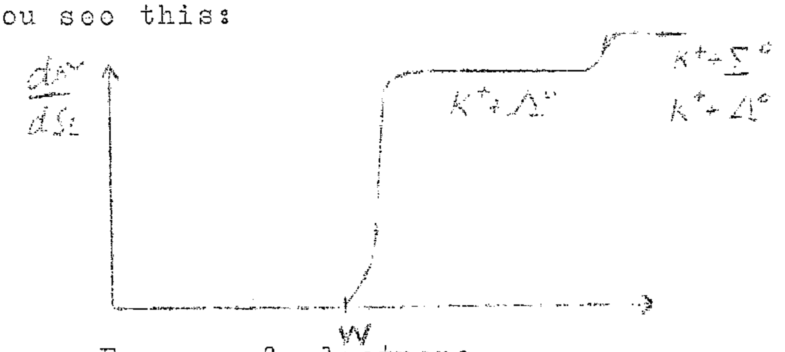
It consist of running the excitation curve of the process $K^+ + \Lambda^0$. We'll talk later about the experiment of measuring the process $K^+ + \Lambda^0$. In anycase you'll have a device to measure the K^+: then you vary the energy of the electrons and measure the number of K particles. This was done at Cornell; what you obtain is this:



Energy of electrons

The curve has probably a very steep threshold; you measure a certain energy of K^+ at a given angle. The two-body process thus defines a certain energy of the γ -rays; if the energy of the electron is equal to the energy W, the process does not go; as you go through the threshold the process does go. This threshold is about 10 MeV wide.

We have also seen another process: $K^+ + \Sigma^0$, in which case you see this:



Energy of electrons

At Cornell we have seen both: since the masses are known very well, we automatically get a very good calibration point for the machine. You have to know the energy of the K^+, but most of the energy comes from the masses of the K^+ and Λ^0, both of which are known well.

One can also calibrate with protons, e.g. $\pi^+ + p$; you can extrapolate to some kind of a threshold, and again you have a calibration point for the machine.

These curves refer to the upper energy of the bremsstrahlung beam which should be close to the energy of the electrons. Actually the energy of the γ - rays is the energy that enters the experiment.

I think this methold will be used in every laboratory as it is accurate and quick; you can do it in an afternoon here at Frascati, where you have intensities 10 times higher than at Cornell.

It took us one or two days. You need a check against this with a pair spectrometer. You actually measure the energy of the γ - rays. This has been done at Cornell and at Caltech.

A discrepancy of about 3% was found between successive measurements at Cornell. This was presumably due to the magnet of the pair spectrometer. I brought this up to show that one must always make several measurements, because these errors can creep in.

Salvini: What is the standard error?

W: I think you can measure these things to 1% very easily, to 0.5% if necessary. We repeat these measurements once a week to be sure that the ratio between these measurements stays the same.

This is important to see that some accident doesn't occur to some meter. I believe it is best to compare the calibration every week.

About measuremons of the electron beam: we measure the average energy of the machine, but there is ne way of knowing that half the beam is greater in energy than this and half is smaller. You could build an integrator that automatically integrates and averages the energy of the electrons as they come out; something that measures the intensity of the electrons and assesses an average value by itself, a computer-like circuit.

We're building such a thing at Cornell; we don't yet know how it'll work. In this way a meter, when there's a beam automatically indicates the average energy. This can be done with an integrator which cuts-off in some peculiar way. The usefulness of this would be that an experimenter could look at this meter during a measurement and write down the average energy of the beam.

Quercia: For the [illegible] measurement of the energy of electrons at a given time during the accelerating cycle we are conside ring two different apparatus.

Both have the purpose of measuring the value of the magnetic field in the gap of the synchrotron when a pulse is produced by an external event (e.g. at the beginning of the decrease in amplitude of the RF_2).

The first apparatus is simply a numerical integrator, the operation of which is started by an unbiased peaking strip and is stopped by the pulse due to the external event. The value of the measured integral of the dB/dt is presented on a dial as a number with two figures. We shall use this appa ratus for rough measurements with an accurancy of about 10%.

The second apparatus, a model of which is now under test, is a biased peaking strip. The bias field can be pulsed up to about 10.000 gauss by a suitable current produced by triggering a thyratron tube.

The amount of current which is necessary for switching the peaker is measured, and is proportional to the value of the magnetic field to be measured, at the time the pulse of current occurs in the biasing coil. This device is suitable to be accurately calibrated in a d.c. magnetic field; the latter, in turn, can be measured with a high degree of accurancy for instance by nuclear resonance methods.

Bernardini: You can build an apparatus that makes a weighted average: e.g. you know that the K^+ threshold starts with an energy (or momentum) dependence which is linear: you thus know the weighting function and you can build an electronic device wich produces this weighted average of the energy.

