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THE SEARCH FOR Z^*'S

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THE SEARCH FOR Z^*'s

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At the 1962 CERN Conference Gell-Mann and Ne'eman were each considering the idea that the $\Delta(1238)$, the $Y^*(1385)$ and the $\Xi^*(1535)$ could belong to either a $\underline{10}$ or a $\underline{27}$ SU(3) group. The absence of any KN resonance phenomena of the magnitude of the then known resonances swayed them in favor of the $\underline{10}$ assignment.[1] This led to the prediction of the Ω^- whose exciting experimental confirmation is well known to all.

The possibility of the existence of KN resonances was raised again in 1966 by Cool et al. in a series of very beautiful precision K^+p and K^+d cross-section measurements at Brookhaven.[2] These measurements indicated a distinct bump in the K^+p cross section as well as in the $I = 0$ part of the K^+n cross section obtained after unfolding of the experimental K^+d cross section. If the resonance interpretation is adopted these would correspond to rather high inelasticity resonances: Z_1^* at 1910 MeV and Z_0^* at 1863 MeV. Confirmation of these measurements come shortly thereafter from work at the Rutherford Laboratory by Bugg et al.[3]

Since that time there has been a great effort on: inelastic cross-section measurements,[4] elastic K^+p scattering,[5] observation of further structure in total cross sections,[6] observation of steps in K^- production yields for γp reactions,[7] and more recently polarization measurements in K^+p scattering with polarized targets.[8] It is these later measurements which place more restrictions on phase shift analyses and limit the number of possible ambiguities, which eventually can lead to a unique solution and answers to the question of the existence of exotic Z^* resonances.

I will now report on three new papers submitted to this Conference and then summarize the present status on Z^*'s.

I. K^+p ELASTIC SCATTERING MEASUREMENTS FROM 1.4 TO 2.3 GeV/c

I'd like to report on the following results based on brand new data from University College, London, and Rutherford Laboratory (Barber et al.).[9] They have measured the K^+p elastic cross section between 1.4 and 2.3 GeV/c at 26 momenta at Nimrod with about 4,000 events at each momentum. The method is a Čerenkov counter and time of flight for particle identification and wire chambers for kinematics. Since this is new data there are only preliminary results available at present. They find that the Legendre polynomial expansions give a smooth variation of the coefficients with momentum. Figure 1 gives typical examples of the differential cross section.

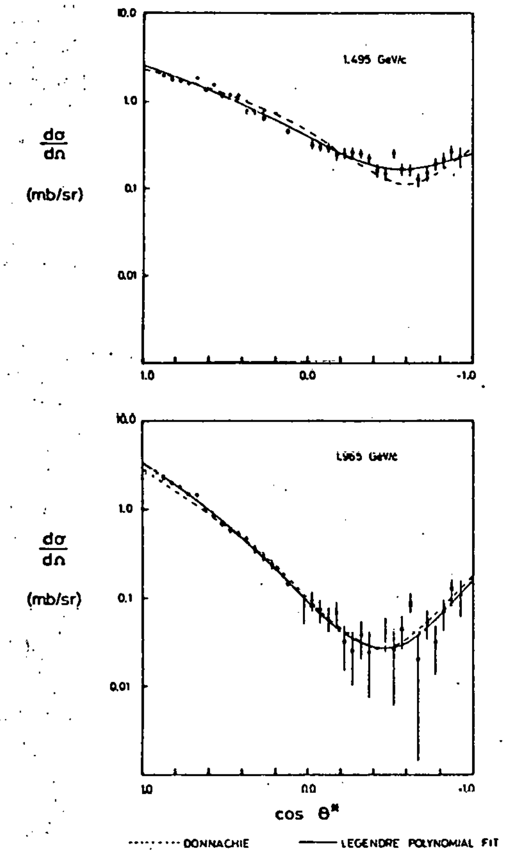

Fig. 1. Differential cross section sample of 4 out of 26 momenta studied by Barber et al.

Four out of the 26 momenta they have measured, 1.5, 1.7, 1.9, and 2.1 GeV/c roughly are shown. These distributions give you some idea of the quality of the data. The solid curves are Legendre polynomial fits. The dotted curves are fit according to a Regge pole model by Carreras and Donnachie[10] which was not calculated from these data, but from fitting all other existing data. This is the next communication I am going to mention. The Regge pole model was actually only fitted at momenta which are roughly equal to the ones given in Fig. 1. In between it does not fit so well, but it does fit where there had been earlier data at 1.5 and 1.96 GeV/c. Figure 2 shows the normalized Legendre coefficients A_1 to A_4 between 1.4 and 2.3 GeV/c. These are fairly smooth all the way; this is essentially true over the entire interval studied so far. At the higher momenta even higher coefficients than those shown as an example are needed. The authors have performed a phase shift analysis but they want to emphasize that it is very preliminary, because their data is so recent.

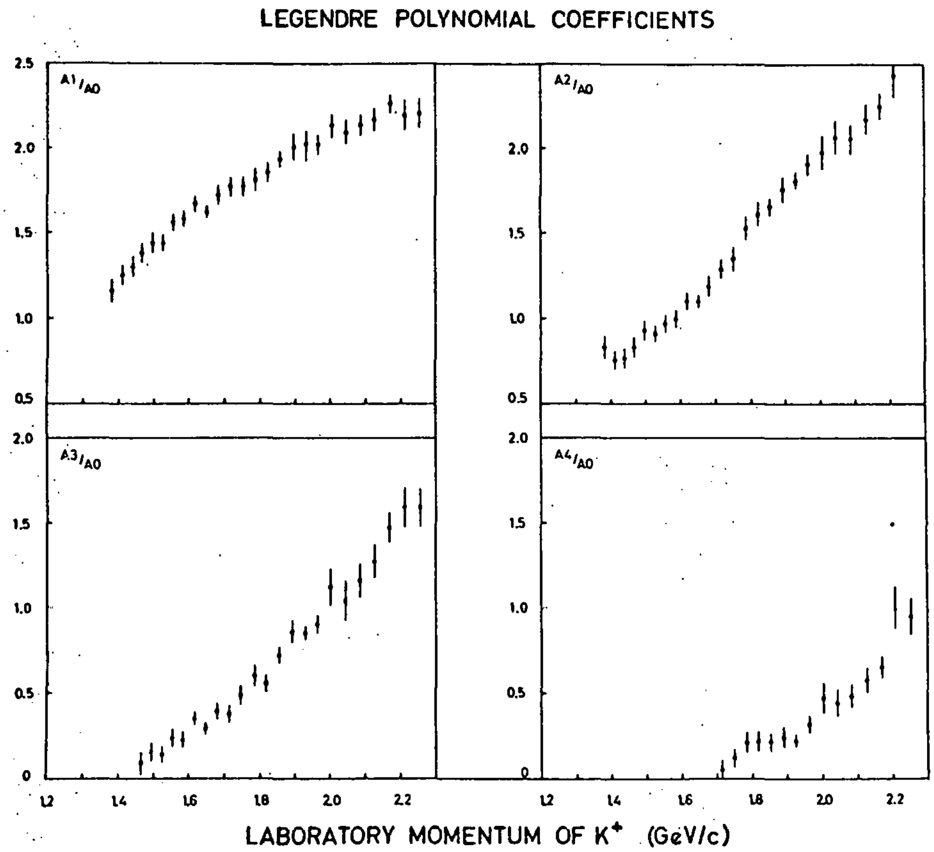

Fig. 2. Fit to data of Barber et al.

