LEARNING ABOUT MESON STATES FROM THEIR PRODUCTION PROPERTIES*

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ABSTRACT

An attempt is made to increase our awareness of the value of including information on production mechanisms in our study of hadron resonances and meson-meson scattering.

My purpose in this talk is to try to convince you to include information from the production data when you are studying meson states. Many of you already are doing that, and others (particularly G. Fox) have urged you to do it, but I think we still take little advantage of the production data compared to what is possible.

At this conference we are trying to learn about meson states and meson-meson scattering, particularly $\pi\pi$ and $K\pi$ scattering. In the past 3-4 years there has been a lot of work done in this field. It is interesting to ask how much we have learned. Two things come to mind: (1) there has been fairly detailed verification that we have some understanding of small t exchange. Most people probably agree now that $d\sigma(\pi N\to\pi\pi N)/dt$ has a zero not at $t=0$ but at a t value about $2m_\pi^4/m_{\pi\pi}^2$ below t_{min}. Even this is a point some theorists were confident of several years ago, but it needed experimental verification, most extensively provided by the SLAC experiment. (2) The rapid variation of the $\pi\pi$ scattering moments at the $K\bar{K}$ threshold has essentially given us the s-wave $\pi\pi$ phase below the f.

Apart from these results most work has just added details to what we already understood (in view of some comments since my talk, it may be worth adding that one should not count as things we have learned things which some individual may have learned or worked out but which were well known to other people or in review talks much earlier).

To learn more, in this field, basically there are two possibilities. First, one could find dramatic effects such as the $K\bar{K}$ threshold; generally we will learn a lot from them because they are dramatic and dominant

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(not because they exist -- we always knew the threshold was there, but the size of the effect is what taught us a lot). Second, one could learn to do better phenomenology with the data and extract more physics information.

To elaborate on the second point, note for example that even in the case of the $K\bar{K}$ threshold one must make a coupled channel model and a fairly sophisticated analysis to get physics out. One is still not able to decide whether there is an s-wave pole in the amplitude below the S^* in mass.

One can make a stronger statement: without a model one cannot in practice learn physics from data. In principle this is not true, but in the real world it is almost completely true. For example, you have heard today several examples of clever amplitude analyses. Even in the best of these, for πN scattering, one only learns the amplitudes up to an overall phase, and one must have additional theoretical input to learn interesting physics about the amplitudes. Similarly, the useful analysis of Estebrooks and Martin[1] assumes that three of the six amplitudes for production of a ρ are zero; this is a good approximation and constitutes a sensible model at this stage. It will be changed if data can be obtained on a polarized target.

Basically, the situation is as in politics. All behavior, even no action, is political. Similarly, there are bad models and there are good models, but never model independence. If you choose to use a bad model to get information from your data, then ...

EXAMPLES OF WAYS TO LEARN FROM PRODUCTION MECHANISMS

(A) Almost model independent

There are a number of ways one can put information about production into the analyses, either to learn more or to gain confidence in what one has already extracted. Here I will mention a few examples. The reader can easily think of more that are relevant to his particular data or interest. (1) In charge exchange production of a pion pair one expects to exchange at high energies only π and A_2. These couple to nucleons mainly by flipping helicity. Thus as long as one is summing over nucleon helicities it is probably a good approximation to neglect all amplitudes involving nucleon non-flip. Then there are four complex amplitudes left if s- and p-wave pairs are produced. One can measure six quantities in this case, so with one further assumption one can determine the remaining amplitudes up to an overall phase. This set of assumptions constitutes a well defined model.

This has been done in most detail by Estebrooks and Martin, as has been discussed at this conference. Whether one can make enough safe assumptions in a given problem to get at important information has to be considered in each case. In the present case of pion pair production the situation will be different when the reaction is measured on a polarized target, as then the nucleon non-flip will show up (hopefully due to A_2 coupling) and we will learn about the relative phases of the amplitudes with flip and non-flip couplings.

For any reaction one can make the necessary number of assumptions and then determine the remaining quantities.

(2) The $\pi\pi$ phase shifts (or anything) must be the same whatever production reaction it is measured in, at whatever energy, if we are in fact correctly extracting them from the production reaction. Near the rho mass that is of course true for the dominant p-wave, but for several small quantities of interest such as the I=2 phase shifts, the s-wave at the kaon mass, the scattering lengths, one should assign a systematic error given by the range in values from different experiments in addition to the usual errors. As an example, the SLAC group[11] has compared the phase shifts from the reactions

$$\pi N \to (\pi\pi) N$$
$$\to (\pi\pi) \Delta^{++}$$
$$\to (\pi\pi) \Delta^{\circ}$$

They find they can use the same $\pi\pi$ phases if they have some nonflip amplitude at the nucleon vertex and some flip amplitude at the NΔ vertex. For the latter case for the π exchange contribution alone one can work out that the ratio of the sum of flip couplings to the non-flip coupling is given approximately by

$$-t\left[\sqrt{3}\left((m_\Delta+m_N)/m_N\right)^2 + \left((m_\Delta+m_N)/m_N\right)^2\right] \approx -20t$$

so that at $-t=.05$ GeV^2 one might expect about equal flip and nonflip contributions, which is about what they find. A more careful analysis needs to be done, but qualitatively this gives us some confidence in the resulting $\pi\pi$ phases.