W : There is a pitfall in which many people are liable to fall . When some physicists stop working on the machine and start working with the machine, they loose the feeling of the machine itself. In other words in an experiment the quanti ty of interest is the energy of the γ -rays, not the nominal electron energy.

The next problem is the Bremsstrahlung spectrum. We have some difficulty with Caltech about which spectrum to use: we use the thin target spectrum, and then Caltech put out a curve for a thick target. At first we didn't believe in it, but later we decided to use something halfway between a thick target and a thin target, to compare our notes. As we had done that, we were in trouble because that is no way to present results. It has turned out that the best measurements do seem to indicate a thin spectrum. The best thing to do is to present results on the basis of a thin spectrum, that is the theoretical first; and then, if you're going to do something different, give a second result based on some sophisticated measurement made at the time by something specified. We should all agree to give our numerical results in terms of some idealized spectrum that we'll all adopt, so that we can compare the numbers. Then if we don't believe that, we can give a second result wich may be based on something else. For instance, at Cornell we have a special thick target, we actually put up a spectrometer and we measure what the energy is, and in that case we get something else. And you can check with that. But I really think we ought to start with the same kind of spectrum each time. I don't know if we can reach agreement now, but we should (when we compare numbers), always choose the same spectra. At present between Caltech and Cornell we always use the thin target spectrum.

Salvini: What do you mean by thin target?

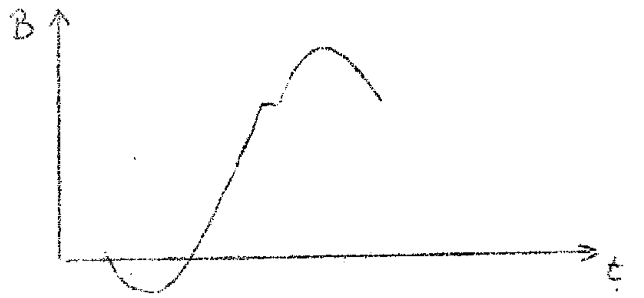
W.: By thin spectrum I mean the spectrum you get by using the Bethe - Heitler formula. At Cornell we have a series of curves at 100 MeV intervals.

S.: What about the calculation?

W.: In Segre's thick book on Nuclear physics, there is a part written by Bethe where he gives all the formulas for calculating those curves, and he gives the numbers of these curves. We have made these calculations, at Cornell, and we sent the results to Caltech. However you shouln't believe these data, but calculate those by yourselves. If there is a difference, we shouldn't each choose our own special calculations, but we ought to fight it out until we agree. In the meantime we could use one or the other because we need same standard curve.

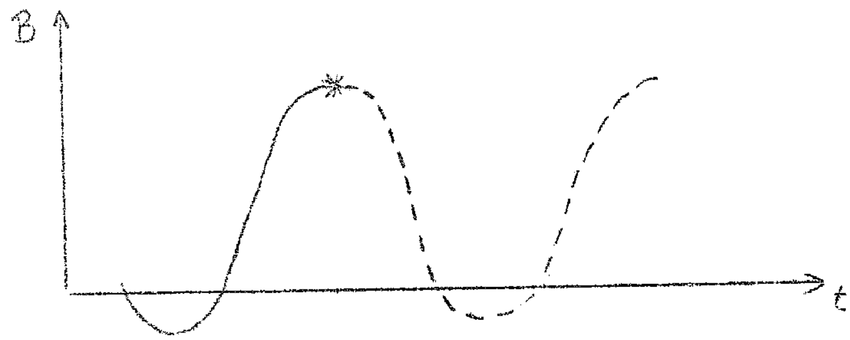
I might add that all experimental curves given so far are in agreement whith this theoretical curve, in the upper region. There is some slight discrepancy at the low side, but I don't know of any experiment in which you use the bottom end of the Bremsstrahlung spectrum; you always use the top. In that case the measurements and the Bethe - Heitler formula agree. Anyway one should not work at less than 50 MeV from the top. At Cornell we work at 150 MeV from the top.

Speaking of the energy of the electrons against the internal target, and the length of the pulse, if you can incorporate the thing they do at Caltech in your machine it would be very valuable. They put a step in the magnetic field, by clamping it somehow.

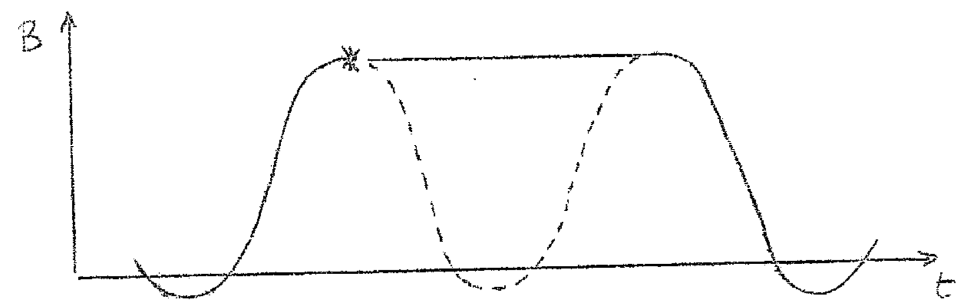


The beam comes out with great homogeneity, and of course you can move the step up and down. We'll try to do something like that at Caltech; my opinion is that it would be worth a lot of money, say 30% of the cost of the machine; also here at Frascati.