Figure 3 shows the results. This phase shift analysis was tied onto the Lee, Martin, Oades[11] solution up to 0.8 GeV/c, but there isn't much ambiguity up to 0.8 GeV. Beyond that it is an independent search at each momentum using starting values close to existing solutions. It is however not a complete random search. They find essentially that the main waves (S_{11}, P_{11} and P_{13}) have a tendency to go

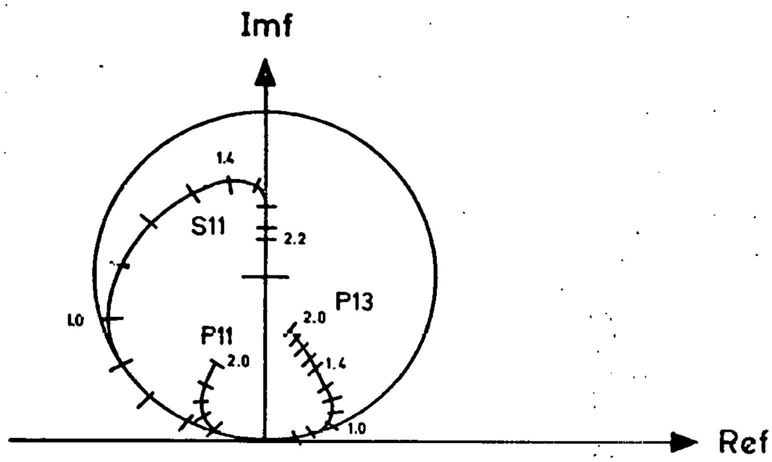

Fig. 3. Phase shift analysis by Barber et al.

towards the center of the Argand plot; namely the negative waves have the tendency that when they reach a certain phase they stay constant in phase and the inelasticity increases. This is true for the S_{11}, for the P_{11} and also for the positive phase shift the P_{13} which is so relevant to all of our discussion today.

II. A REGGE POLE ANALYSIS

I now want to go on to another contribution to this Conference. This is a Regge pole model by Carreras and Donnachie from Daresbury and also Kirsopp[12] who has done a phase shift analysis from the Regge pole model. In this Regge pole model they have attempted something different. Rather than trying the phase shift fits which other people have attempted, they try to fit all data above 0.7 GeV. That even includes data up to 12 GeV (whatever was available in the high energy region). They have also included all of the polarization data (including that at high energy). They carried out a Regge expansion; they mentioned a specific form, I won't go into the details, but its a parameterization which Das has used before. Furthermore, they allowed specifically for $K\Delta$ and K^*N thresholds.

They get χ^2 on this large amount of data of 1750 for 1000 or so degrees of freedom. By and large it looks like a reasonable fit. There are three basic results. The first is surprising and needs to be looked at carefully. This is that they get a positive S wave instead of a negative S wave. I will mention later the previous basis for the negative S wave. They get a negative P_{11} wave, as is the case for all the other solutions we have seen. Finally, they get a positive P_{13} wave. Figure 4 shows the positive S wave solution, in the Argand plot, you note that they have actually two solutions. They show both of them because though they prefer one, they cannot rule out the other. In both the S wave has a positive circle which

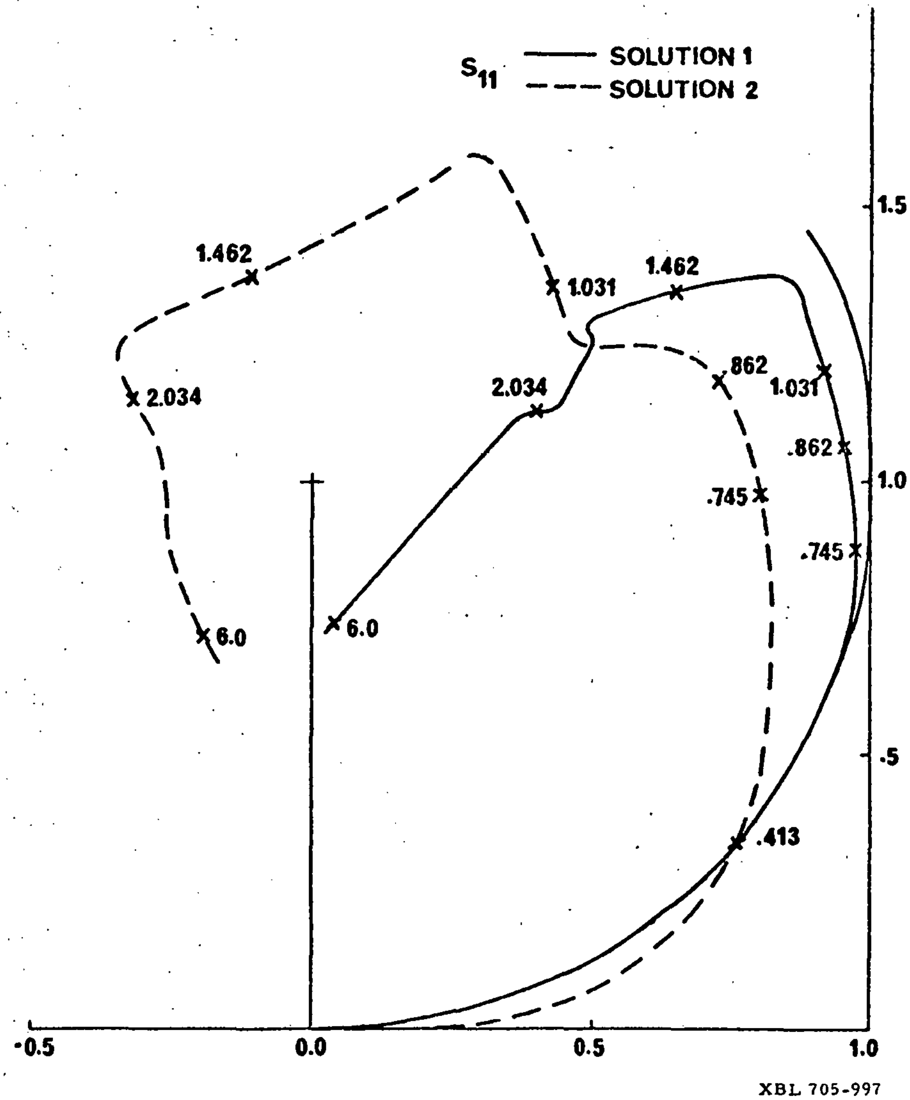

Fig. 4. The positive S wave solutions obtained from the Regge pole analysis of Carreras, Donnachie, and Kirsopp.

varies very slowly with momentum; it should be noted that the curves go up to 6 GeV. Figure 5 gives the P_{11} solutions. As I said it looks as though it has the same sign as all other solutions. Figure 6 gives the P_{13} solution which again is positive; however, the authors say that it has no resonant features. Now consider the

