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MASTER

THEORETICAL ASPECTS OF THE NUCLEON-NUCLEON WORKSHOP

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Résumé - Ce rapport est centré sur l'étude des voies inélastiques dans la diffusion N-N entre 300 et 1500 MeV. Les sujets couverts comprennent la manifestation de la structure en quarks, les dibaryons, la dépendance en modèle des inélasticités N-N prédites. Nous présentons une comparaison entre les résultats donnés par les modèles conventionnels et un ensemble de données expérimentales en rapide expansion. Nous concluons qu'il n'y a, à présent, aucune évidence nette de la présence de résonances dibaryoniques non conventionnelles dans le système N-N aux énergies intermédiaires. Nous présentons aussi quelques remarques concernant d'une part une contribution théorique à la diffusion élastique et, d'autre part, des résultats expérimentaux nouveaux pour la photodésintégration du deuteron et d'échange de charge dans le système pion-nucléon.

Abstract - This report concentrates on the inelastic NN system from 300 to 1500 MeV. Topics covered include the visibility of quark signals, dibaryons, the model dependence of predicted NN inelasticities, and a review of how well present conventional models compare with a rapidly expanding database. The general conclusion is that there is so far no clear evidence in the NN system at intermediate energies for unconventional dibaryon resonances. Short remarks are also made concerning one theoretical contribution on elastic scattering and on new experimental results for deuteron photo-disintegration and pion-nucleon charge exchange.

I - INTRODUCTION

Catherine LeLuc has just told you about some of the matters discussed at the Workshop on NN Scattering at Intermediate Energies held a week ago last Wednesday. These included antiproton-nucleon reactions and the database for elastic scattering, comparing especially the less-well-known np situation with pp scattering. She also reviewed the experimental programs underway at various laboratories active in this energy range and then the status of elastic phase shift analyses.

My job today is to cover all the rest, which largely means the theoretical aspects of the Workshop. There was only one contribution on elastic NN scattering, and I discuss that first. I then will say a few words about two experimental subjects which were discussed, photo-disintegration of deuterons and pion-nucleon charge exchange. The bulk of my talk, however, addresses the inelastic region from 300 to 1500 MeV, where single-pion production is the dominant inelasticity. This is the region of the controversial (non-strange) dibaryon resonances. Besides discussing dibaryons, I will comment on model-dependence in theoretical predictions of inelasticities in high-L partial waves. That will be followed by reviewing present-day conventional models, comparing them with the rapidly expanding database in this energy region.

II - ELASTIC SCATTERING

Various ambiguities and peculiarities of elastic NN amplitudes were discussed by A. Gersten in a very compact ten minute presentation. He first explained his "zero method" for generating distinct phase shift solutions (given a cutoff angular momen-

tum L_{\max}). This leads to a set of discrete ambiguities, many of which can be eliminated for reasons of continuity and unitarity. Given an incomplete set of experiments, such as σ , A_{NO} , A_{NN} , D_{NN} , and K_{NN} , these ambiguities can nonetheless plague a phase shift analysis. The method is probably more useful, he suggests, for analysis of reactions like $p\bar{p}$ into two pseudoscalar mesons than for elastic NN scattering.

Gersten also described his "no exchange model" for NN elastic scattering. By this he means a pion exchange model but without any pions being exchanged when the two nucleons are closer than, say, 1.1 fm in configuration space. In this way, it is possible to provide a very good description of the experimental double-helicity-flip amplitude $\Phi_2 = \langle ++ | T | -- \rangle$ for pp scattering from 100 to 500 MeV. The predictions of this model are similar to, but a considerable improvement upon, the "poor man's absorption model", in which powers of t in numerators are simply set equal to μ^2 . This all very interesting but, as Gersten emphasized, it leaves us with the mystery as to why no other mesons seem to be involved in the dynamics of the Φ_2 amplitude.

In the discussion period, M. Moravcsik reminded us of his recently published work with G. Goldstein in which the resolution of discrete ambiguities is shown to require measurements of spins along three independent directions.