Another thing I'm dreaming about is the following: to clamp the machine at the top, instead of letting it go down and letting it come up again:



That is to miss the next cycle, and to clamp it here, held it through a whole cycle,



and then let it go down again so that the beam can be held for better than 50% of the duty cycle.

The great advantage of Synchrotrons that makes them the joy of experimenters, is their long pulse times. On this aspect the Stanford LINAC is competitionally just nothing because of its short duty cycle.

This device is not such a technical problem and can be done.

S.: Is this an electrical device?

W.: There is a "General Electric" mechanical switch that will do this, for example.

This won't be done this year or even next year maybe, but sometimes this may make a lot of difference to the kind and quality of experiments that can be done with these machines.

The next problem is the determination of the equivalent Q of the quanta. Here we have up to now confusing situation with the adoption of different calibrations between Cornell and Caltech, with disagreements within Caltech of about 14%; e.g. Cornell, Caltech, Illinois and Berkeley all used differe nt calibrations. I don't think any of us ever got things straightened out, except that we were in rough agreement. We didn't even know what standard pressures and temperatures we were referring to.

I hope this doesn't happen in the future.

There were differences as much as 20% between Laboratories, not resolvable, simply because we didn't know what our inter - calibrations were. You're lucky to be talking with somebody who has a solution for this problem.

I'm speaking objectively because I realized this difficulty when we started to get our machine in operation two years ago, and so I posted a 1 $ prize to anybody in our Laboratory who came up with the best device for monitoring the equivalent quanta. Naturally, I modestly made my own contribution, and strangely enough I won the prize.

The device I designed is called the Quantameter, and I would like to recommend this strongly as a standard between Laboratories.

Salvini: Is it enough to see the description of the Quantameter, or have some changes been made?

W.: No; we used the Quantameter as described in Nuclear Instruments 1,101 (1957) . I only want to add that we used two of these, one with 10 plates and one with 20. They had exactly the same calibration, to 0.5%. I really think this device will prove to be quite accurate. I add that the device is absolute, in the sense that you can calculate its response probably better than you can calibrate it with a pair spectrometer, because the only thing you have to know is the relative ionization loss between copper and argon plus CO_2, that you can calculate very accurately. On the other hand you must know the energy per ion - pair of the electrons in the gas, and that too has been measured by many ways. All these agree to about 1%, this could be measured more accura tely; but the only number you have to know is the energy per ion - pair.

Then one should know the absolute calibration. The other things are measurable, e.g. the width of the plates and of the gap. These things can be measured after the whole device has been assembled, and you can know them to a fraction of 1%. Any way unless somebody has a better device in mind, I propose we should all use this. We sent the plans to Caltech; there Gomez had one built and the calibration between the two agreed quite closely: I believe to a fraction of 1%.

We had built 3, and they too inter - calibrated exactly. And I think you can built them confidently and get the same results in all Laboratories. I will strongly urge the use of these Quantameters, and I should also strongly urge the absolute calibration by means of these calculations, until so mebody finds a better way of calibrating them, and then we should all get together in some fashion and agree on a new calibration.

Salvini: One of us, Dr. Murtas, has started to build one of these Quantameters. Let'me ask this:
I understand that the calibration of this device can be made in an absolute way. Would it be possible to have a Quantameter to borrow? As we are building one from the drawings, the only doubt is the kind of materials which could affect the recombination.

W.: I think you should be able to build one. Because this thing uses copper, and if there 's one thing you can get pu re in a Laboratory it 's copper. Copper is available electro litically pure in all countries. Argon and CO_2 are also easy to get pure. We'd be glad to send one through; we have an extra one that we can keep travelling between Laboratories as inter - comparison.

S.: Unless we arrange things this way: one of us, for instan ce Cornell, builds these devices for all the Laboratories.

W.: Well, it might be a good idea.

Ehrenberg: What is the effect of variations in the collection voltage and of impurities in the gas?

W.: As I recall, if the collegation voltage is 200 volts, va riations up to 20 or 35 volts have no effect. As for the gas, it turns out that the calibration constant for pure argon and for argon plus CO_2 is the same.

With high intensities, however, the use of CO_2 is advi sable to keep the multiplication down.