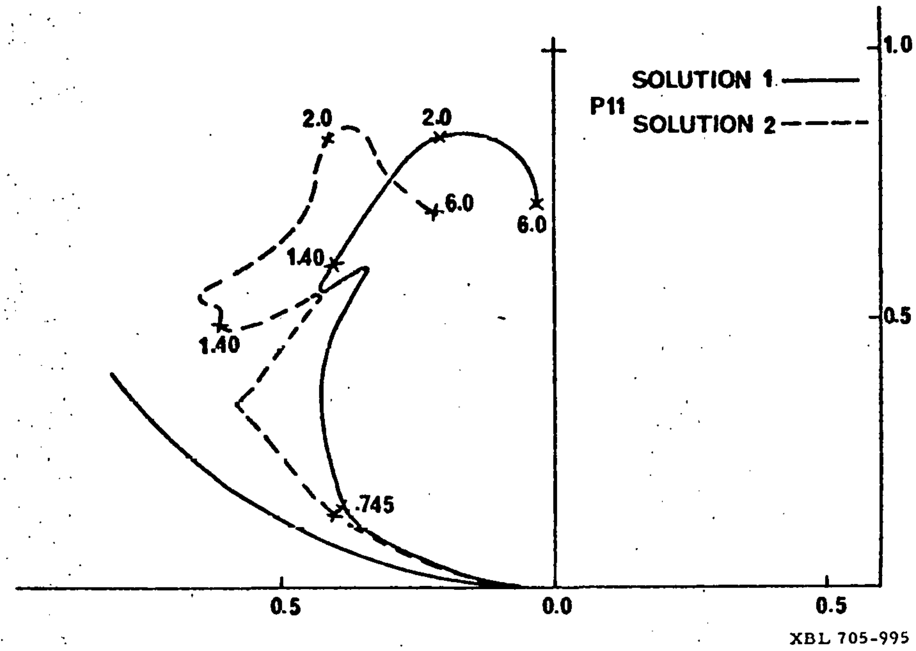

Fig. 5. The corresponding P_{11} solutions.

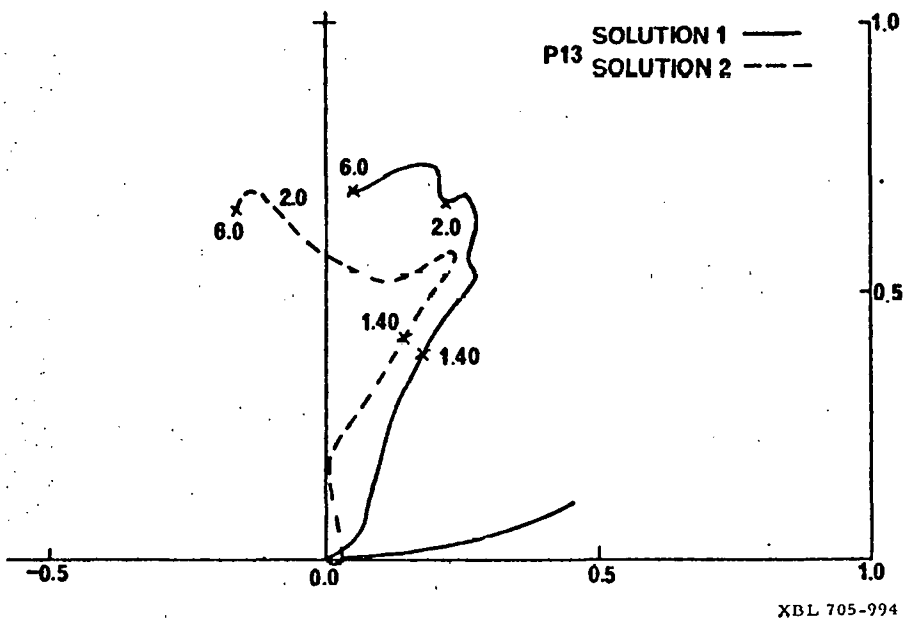

Fig. 6. The corresponding P_{13} solutions.

question of the negative versus the positive S wave. When we got this paper last week, we went back to our phase shift program and tried it for two momenta at 0.86 and 0.96 GeV/c with a positive instead of a negative S-wave phase shift. The answer is that either S wave fits well. Taking any given momentum you can fit with the positive S wave just as well as with a negative S wave.

A. The Old Evidence for the Sign of the S Wave Phase Shift

The reason for which most people use the negative S wave comes from an experiment which we have done nearly 10 years ago which showed interference between the elastic scattering and Coulomb scattering and which displayed constructive interference; namely, that the S wave is repulsive, just as the K^+p Coulomb interaction is repulsive. Now, in order better to answer the question about the sign of the S wave, I will show you some of our very old data. Figure 7 is from the experiment by Sula Goldhaber and co-workers, a collaborative experiment between LRL and UCLA. Figure 7 shows the elastic scattering from 140 MeV/c which was our lowest energy point up to 640 MeV/c.[13] Here the solid curve is a fit with a negative S wave while the dashed curve is a fit with a positive S wave. You see very clearly there are several standard deviations difference at 140 MeV/c between these fits. This data was divided into eight separate momenta and essentially for each one of them these features are observed. The positive S-wave solution is not shown at each momentum however it behaves similar to the 140 MeV/c data. By 640 MeV/c it becomes difficult to distinguish with the available data, but there are six momenta in between which all show the feature of constructive interference. Now the point which Carreras and Donnachie make is to say that if they take these data and add all other results and then compare with their solution the few points showing the interference make little difference in the overall chi square and this effect is lost. This is perfectly true but it means ignoring a very clear systematic effect! While I feel that the negative S wave is actually pretty firmly established, I would like to urge other experimenters to think about checking the result. At the moment it is based entirely on one single experiment carried out ten years ago!

Figure 8 is again from this old experiment just to show you the argument as to why we are dealing with an S wave at low momenta. If you have a flat distribution it can be $S_{1/2}$ or it can be $P_{1/2}$ or it can be a mixture of $P_{1/2}$ and $P_{3/2}$. However, only the S wave is going to give you a linear dependence between the phase shift and the momentum. The P wave would give you a p^3 dependence which is very different from the observed result in Fig. 8. Again the point which is used as the basis for all the dispersion relation calculations (which Dr. Wagner mentioned) coming out with a negative sign for the real part of the scattering amplitude also uses as an input the negative scattering length which is obtained from these data. If indeed there were a positive S wave at low energy, quite a number of relations would have to change very drastically.