Similar studies should be made for the $K\pi$ phase shifts in KN reactions with N and Δ recoil.

(3) There are some physical region effects which should go away at the π exchange pole after an extrapolation, such as ρ-ω interference in the mass spectrum, or a φ peak in $m(K\bar{K})$ in $\pi N \to K\bar{K}N$ since $\varphi \not\to 2\pi$.

(4) Production data can provide a very important check on "daughter" states, i.e. on situations where it appears one is observing one state under another, such as the familiar s-wave under the f. If we are producing two resonances of spin S and S-2 (I will concentrate on the f and s-wave example, where S=2) the production amplitude has the general form

$$M \sim M_f(s,t)P_S(\cos\theta) + M_\varepsilon(s,t)P_{S-2}(\cos\theta)$$

where the $M_x(s,t)$ describe the production and I have only written the amplitude for helicity zero for simplicity; the symbol "ε" represents the state of spin S-2. If it should happen that the two production amplitudes $M_f(s,t)$ and $M_\varepsilon(s,t)$ (which in general are unrelated) occur in the ratio (S-1)/S at all s and t values, then the production amplitude would be proportional to $\cos\theta P_{S-1}(\cos\theta)$ by the Legendre function recursion relation, and one would get a pure $\cos^4\theta$ angular distribution in the f region as has sometimes been observed. If one is really observing an s-wave under the f then by going varying s or t so natural parity exchange gets important (natural parity exchange cannot produce an s-wave pion pair) one should see the P_2 angular distribution come back.

An experiment[2] at 13 GeV/c has observed somewhat more bump in the middle than most lower energy experiments, which is encouraging, since the A_2 should get more important relative to the π as the energy increases. To be sure one is seeing an s-wave under the f, one should systematically vary s and t and watch the distribution change from $\cos^4\theta$ to $P_2^2(\cos\theta)$.

In general when one has interfering resonances one can separate them out by varying the production conditions. This is probably the main place where production information should be extensively used to study resonance properties. It has been neglected, partially because people don't often go outside their own data; even in one experiment, however, the t dependence of the decay angular distributions can be used to separate interfering resonances.

(b) More model dependent examples

We have heard extensive discussions from Field and Matthews in this session about the nature of the scattering amplitudes for various reactions. The point of all this is that the reaction you are thinking about at the moment is not the only one in the world, and moreover the amplitudes being measured in any given reaction have usually been at least partially measured in some other reaction already. Thus information can be used from the other experiments to learn more from those currently under study.

For example, there are many places where π and A_2 exchange occur. From $np \to pn$ one can see that even at t=0 the A_2 exchange is important; the same thing should be true for $\pi N \to \rho N$ in the appropriate amplitudes, which are those with ρ helicity ± 1.

For a long time to come, however, all physics we learn by comparing reactions will be somewhat model dependent. If you want your results to be correct, you must use a model which is sufficiently correct to avoid misleading results.

The situation is somewhat like the case with phase shift analyses. Beyond a certain partial wave one must set all phase shifts somehow. It used to be that all higher partial waves were set to zero; that is a model although not a very good one. For ten years people have been trying to do better by using our knowledge of the long range forces to fix the higher partial waves, and now I think it is clear that the results for the lower phase shifts are better physically than they were when the higher partial waves were put to zero.

Similarly here we do not yet have long established models for calculating all amplitudes. But we have learned a lot in the past five years and now I think it is possible to be confident of a number of aspects of the situation. The basic useful assumption is the old conjecture of the Michigan group, which seems to be approximately correct, that

> FOR A GIVEN EXCHANGE (e.g. $\pi, \rho, \ldots$) AND A GIVEN n,x (these are helicity flip quantum numbers, defined below) AN s-CHANNEL HELICITY AMPLITUDE IS APPROXIMATELY THE SAME FUNCTION OF s AND t IN EVERY REACTION WHERE IT OCCURS.

For a reaction $a+b\to c+d$, with particle a having helicity λ_a, etc., one can label all the amplitudes by the helicities. Then n and x measure the amount of helicity flip and are defined by

$$n = |(\lambda_c-\lambda_a) - (\lambda_d-\lambda_b)|, \qquad n+x = |\lambda_c-\lambda_a| + |\lambda_d-\lambda_b|.$$

The above conjecture is that instead of a new amplitude for every set of helicities, all the amplitudes with a given n,x in a given reaction for a given exchange are the same, and even those for different exchanges or different reactions are approximately or qualitatively the same. At a detailed level this is now known not to be true (e.g. vector exchange amplitudes are more peripheral than tensor ones) but in the small t region where most data are the conjecture is approximately true, and it holds well for magnitudes over a larger range.