Bernardini: I think you said the response of the Quantameter is absolutely independent from the maximum energy of the electrons. I wonder what the influence of the nuclear proces ses is: e.g. nuclear processes producing ionizing particles,

with a cross section strongly energy - dependent.
W.: In my article I wrote about this, but maybe not enough
I believe that if you're satisfied with 1% accuracy you sho-
uldn't worry about this: for 0.1% accuracy, you might have to look it.
Quercia: How much current does the device produce?
W.: I don't remember the actual figure. I do know it is high enough not to give any problems. Dr. Littauer built a conventional integrator for use with the Quantameter. I'd say roughly 10^{-11} amperes.
Ehrenberg: What about collection voltage?
W.: The question is rather important. If you plot the response of this device, with a given beam, as a function of the collection voltage, then your greatest danger is that you put too much voltage: if you put 600 volts with pure argon, then it starts to multiply. If you're talking about multiplication of 1%, that 's a pretty low value of E/p of course. If you add CO_2, the electrons energy is of course lower, and everything is better. You can go to higher voltages without multiplication. If you go to really high intensities, there may be some problems about recombination. But then you may have to use some other device, such as that used at Stanford, an electron emission device. But again I believe you can always inter - calibrate the two, by using the Quantameter as a Standard.
Tollestrup: One criticism that can be made is the following: all those gas counters have non - linear effects. It was found comparing a Čerenkov counter, known to be linear, with the old Cornell chamber, that the latter showed some non - linearities amounting to about 20%.
W.: The old Cornell chamber was probably the worst chamber you could get because: A) it had a 1 inch collection gap as opposed to a 1 ÷ 2 millimeters collection gap in this thing; B) There was an error due to the uncertainty of the pressure: this error is notoriously bad.
T.: I mentioned that, because it was my impression that when you got to a plateau you collected all bearers, but I don't believe that any more. My objection was meant as a warning. The second thing I wanted to know is how you measure the ratio of the energy loss in the copper to the loss in the gas. As I understand, it 's quite hard to determine the loss in copper. I would encourage people to calibrate this thing with a pair spectrometer, until these problems are understood a a bit better.
W.:I don't agree with you. I think you should calibrate this thing differently as soon as there is another method. But you remember there was a 14% discrepancy between the pair spectrometer and the shower method at Caltech, and as far as I know this has never been resolved. Any pair spectrometer is, I believe, incapable of an accuracy much better than about 10%. If one handles and calibrates them with extreme care, one might do better, but I haven't seen anyone handle

them with such care yet. As soon as there is a better method of calibrating the Quantamoter it should be done, and we should fight it out and in the end agree or disagree about the new number. At present there are no numbers that I would believe, with the possible exception of the method at Illinois. That method agrees exactly with the present one, as far as we were able to make the comparison. About the energy loss I disagree with you; I believe you can calculate the loss between the Cu and the argon: this is not a sensitive effect. It's true you do have to put in a calculated correction for the density effect, but I believe this is known very well from the curves of Sternheimer: the latter has made some important work on the subject (Phys. Rev. 88, 851, 1952, ib. 103, 511, 1956). This work has been tested experimentally and rather frequently. I think you do know about this rather well; the only theory that comes in, not very, exactly, is that you have to know the tracklength. But whether you calculate that for air or for copper, the correction does not make any difference. The correction comes in very insensitive - like.

Tollestrup: If you read the data on Segre's book, the uncertanty on the density effect is quite large. He also makes the statement that it would be nice to have some better numbers. Maybe Sternheimer has done this.

W.: Sternheimer's work come much after Bethe's article in Segre's book. I believe Sternheimer made much better calculations.

Salvini: In this Laboratory we are developing two projects for the calibration. One is a calorimeter with Hg for measuring the Q.(x) The second is your Quantameter. We will check these two methods between each other when we have the beam.

W.: I just want to say that the calibration of the Quantameter agrees with the measurements made with a pair spectrometer by De Wire at Cornell. It agrees, as far as I know, with the Illinois method; when we get more data we might find an interdependence. Until then we should continue to use the number in the original paper on the Quantameter. When we change, we should really have a fight about it, all get together and then change it so that all our cross sections will change together; not start to adopt something at Frascati, something else at Bonn, etc.

Salvini: The calorimeter method probably can hardly be pushed to a precision of 1%, so that as far I can see, the only quantameter that should reach the 1% level is your Quantameter. It would not be easy to check your Quantameter, the calorimeter and the pair spectrometer within 1%.

W.: In that case one ought to use one number, it doesn't matter whether it's right or wrong. We ought to use the same number and then make departures. We might change that number sometime as I say, after a fight.

(x) Cialdea is trying to use the dilation of Hg for measuring the Q.

S.: This is a little bit of a philosophical implication: Suppose that our four Laboratories work with the same calibration, wich is wrong by say 3 or 4%.

Do you think that this would give us a better understanding of physics, rather than working with different calibrations with an uncertainty of 5 or 6%?

W.: Yes very much so. You see, you can give your numbers and the calibration you believes together with the data obtained on the basis of the 'international' calibration.

It seems to me, that the first thing to do is to just compare our numbers between Laboratories.

S.: Could you calibrate your Quantameter when you put on the beam a LiH hardener?