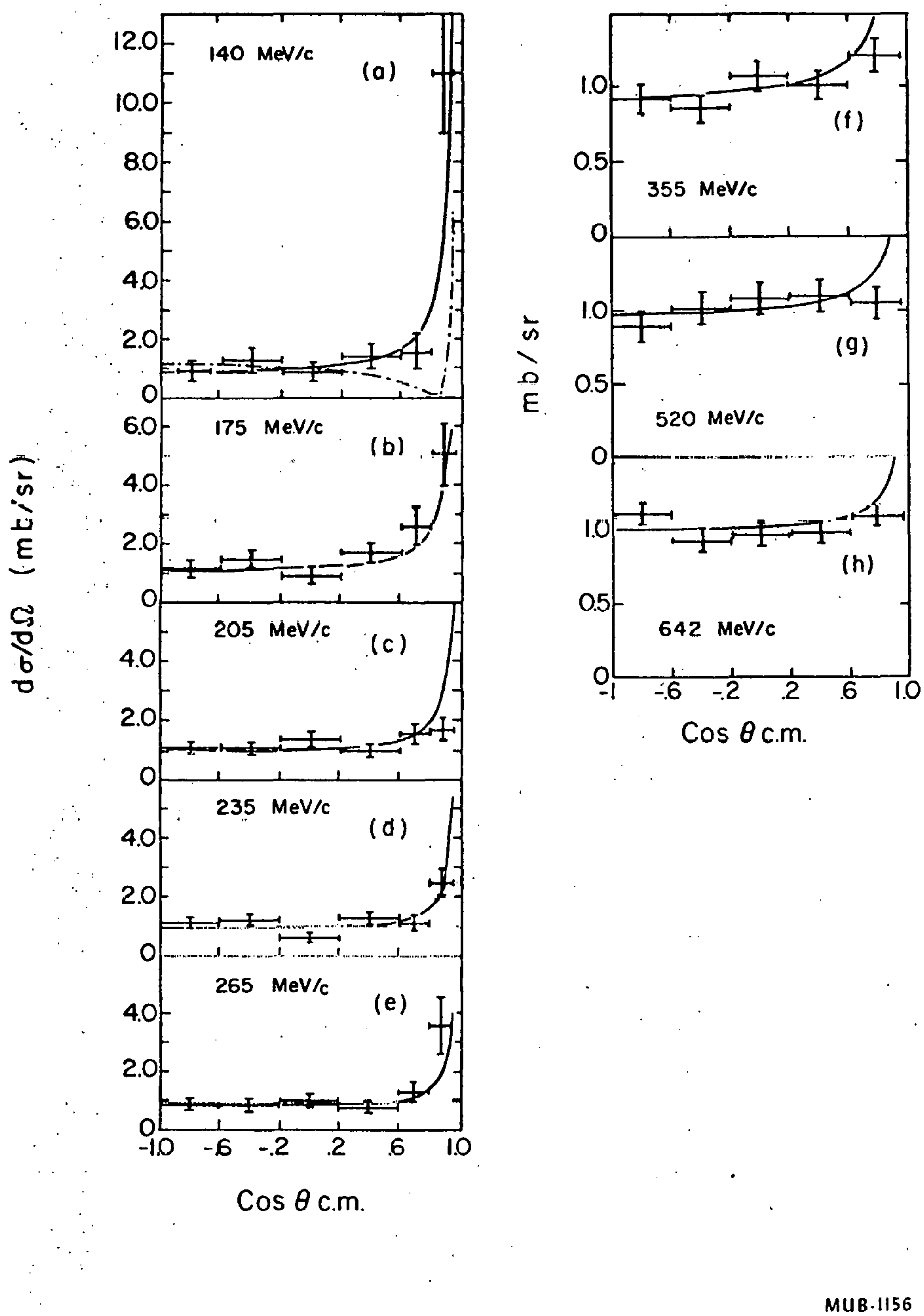

Fig. 7. The low energy differential cross section data of S. Goldhaber et al. giving the evidence for a negative S wave phase shift.

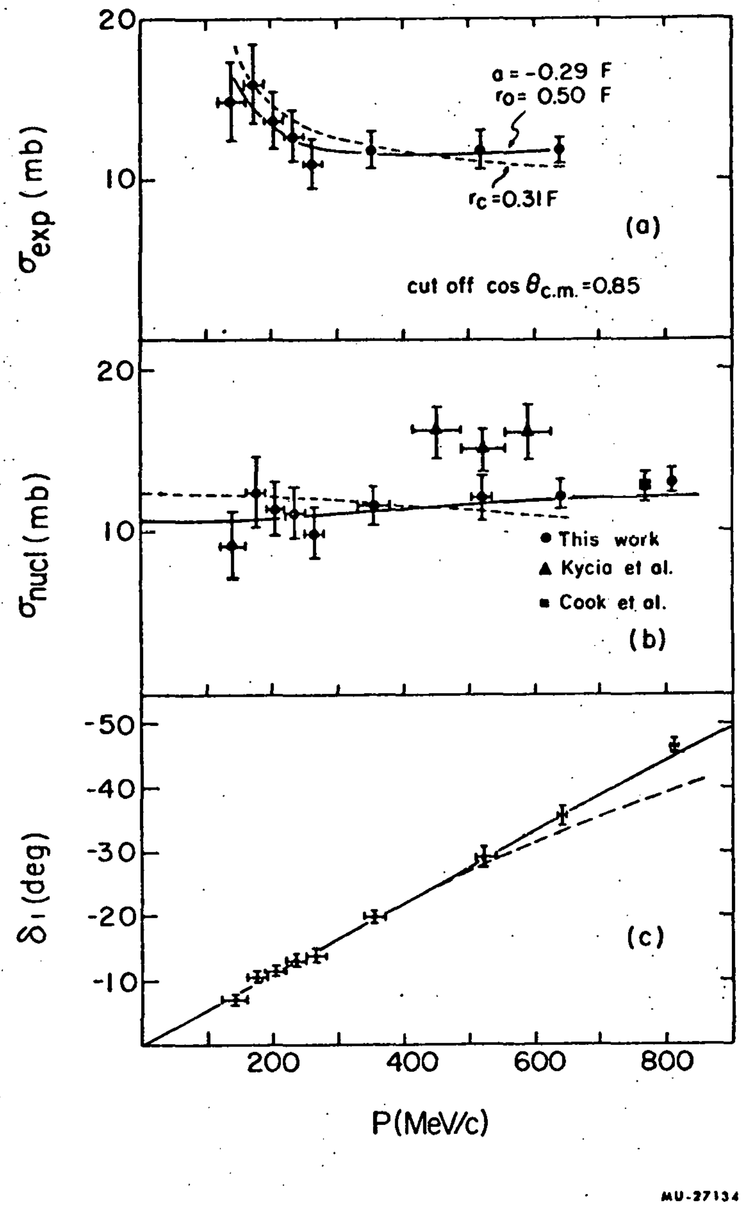

Fig. 8. Cross section and phase shift for the low energy data showing the characteristic S wave-linear momentum dependence and value for the scattering length.

III. K^* PRODUCTION FROM THE $I = 0$ K^*N STATE

Now I want to come to a third contribution. This is from our group, Hirata et al.,[14] and it is new data on the deuterium experiment. These experiments are fairly complicated so although the exposures have been taken some time ago we are only now completing the analysis of the deuterium data. This is in the momentum interval

860-1580 MeV/c which covers the region of the Cool bump. Now in this particular result I am going to show you the data on K^* production specifically. There we can isolate the K^* production in the $I = 0$ state. From isospin configurations you get the following results:

$$\sigma_o(KN \rightarrow K\pi N) = 3[\sigma(K^+n \rightarrow K^o\pi^+n) + \sigma(K^+n \rightarrow K^+\pi^-p) - \sigma(K^+p \rightarrow K^+\pi^o p)] \quad . \tag{1}$$

This is true for whatever happens between the K-nucleon and π. In most of our data all of these three cross sections are available so one can just take this algebraic sum and obtain the $I = 0$ cross section. For the data at the highest momentum 1580 MeV/c the last cross section is not available to us, and one can use an alternate formula where you specifically say that you are considering K^* production:

$$\sigma_o(KN \rightarrow K^*N) = 3[\sigma(K^+n \rightarrow K^{*+}n \rightarrow K^o\pi^+n) + \sigma(K^+n \rightarrow K^{*o}p \rightarrow K^+\pi^-p) - \frac{1}{2}\sigma(K^+p \rightarrow K^{*+}p \rightarrow K^o\pi^+p)] \quad . \tag{2}$$