One important confirmation of this hypothesis is the apparent validity of our prediction[3] that the n=1 π exchange amplitude has a (complex) zero near $-t=0.6$ GeV^2. As you have heard in Matthews' talk, the prediction is basically satisfied. (The situation is not completely clear yet, however, since the higher energy CERN-Munich experiment does not see the dip (but in a detailed model the dip will move out[4] in -t with energy and calculations[5] may be consistent with the CERN-Munich results), and since the effect appears to be cleaner in the non-charge exchange reactions rather than the charge exchange ones, contrary to naive expectations.) Thus all known n=1 amplitudes are consistent with having a dip as in the conjecture. However, the position depends on the exchange and the reaction somewhat, with the short range tensor exchanges (e.g. A_2) having the dip further out in -t with a range about 0.7 times that for the vector exchanges.

We have heard about another result at this meeting which means our conjecture above can only be approximate. Namely, both Martin and Ochs have discovered that in the CERN-Munich data the difference between the full n=0,x=2 amplitude as $t \to 0$ and the pion pole is a decreasing function of the pion pair mass. Writing $M(n=0,x=2) = t/(t-m_\pi^2) - C$, they find that C decreases from near one at the rho mass to near $\frac{1}{2}$ at the f mass. With that definition, C is made up of about 1/3 A_2 exchange and about 2/3 absorption of the π in $\pi N \to \rho N$. It seems likely that part of the effect they have found is a decrease in the strength of the coupling of the A_2 to pion pairs of increasing mass; indeed, such an effect has been predicted by Hoyer, Roberts and Roy[6]. The rest of the effect will mainly arise from a decrease in the total cross section of the pion pair-nucleon system. To get precise numbers a detailed calculation is needed, but simple estimates suggest that one should predict that $\sigma_T(fN) \approx 15$ mb. It will be very interesting to see if measurements on nuclei can give such a result. A small part of the decrease should come from a decrease in the sum over non-elastic intermediate states because of the increased change in mass, but this should not be more than about 10%.

At this point I could take the amplitudes from our detailed analysis[7] of $np \to pn$ and give detailed predictions for the polarization measurements in ρ and K^* production, because the same s-channel helicity amplitudes are involved. Since the polarization measurements will not be done for some time and there will be detailed predictions available[5,8] I will restrict myself to only using the NN analysis as a guide to make some remarks on two topics of interest here.

PHASE COHERENCE

It has often been assumed that the three amplitudes for producing a ρ with nucleon helicity flip (four counting the s-wave production) have zero relative phase. The analysis[1] of Estebrooks and Martin disagrees with this, and so does the absorption model or the lessons of the $np \to pn$ analysis assuming the s-channel helicity amplitudes have a common structure. The basic point is very simple.

At small t the amplitudes with net helicity flip n>0 feel little absorption whatever the model, so if the pion pole is mainly real they are mainly real. But the amplitude with n=0,x=2 has a pion pole that is mainly real and which is cancelled at some t value (exactly where is model dependent, with any -t value in the range 0.02-0.05 GeV^2 being reasonable) so that that amplitude is purely imaginary at that t value. Thus at a point near $-t = m_\pi^2$ one amplitude is purely imaginary and the others are mainly real, with almost complete phase incoherence. If it should turn out that phase coherence held for $\pi N \to \rho N$ and not for $np \to pn$ it would have important implications not only for ρ production but for our entire view of particle reactions.

THE WILLIAMS' MODEL

In the past few years the Williams' model[9] has been exceedingly valuable in the study of pion pair production. It has been very effective in increasing the utilization of proper extrapolation techniques and in increasing our insight into the details of pion pair production.

However, there are situations where continued use of the Williams' model will get us into trouble. I suggest that it is time to go beyond the approximations of the Williams' model to a more realistic treatment. To repeat what I said above, the results one gets out of the data will only be as valid as the model used to get them.

Some of the shortcomings of the Williams' model are

-- Its amplitudes are coherent in phase; see the previous section.

-- It allows one to fit pion pair production data out to -t=0.15 or so with no other contributions. But there are many indications[10] that considerable A_2 exchange must be present there. Indeed, if the $np \to pn$ analysis[7] is a good guide the A_2 is important even at t=0 in the amplitudes with $\lambda_\rho = \pm 1$, perhaps as much as 1/3 of the full contribution.

-- More theoretically, it is not really an absorption model (in spite of what it is called) because it does not remove partial waves in a smooth way but instead artificially simulates the effects of absorption. When one has reached the level of looking at extensive data in detail and of needing to consider interferences with other exchanges then it may be very important to be as realistic as possible.

SUMMARY

I would like to emphasize the following points.

(1) If you want to stay in the business of meson-meson scattering, and you want to learn something, then

either (a) find new effects of unexpected importance, or (b) use production information and theoretical models much more.

(2) Your experiment or model is not the only one in the world.

(3) Always publish normalized $d\sigma/dt$ and s-channel density matrices, as well as anything else you want. Then other physicists can utilize the production data in trying to understand what is going on.

(4) All assumptions made to extract physics from raw data are models. Some are more correct and more useful than others.

(5) There is a good possibility that s-channel helicity amplitudes are simple and approximately common to many reactions. There are only a few kinds of s-channel helicity amplitudes in the world, and much may be learned about your reaction by studying the amplitudes it has in common with data from other sources.

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