W.: The Quantameter measures the equivalent quanta defined as follows: you have to determine the energy of the electrons as determined by the Hydrogen. You measure the total energy on the Quantameter. This is the only thing that it will measure, and it'll do it very well. If you know something about the spectrum then you might go out the quanta.

S.: The problem is then this: in order to get the quanta you need the spectrum; and how much is the uncertainty on the LiH Bremsstrahlung spectrum? For instance, how does Cocconi solve this point?

Cocconi: Personally I use the spectrum: (see photograph at the end) shown on the slide.

Quercia: I believe that to have an absolute calibration of better than 1% has no meaning. In fact in the measurements of cross sections there are other sources of error whose influence is usually much greater: e.g. the density of the hydrogen used for the target.

Jentschke: What Quercia says is logically obvious: as long as there is no accurate method of calibrating the Quantameter in an absolute way, the influence of this error will never show up.

W.: Of course later on we can change the old numbers in the linght of new numbers. As for the liquid Hydrogen, we should make sure we use the same density. You can find different densities in differents books, so we should all agree to use the same handbook for consultation.

Tollestrup: Shouldn't we correct for the bubbles?

W.: Of course, one should state in the paper that the Hydrogen was bubbling and you made some sort of an optical measurement of the average density that brought it from the stan dard value to some other value. But at present if you look through the literature, you find that they use values that differ quite appreciably for the density of the Hydrogen. The National Bureau of Standards has put out a number that I believe better than the one we 've been using at Cornell.

Perhaps we've discussed this problem quite enough.

The next problems are more difficult to discuss, as they have to do with the collaboration between Laboratories on the actual problems we have to work on.

Tollestrup: There are a lot of numbers that come in to an experiment if it's an important one: if one 's trying to compare π^o cross sections between Caltech and Cornell, it might be nice to by - pass a lot of these problems and refer the cross - sections to counting rates from C at 90° let's say from the numbers of protons,

or something like this we could all agree on: we could normalize the angular distribution so that if you have a point we could plot it on our curves and vice - versa.

W.: That's an excellent idea: of course we'd make all our measurements on Hydrogen or deuterium.

This is a good standard and this is the way to do good physics in this field. But occasionally you do use C as a source of protons, electrons and pions for calibration purposes. We all should measure the C too as an inter - comparison. We could then refer to the counting rate of C. Another thing we do is to refer K to protons in Hydrogen. (The protons usually coming from the π^o reactions). These calibrations would be indipendent from the magnet calibration.

Not give the numbers of say, protons, but the ratio of say K to pions or of K to protons, whith you always do measure anyway. Eventually we should have curves to pass around of the protons from C.

Salvini: For instance try to specify some of these standard measurements. To what reactions were you referring?

W.: Well, C(γ , p). You bombard a piece of C with γ - rays and at the angle of the experiment and with a given momentum, you then measure the numbers of protons coming from a C standard target.

You might be measuring K^+; you'd have a liquid Hydrogen target. In that experiment you'd measure the number of K^+, the number of protons from the Hydrogen and the number of protons from C; there would be no problem.These two numbers should be checked between Laboratories, as the ratio should be the same.

Salvini: How long do you think it would take to make a good 1% measurement with C?

W.: As there are many protons it should take a short time.

Bernardini: The experiments are probably easier at low energies: at high energies it is harder because you have a question of solid angles: this is important because if you choo se a small angle you don't worry too much, but if you choose wide angles, the kinematics becomes immediately involved, and the definition of the solid angle is a very important point. In other words this comparison is not so easy as it looks.

W.: Of course. Whenever I see Walker we immediately start talking about the ratio K^+/protons.

The second part of the collaboration is also very important, and that's the question of kinematics. In every experiment there's a lot of kinematics: if you're doing say

$$\gamma + p \rightarrow K^+ + \Lambda^0$$

at a given energy there's a certain relationship between the energy of the K^+ and the energy of the γ that made it, and the angle at which they come out, and the ratio of the cross section in the center of mass to the cross section in the Laboratory. We have curves of these at Cornell and Caltech, and I think you have a program of computing these things here; of course we should end up with the same set of curves. This is a problem of communication; we should be very careful that when we make a set of curves of any kind in one Laboratory, these curves are sent to other Laboratories for comparison. We should all have the data and inter - compare to be sure there's no discrepancy in the kinematics. The main reason for this is to discriminate against errors.

Salvini: In the last couple of months, we wrote to all the Laboratories interested in kinematics to ask them which kind of kinematics they had done, what book they had used etc. We had replies from pratically all of them. Now one us, Dr. Turrin, is trying to get all this material to see just this point, i.e. masses, discrepancies, and what can be done about it. We could send a report on the result of this inquiry.

After that we could propose same mass for these calculations. We could also make calculations if you're interested.