The main difference is that for the last term one can use the cross section $K^+p \rightarrow K^{*+}p$ with $K^* \rightarrow K^o\pi^+$. In Fig. 9 we show the algebraic sum of these three cross sections at 1210 MeV/c as an example. Here the first cross section, $K^+\pi^-$, as you see, has large K^* production; in the $K^o\pi^+$ cross section you can see a K^* as well. The $K^+\pi^o$ cross section also has a K^*. Now we add the first two and subtract the third which yields the final K^* distribution. We can see that this method works, because when we look at the π nucleon mass distributions there is some Δ present. But on subtraction the final mass distribution shows essentially no more Δ, as expected for an $I = 0$ state, which proves that we eliminated the $I = 1$ part. Figure 10 shows what the angular distributions do. Again we add the first two angular distributions and subtract the third one and that gives the resultant angular distribution for $I = 0$. Here θ is the K^* production angle in the overall center of mass, α is the decay angle of the K^* (Jackson angle), and φ is the Treiman-Yang angle of the K^* decay both in the $K\pi$ center of mass. In the communication by Hirata et al.[14] we show these distributions for three momenta 1200, 1360, and 1585 MeV/c. What is relevant to our discussion today is the question of the angular momentum state of K^* production. You see that the production angle is extremely sharp even at 1200 MeV/c. We must remember that 1200 MeV/c is just slightly above the K^* threshold (K^* threshold is at 1080 MeV/c or so). As soon as we can produce it, the K^* is strongly forward peaked which means that in terms of s channel K^*N production there are many angular momentum states involved. An analysis requires up to $\ell = 4$. I won't go into the details, only enough to say that it looks like the K^* is produced by pion exchange in this process even near threshold. Furthermore, there is a great similarity between the $I = 1$ K^* production cross section and the $I = 0$ K^* production cross section. We show that if you put a curve through the $I = 1$ K^* production cross section the same curve scaled up by a factor of 2.5 fits rather well to the $I = 0$ K^* production cross section. The physical significance of this is still under study.

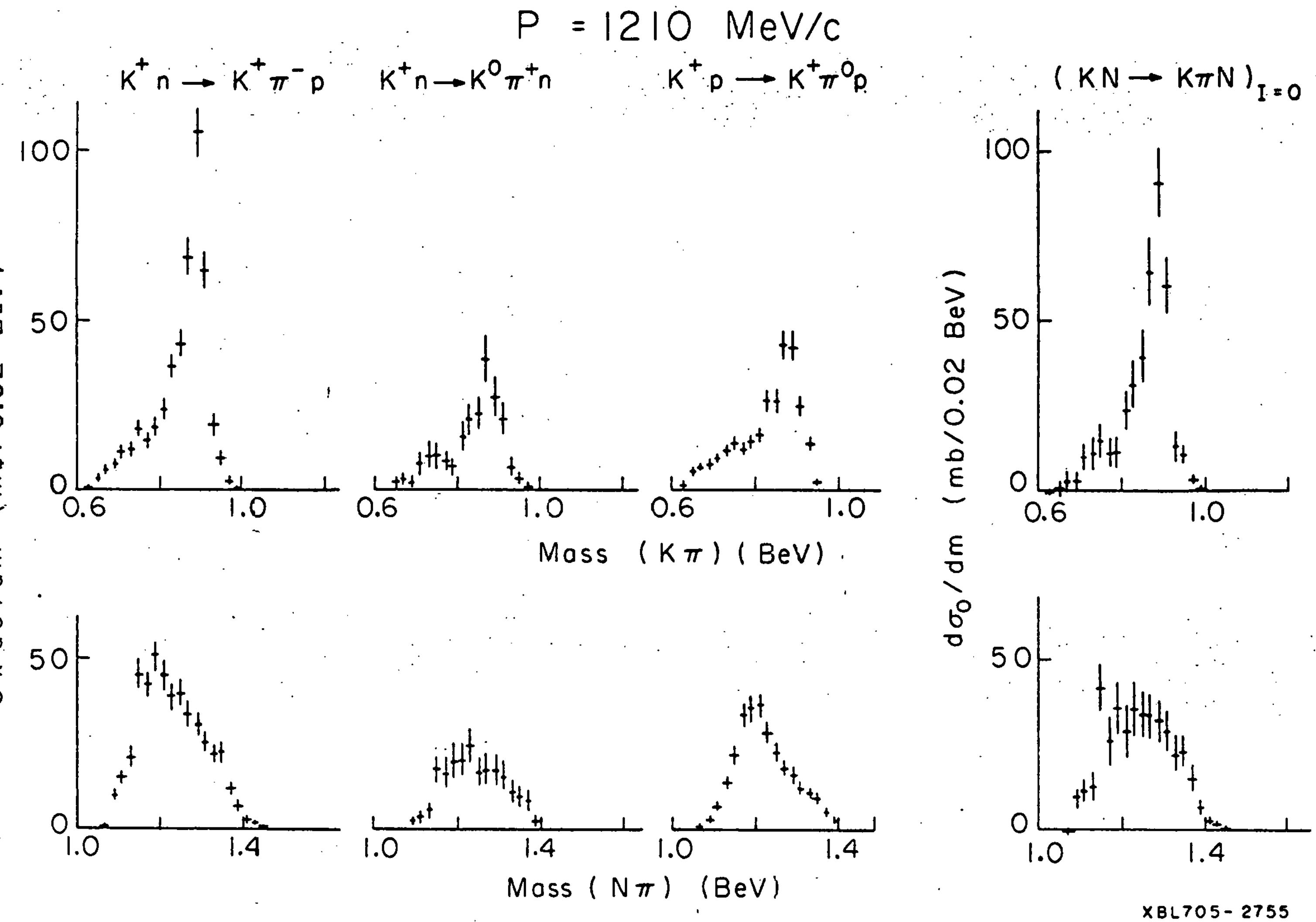

Fig. 9. Data of Hirata et al. The I=0 K^* production evaluated according to Eq. (1) from the algebraic sum of 3 cross sections.

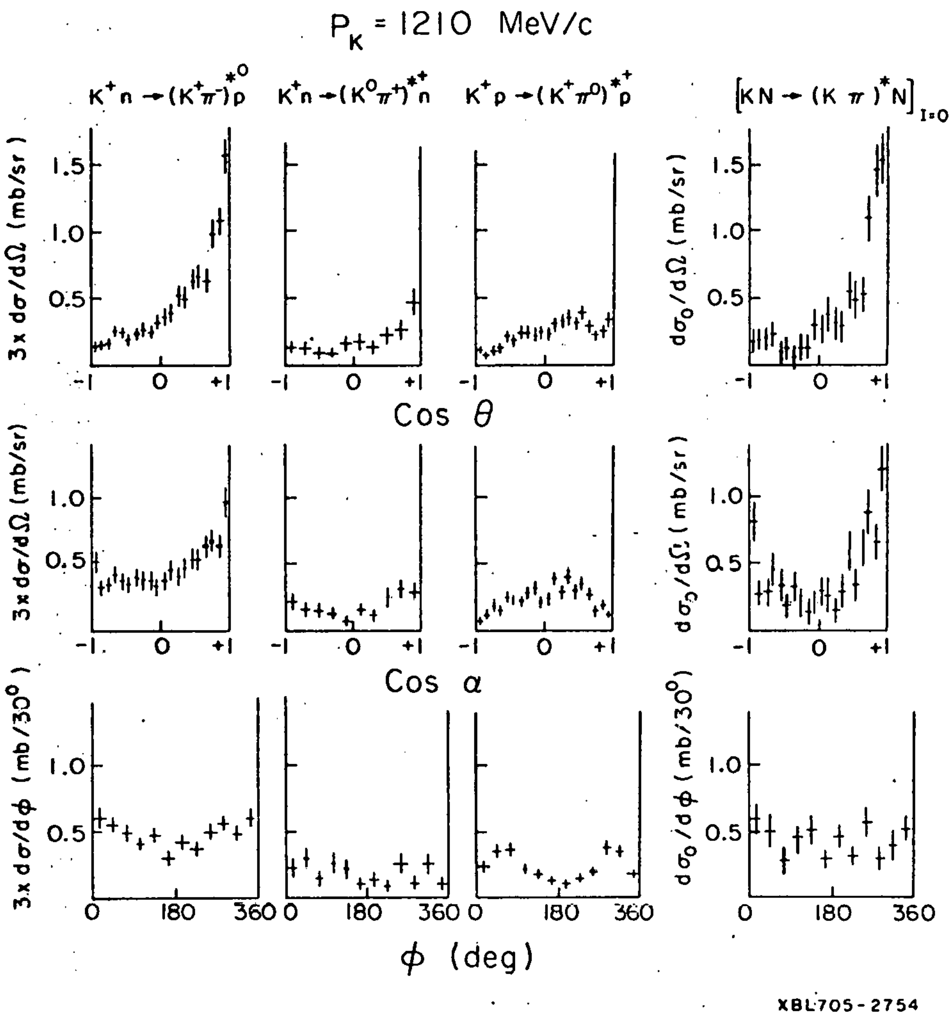

Fig. 10. Construction of the corresponding angular distributions.

IV. THE QUESTION ABOUT THE EXISTENCE OF A Z_0^*

In 1968 we showed our data for the $I = 0$ inelastic cross section (Hirata et al.[15]; see Fig. 11). If we compare this with the $I = 0$ total cross section deduced at that time by Carter from a linear combination of the results of Cool et al. and Bugg et al. which were available then (the Jenkins data was not yet available) then we found that subtracting the inelastic from the total you get a rather sharply falling elastic cross section in the $I = 0$ state. There is one other interesting point which was known from earlier work in collaboration with Stenger et al.[16] at UCLA and that is that the $I = 0$ scattering length at zero energy is very small; essentially it was deduced to be 0.04±0.04 Fermis. This means that the S wave cross section at zero momentum must be dropping down very rapidly.

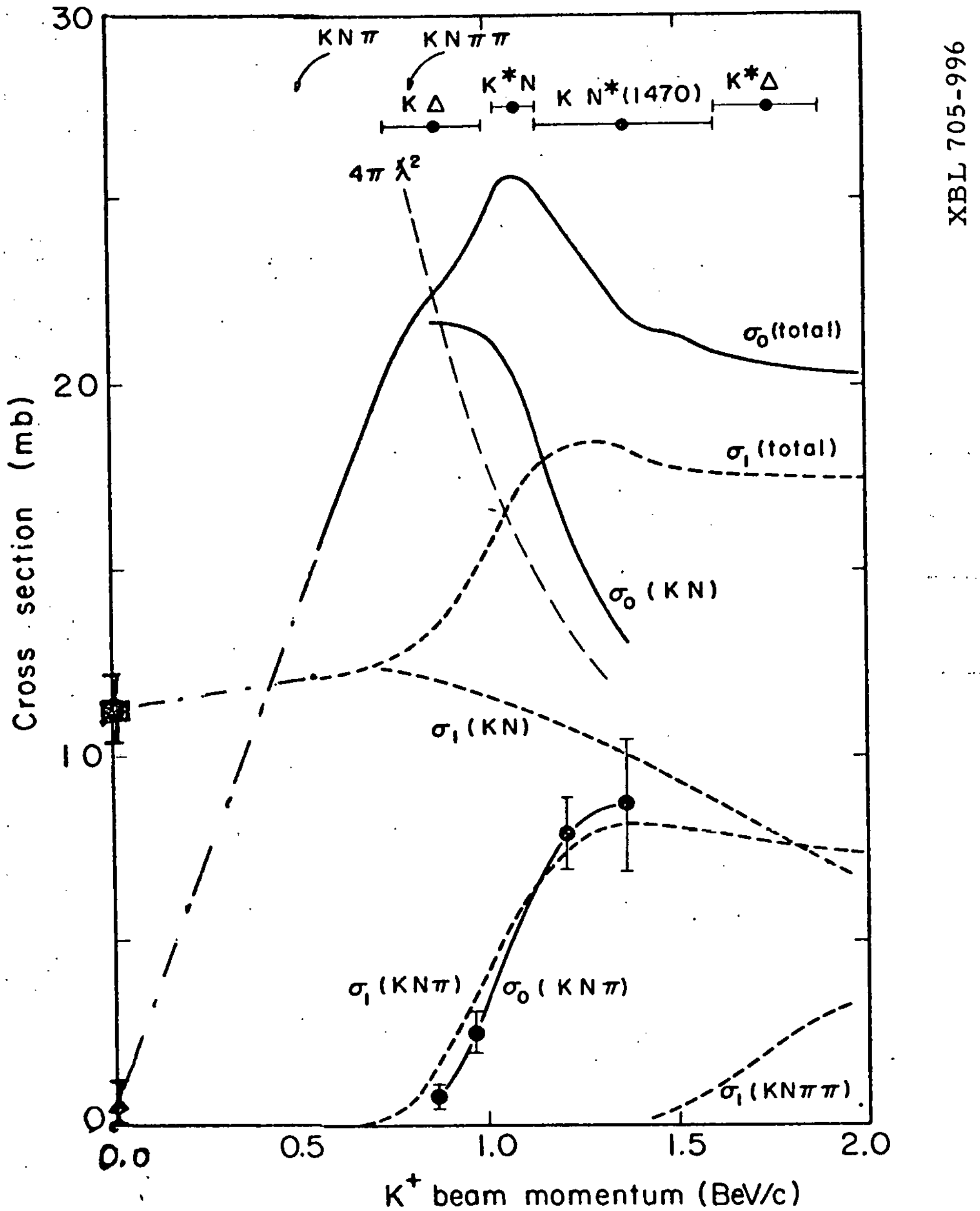

Fig. 11. The I=0 and I=1 total inelastic and elastic cross sections. The I=0 cross section was obtained by Carter as an interpolation between the data of Cool et al. and Bugg et al. This curve is shown extrapolated to zero energy to the scattering length value deduced by Stenger et al. The I=1 cross section is similarly extrapolated to the scattering length value of S. Goldhaber et al.