Cocconi: You're putting together a radiation handbook. Wouldn't it be a good idea to put out a regular issue to have a complete set of these curves? You couldn't do this from the beginning because it depends on the problems. It would be nice to have a copy of these graphs available for everybody. We could set up a committee for collecting all this data and sending it around.

Bernardini: Tollestrup dit an extremely good job for CERN with a very good agreement with all the mathematicians.

Salvini: CERN has made a kind of general preparation and it would cost around 30,000 Lire per reaction to have the special kinematics done.

Wilson: We could start to talk about collaboration between Laboratories and it gets tough. I said tough because I know that it is difficult to get collaboration even inside your own Laboratory. In such a big field like this is (I had this impression working on the K - mesons experiments and also on the π^+ - mesons experiments) I get discouraged when I think that all is to be done, and if Cornell were the only machine working, I would be very unhappy because there is just too much to be done. It requires a lot of effort and I would hope that it shall not develop as in the past.

I remember that we first made a measurement on π^+; than in the some other place (I don't remember where) they have made also a measurement on π^+; and they agreed, and we were happy. Then a second place has made a measurement on π^+, and they agreed too, and this made us more happy. Then, after that, some other fourteen places have made measurements on π^+, and they agreed too. This was a waste of time, because the errors were always the same and we didn't learn anything more.

Maybe it'sbetter that people get together and decide to measure something crucial in some other region.

If we start to talk of collaboration,I think we have to take in mind that each Laboratory has it's own style in every Laboratory we have individualists, we have things on which we want to work.

There are two ways for having collaboration: the first one is very good communications, so that you avoid overlap. I think we have good communication - both on things to be done or already done - between Cornell and Caltech; when we have finished our experiments we communicate on the phone and we try to exchange people as visitors. But we could do something more than that. Going to the extreme, we could set up a committee with representatives from Cornell, Caltech, Frascati and Bonn, and say you do that and you do that, and devide the work. That wuld be a kind of formal committe, of course there would be some repetition of work. For instance, I know that you in Caltech have the principle to repeat a measurement twice with different methods. And in Cornell we also try to repeat the measurements. I think that that is something we are not to do in the future, because it is enough or even better to repeat the measurements in different Laboratories. If there is a disagreement, then a third Laboratory could repeat the measurements.

Salvini: This committee you are talking about is supposed to discuss which the best and new interesting experiments are or just work out an agreement between experiments that the Laboratories are doing indipendently? I am a bit afraid of too much planning. On the other side this is advantageous. I would think that a certain percentage of the work could be planned almost completely, and another percentage would be quite indipendent.

W.: I have just talked about that idea, but personally I am not favourable.

S.: Of course, also with complete indipendence there is a lot of exchange to standardize and normalize but in essence more a posteriori than a priori.

W.: Maybe there is somebody who wants to make a case for this committee?
Others: No.
W.: So we must not consider the superstate committee.
Bernardini: There are two kinds of experiments: one that implies something smart really very original; these are very few, and of course you have to keep a maximum of freedom.

There are other experiments, which are more or less standard and are to be done just because you have the machine and somewhat noblesse oblige, and then if you just go on this kind of experiments in only one Laboratory you will have one group doing the same experiment for four or five years and that would be extremely boring. This kind of experiments can be discussed, for istance in the same spirit as Columbia, Bologna, Pisa and Michigan Groups decided about the collaboration on the bubble chambers. For instance let's say that it's very important to know what the angular distribution of the pair production is in conjunction with your model of the second strong interacting state. Let say that one Laboratory could study one energy of emission, a second Laboratory another energy, and so on. These experiments at different energies furthermore automatically control each other. These experiments are not very few and we could get this massive work done very quickly. This would be very useful information and we would have the advantage of distributing work among various groups without boring one group with two or three years of work.
W.: I would like to make a concrete suggestion: I would hope that Cocconi convinced you that the way to look at the multiple production is to look in a cloud chamber (diffusion chamber with a magnetic field). For looking at the multiple production you need a thousand particles, and you have already taken a run of 250 particles.

It is conceivable that you shall build a similar machine here, but at beginning of the collaboration we could take pictures (our runs are of only a few days) and send the films here for the scanning. This would be the beginning of the cooperation; later on you will vary the conditions of the exposure, but you will have the same equipments and scanners, and you shall be in businnes.

In this way we could compare results and get twice the results. This is the kind of thing where you need group labor.
Salvini: This is an interesting suggestion. First because Cocconi 's experiment is giving that sort of information that you cannot get in any other way. The other is that the University of Genova has prepared a diffusion chamber at high pressure that I think is ready.
W.: At the beginning I don't think that there would be a difference in exposing here or in Cornell. The γ - rays would be the same. This matter could be discussed by a small group

of interested people.

I would think that a happy situation would be to have machines with various intensities. Our intensity is about 10^9 electrons per pulse, that seems to be about the same as at Caltech, but we have more pulses. If you get the figure of 10^{10}, then experiments would be opened to you that we can not even think about. In this case you will do the experiments with high intensity; we would specialize in experiments at medium intensity, and Caltech do experiments at low intensity.