We pointed this feature out, and point out a distinction between the $I = 0$ and $I = 1$ cross sections: the $I = 0$ cross section drops off rather sharply, while the $I = 1$ cross section, which comes from the data I showed earlier remains more or less constant at threshold. The threshold limits for $I = 0$ and $I = 1$ are deduced as ≈ 0.2 mb and ≈ 11 mb respectively. This, of course, means that for the $I = 0$

state there occurs a rather rapid rise essentially from 0 up to some high value and then the elastic cross section drops off again. In fact with the numbers we had then the elastic cross section rises to $\approx 4\pi\lambda^2$. The Brookhaven group (Abrams and co-workers[17]) analyzed this same data and made the point that this rapid rise together with the fall of the elastic cross section is a very striking feature and could be a Z_0^*. Figure 12 shows the most recent analysis by the Brookhaven group which I obtained from Dr. Cool (it includes the results of the University of Arizona experiment at the Bevatron by Jenkins et al.). This figure was already shown by Dr. Cool in his talk. There is at present some confusion as to exactly how the

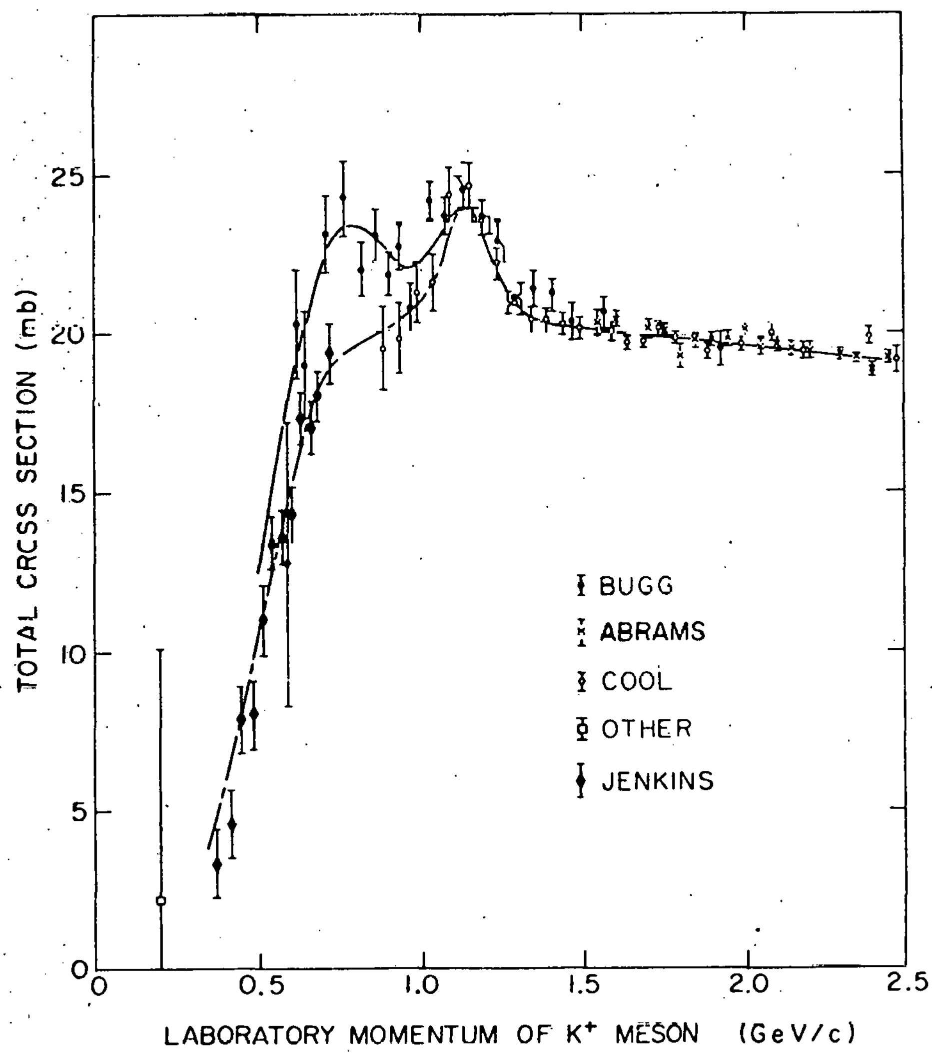

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Fig. 12. The I=0 total cross section evaluated by Cool et al. including the new data by Jenkins et al.

curve goes through the points and whether we are dealing with one or two peaks, but there is no doubt that there is a rise from zero through a peak near $4\pi\lambda^2$ followed by a drop off of the elastic cross section. Figure 13 gives two possible alternative interpretations of the I = 0 elastic cross section depending on just how you treat

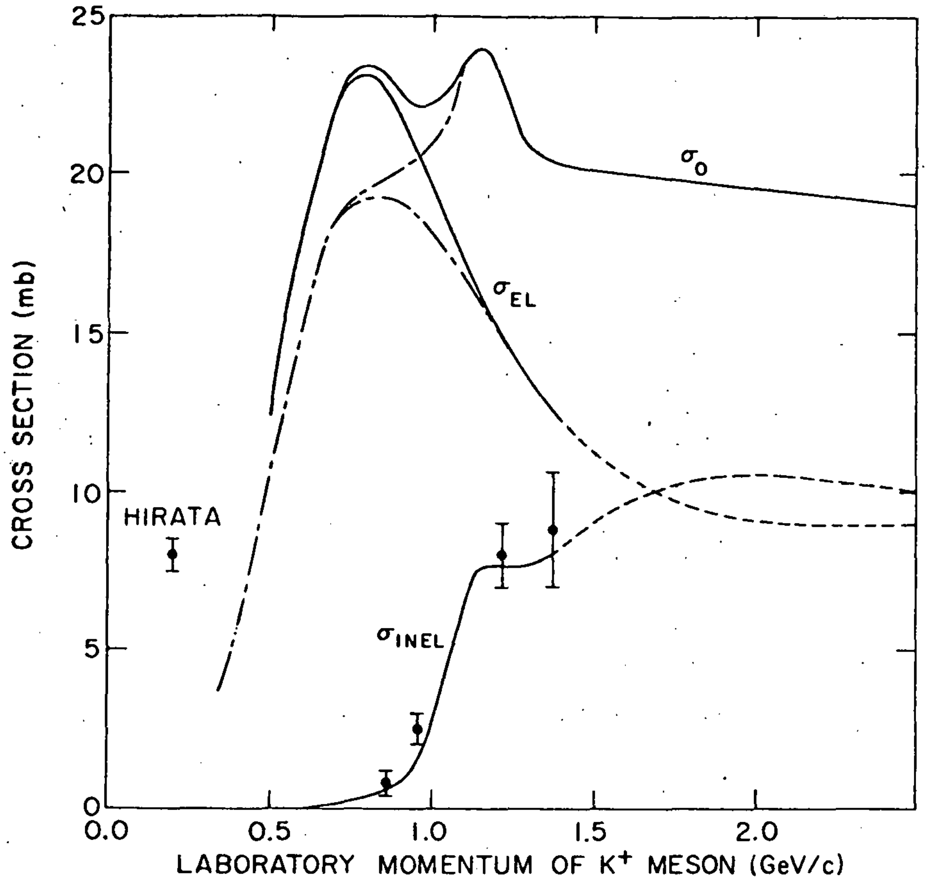

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Fig. 13. The resulting I=0 elastic cross section showing 2 possible interpretations of the data indicative of the current uncertainties in the 0.8-1.2 GeV/c region.

the data in the unfolding procedure and Dr. Lynch discussed this in his talk at this Conference. I won't go into this, but one of these curves should be right and for the present a decision between them is not that critical. Finally, Fig. 14 is a contribution by Dowell[18] to the Conference which again emphasizes the features of the Jenkins and Bugg data; he independently points out that this very large I = 0 peak occurs. I think the main difference in the analyses are the approximately 3 millibarns difference in how high the peak goes and the subsequent question as to whether this should be considered as two structures or a single structure.