Of course, if somebody get 10 times the intensity, in the other places they will sit around and try to get more intensity too.

Or, if you get your electron beam out, that would be the place to specialize on that. We certainly do not have any plan to get our electron beam out in the near future.

Salvini: Speaking of the electron beam extraction, it is actually too early for us to make any prevision. Some of us have made some calculations; in particular Dr. Diambrini and a calculation has been done on a new method proposed by Dr. Turrin.

Let me ask first Wilson, than I shall ask Tollestrup: Did you consider this problem in your Laboratories?

W.: We have considered the problem, and I feel that is quite possible to get the beam out of our machine.

There are two metods: First, you could get the beam out in a pulse. That is easy. We build a coil that would put the beam out.

S.: That would be a Stanford beam.

W.: Yes. I don't know if would be useful for us.

S.: There would be less in intensity by a factor 100 than Stanford and as fast or faster.

W.: Probably faster: 10^{-8} seconds. It may have some use. In a Laboratory where you build for some time, and you begin doing physics, you find that is very difficult to stop and to do some work on the machine.

There is another possibility, and that is to get the beam out on a resonance. In this way you can get the beam out in a long time and exploit the advantage of a Synchrotron. I don't think you have to consider anything else.

However, I think we have the straight sections; we have a beam that is few millimeters in diameter, and that is the place to do the experiments. You can hold the beam and you can get the Hydrogen there. You can get a factor of 100 - 1000 by letting the beam make multiple traversals.

That has been dose at Cornell: we have put a liquid Hydrogen target right in the beam of our machine (Edwards is doing this). Unfortunately the first time the target blew up and filled the whole dougnut with H_2. What we did do, was to put in an Aluminium foil, a very thin foil. We did see the beam go into the foil, many times, and there is an increase in intensity of many many times: I don't know yet of how ma ny times exactly. In other words if you can make 100 traversals, the intensity is 100 times higher. What we hope to do eventually is to shift the beam to the H_2 target by changing the frequency, and then leave the R.F. on so that the beam is multiply scattered. The damping is still quite strong, due to the emission of radiation, so that you can hold the beam down for some time. Anyway this thing makes us think about electron - proton scattering: if we just brought our beam out we couldn't think of it at all. Here we have a beam with perfect geometry, long times, all the advantages. So this place (in the doughnut) is the place to do experiments. I don't see any advantages in getting the beam out, and wouldn't worry about it.

S.: How big is the aluminum foil?

W.: Well, its diameter is about 1 cm: it has a very thin Ni coil holding it in.

S.: Will the indefinition of the thickness of the target be a problem?

W.: No problem. Because you can monitor the γ-rays, and that tells you how many electrons come through, since you know it's liquid Hydrogen.

S.: So the γ - rays will be the monitor of the electrons that went through.

W.: Yes.

S.: About the magnetic field around the target: do you use wires to correct for it?

W.: No, there 's no magnetic field in this region.

In these problems you are interested in large - angle scattering, but you can look for protons, electrons, coincidences with γ rays etc. I mean there are no limitations; it is all open at Cornell as it is here. You can have the electrons all come to one side while the protons only go forward. The electrons can go through the foil, and you can stand back and put a magnet that will measure them.

S.: For instance, do you tink that experiments of the kind Budini proposed yesterday could be considered?

W.: I don't see why not. For example the 2 electrons coming out could be perfectly well measured if the angles are large enough. I don't see any difficulty. There'll be problems, but I think it can be done.

S.: How about the back-ground?

W.: One can use shields of some sort, but I don't think it will prove necessary. Edwards has put counters looking in to this region and he found it reasonably clean.

S.: If I remember correctly, there has be somebody, maybe at Michigan, who tried something like you propose: what came out of it?

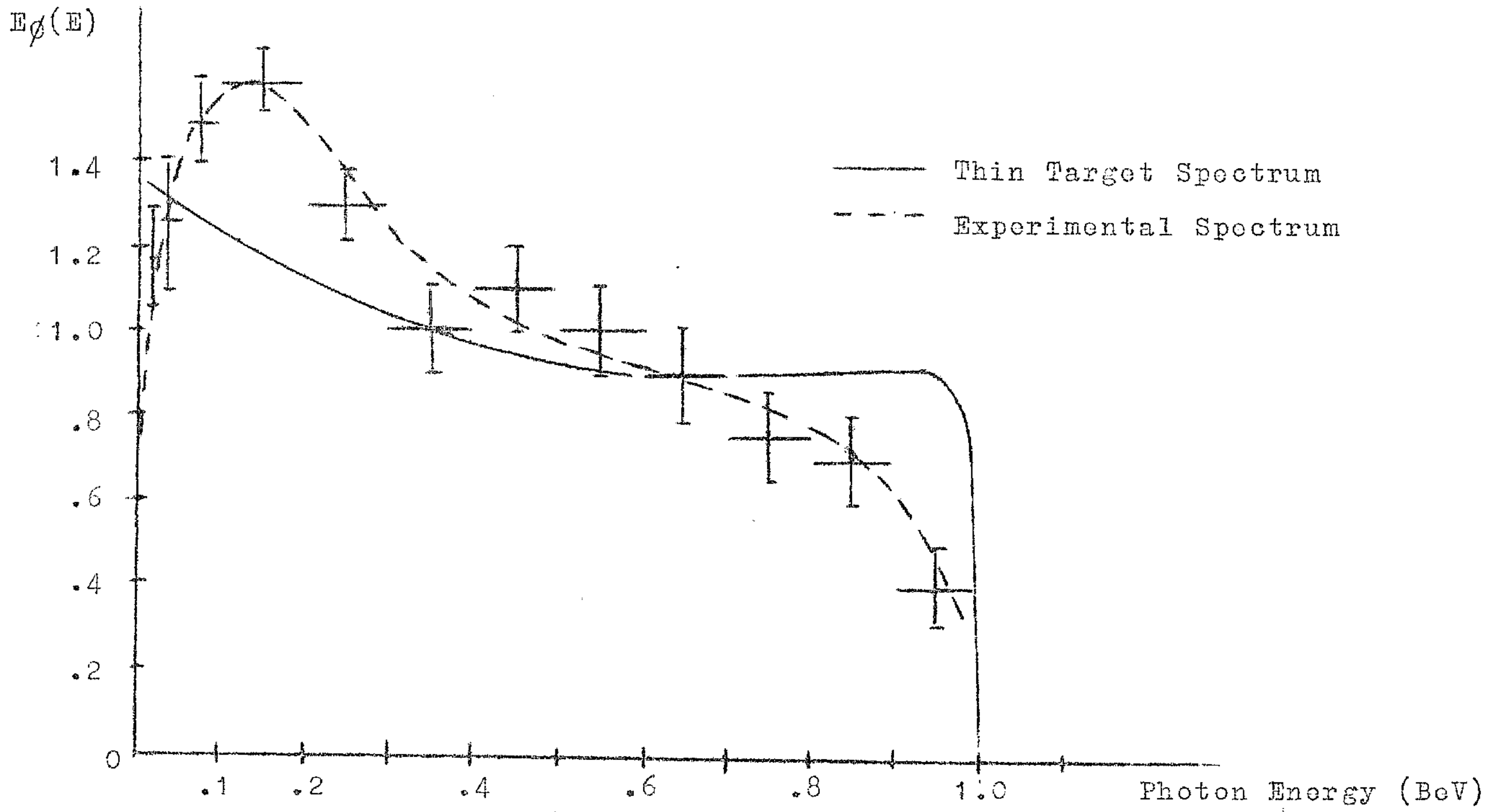
W.: I think a good result. They worked on electron scattering as I remember.

S.: Would you consider also electron - pion production?

W.: I don't see why not. I think that at present the most interesting experiments would be the γ - rays, but you wouldn't understand the electron - pion production: you want to do the γ - proton - pion experiments. The γ rays are the simplest, thus the obvious thing to do first. Later on you will want to do the electron - pion production, and you can do it in this way; I think it's the ideal way to do it too.

I expect we should use great care to keep our straight sections completely clear from everything: except for the doughnut you can walk every where.

In the future you can imagine a large scattering chamber there, a large magnet focussed there to measure all the electrons, protons or pions coming out.



BREMSSTRAHLUNG SPECTRUM AFTER 2.75 RADIATION LENGTHS OF LiH

E_{max} = 1 BeV

Elenco dei partecipanti

ALBERIGI
AMALDI
AMATI
BACHELET
BALATA
BARBARO
BARONI
BELLETTINI
BENEVENTANO
BERNARDINI C.
BERNARDINI G.
BÈRTANZA
BIZZARRI
BLOCH
BOLOGNA
BRUNELLI
BUDINI
CASTAGNOLI
CINI
COCCONI
CONFORTO
CONVERSI
CORAZZA
CORSON
CORTELLESSA
DIAMBRINI
DI CAPUA
EHRENBERG
FERRO-LUZZI
FINZI A.
FINZI R.
FRANZINI
GATTO
GHIGO
GIGLI
HABEL
INFANTE
JENTSCHKE
JONA-LASINIO
LADU
MANFREDINI
MARCONERO
MARINI
MASSAROTTI
MEZZETTI
MONETI
MONTELATICI
MORPURGO
MUCHNIK
MURTAS
OLIVI
PEDICINO
PELLEGRINI
PIZZELLA
PUGLISI
QUERCIA
QUERZOLI
REALE
RISPOLI
SACERDOTI
SALVINI
SANNA
SCHLIER
SIRCANA
TOLLESTRUP
TOSCHI
TOUSCHEK
TURRIN
WILSON