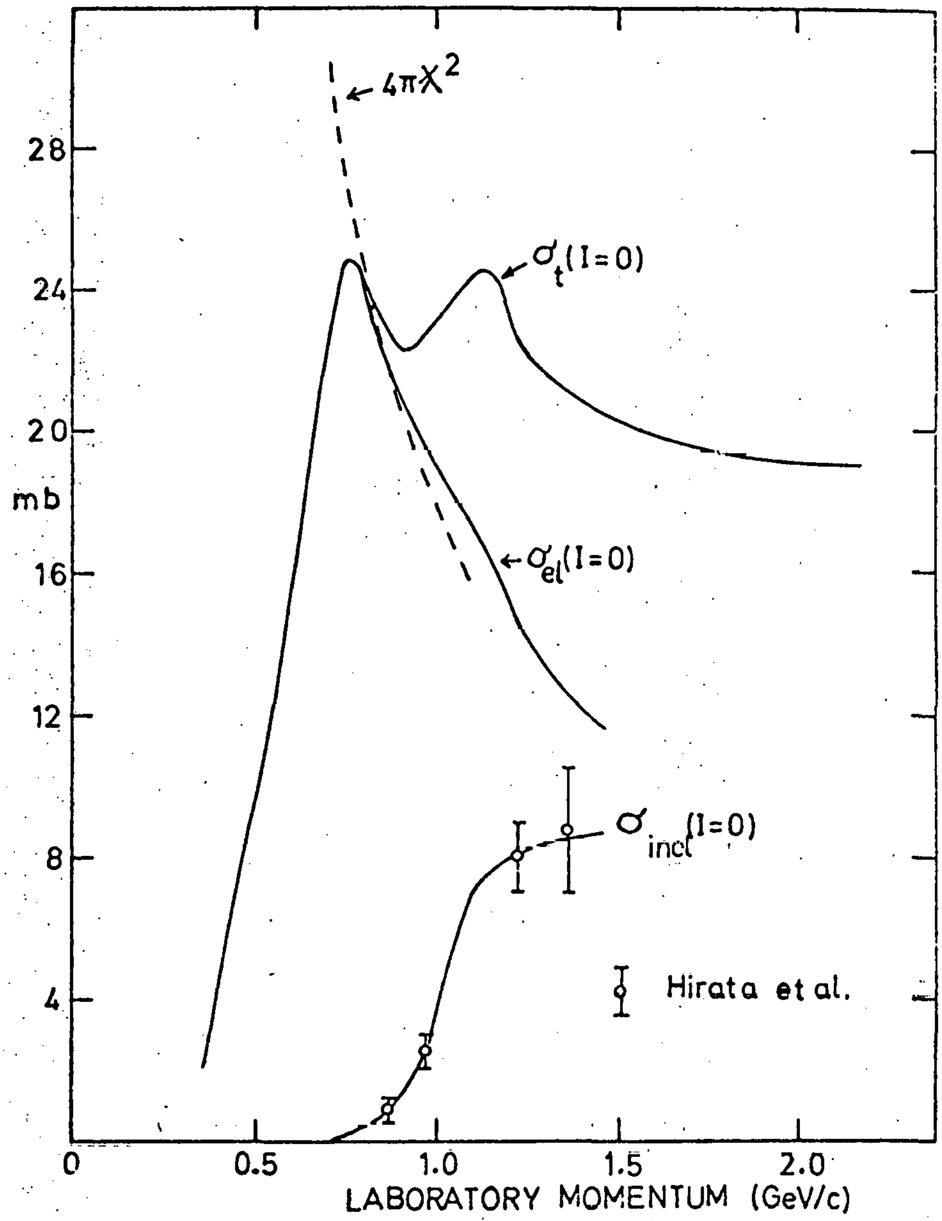

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Fig. 14. Similar results deduced by J. Dowell.

V. SUMMARY ON EXISTENCE OF Z^*'S

This leads to the final point, what can we say as a summary to all the discussion we have heard?

(1) Is there a Z^* ($I = 1$) at the Cool bump? I think what we have learned is that if the P_{13} curve is indeed a resonant circle on the Argand diagram then the answer is yes, there is a resonance there and its spin and parity would be $3/2^+$ with a mass somewhere in the region of 1900 MeV and width not yet determined. There is however the other possibility that there is a phase shift solution which is not resonant, namely that all solutions are heading for the center of the Argand diagram which would be the onset of diffraction processes. I feel that at the moment both possibilities still exist.

(2) The second question is does the Z_0^* exist and the answer is: what we know at the moment is that there is a large peak in the elastic cross section, $M(KN) \cong 1700$ MeV, $\Gamma \cong 500$ MeV; however, we need a phase shift analysis in order to be sure that this is a resonance. It has been suggested as a resonance by Abrams et al.[17] and also by John Dowell.[18] My feeling is that we will have to wait until we can do a phase shift analysis in the $I = 0$ system to be sure. We have done some phase shift analysis at lower momenta[16] and they show that the one wave which is large and positive is the P_1. This solution is favored by polarization measurements on the charge exchange reaction $K^+d \rightarrow K^o pp$.[19] If there is a resonance, it probably will be a $J = 1/2^+$ (a P_1 state).

Now why are we so cagey on this question since we have very readily accepted many, many resonances in the Y^*'s? The reason is really related to the point of view that the Z^* is a different animal from the Y^*; namely, in terms of the quark model in the case of the Z^* we need more than the usual qqq structure characteristic of a Y^*. Here we are dealing with an exotic structure such as $qqqq\bar{q}$. We could ask if three quarks bind why not five? But then we have to ask if five why not seven, nine, and finally why not 137? The main point to realize is that the Z^* is a completely different structure. Although we have established the qqq structure, there is a need to establish this new one as well. That is why one has to be more rigorous in looking at all these questions than if we were just examining one additional Y^*. Now what I would like to pose as a question to the theorists here is the following: we do know that there is a positive P_{13} phase shift. Thus we are dealing with an attractive force. The question I have is, in what sense is the world different (a) if you have just a positive force which does not lead to resonance or (b) you have a positive force which is strong enough to lead to a resonance? In other words, what other features of our world would be different for these two cases. The positive force seems to be there in any case. The phenomenon has the qualitative features of an attractive force but what we are arguing about is the question of whether there is actually a resonance.

A second question is what should one do experimentally to really pin this down completely. We had a discussion between the members of the group giving the various talks in this session today. We thought that if very precise differential cross section and polarization measurements could be made at one intermediate momentum at least it would help in removing ambiguous solutions. Secondly, if this exotic resonance exists, and assuming the SU(3) picture applies here, this is only one out of 27; one should look for the other 26! Thirdly, there is the K^-p backwards scattering experiment which I believe is being done by the Stony Brook group at Brookhaven and also by various groups at CERN. If you have K^--proton backward scattering, the exchange particle would have to be a Z_1^*. As far as I know at present most differential cross section measurements either have only a limit in the backward direction for K^-p scattering or it is more or less flat. If one could measure $d\sigma/du$ as a function of u (the very backward scattering) and if one really observes the distinct characteristics of an exchange particle at sufficiently high energy so that one is out of the s channel resonance regions, I think that would be a very excellent demonstration of Z^* exchange. So far, to my knowledge, none of the exotic resonances has been observed in exchange processes at high energy.

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