III - SOME RECENT, RELATED EXPERIMENTS

Somewhat isolated from other subjects at the workshop was the discussion by W. Meyer of photodisintegration of the deuteron at intermediate energies. A new experiment, done at Bonn, has measured this process using a vector-polarized deuteron target with a bremsstrahlung beam at 550 ± 50 MeV. The asymmetry in the pn final state was measured at 13 angles over the whole CM angular range and shows a rise through zero to positive values of about +.3 at the most backward angle. Meyer compared this data with three theoretical curves: a calculation done at Bonn based on a few leading conventional Feynman diagrams, another conventional calculation by a Tokyo group, and the same Tokyo calculation with added dibaryon terms (fitted to reproduce the old Kamae data on recoil proton polarization /1/, one of the first experiments claimed to show dibaryon resonances). Meyer concluded, "All analyses fail, with or without dibaryons", although to my eye the Bonn curves doesn't do so badly. In view of the skimpy nature of the theoretical models so far applied to this process, however, it is probably dangerous to draw any conclusions. Theoretical work is needed here.

Another photodisintegration experiment, involving a polarized photon beam on an unpolarized deuterium target and measuring the polarization of the recoil neutron (1), was reported by a Yerevan group headed by G. Vartapetyan. This experiment at 300 to 500 MeV, the first such double-spin measurement in this field, is still in a preliminary stage of analysis. No attempt was made to compare with model predictions, and, indeed, it would be surprising if any such predictions have yet been made.

There has been one new development in πN physics, generally a slowly changing field at intermediate energies. B. Nefkens described new results of πp charge exchange using a polarized target. Four incident momenta were studied: 300, 470, 586 and 625 MeV/c. The lower-momentum results for the polarization asymmetry agree well with the Karlsruhe-Helsinki and CMU-LBL phase shift analysis predictions. The data at higher momenta, however, disagree strongly. Moreover, the triangle inequality for the polarization (it applies to A_N as well as to differential cross sections) is "on the edge" of showing an isospin symmetry violation for the higher energies. With these data, one has to wonder (again) if there may not be something funny about the Roper resonance in the P_{11} πN partial wave.

IV - CAN ONE SEE QUARK SIGNALS AT INTERMEDIATE ENERGIES ?

I turn now to the main consideration in this report, nucleon-nucleon reactions above the inelastic threshold. The basic question to be faced is whether a conventional description suffices to describe the experimental situation. Here, and below, a "conventional model" means one involving nucleons and deltas interacting by potentials or by meson exchanges. An "unconventional model" refers to one which necessarily involves quark degrees of freedom, such as the color quantum number.

A. Rinat reported on a calculation of the $pp \rightarrow d\pi^+$ reaction in which he incorporated quark effects in the $\Delta N\pi$ vertex by means of a cloudy chiral bag model. The changes in going to this more complicated description are small, of the order of 10% or less. Rinat feels -- now -- that the amount of effort involved in such calculations at medium energies is hardly worth it. For the energy regime under discussion, he says, it is probably most efficient computationally to use the "collective coordinates" of baryons and mesons.

In discussion afterwards, E. Lomon suggested one should not be so pessimistic. Perhaps we haven't seen quark degrees of freedom yet because their major effects lie at center of mass energies above 2.5 GeV, somewhat higher than have been investigated up to now. You will hear more about this from Lomon later this morning.

Altogether, I must confess that I am pretty sympathetic to Rinat's point of view on this question. Certainly, at this point, there is no clear and distinctive signal for quarks in intermediate-energy NN physics. There are a few suggestive experimental data that, if they prove to be correct, may require eventually a quark-based explanation. I will mention a few of these as we go along, but, so far, conventional physics seems to be adequate to describe the physics in this energy region.

V- THE DIBARYON PROBLEM

The question of whether there are dibaryon resonances in NN scattering at medium energies is now almost eight years old. It is clear that something interesting is happening in the 1D_2 and 3F_3 partial wave amplitudes, but whether the underlying dynamics is conventional or not is still very controversial. Besides discussion of these two well-known cases, there were a few suggestions at the Workshop of new possibilities for dibaryon resonances.

It was strongly emphasized by P. Kroll that, as far as the Particle Data Group's criterion for a resonance is concerned (approximate Breit-Wigner behavior in an Argand plot), the 1D_2 and 3F_3 waves have "four-star resonances". The counter-clockwise looping behavior as the energy increases is clear. It is also very clear that these partial waves are very inelastic, with most of that inelasticity coming from single-pion production, $NN \rightarrow NN\pi$. What is not clear is whether this behavior is due to "theoretical resonances", i.e., poles on the second (or higher) sheet of the complex energy plane. The basic motivation for raising such a question is the importance of the $NN \rightarrow N\Delta$ thresholds in these partial waves. The coupling to the inelastic $N\Delta$ channel provides an attractive force which may or may not give rise to a resonance pole. Certainly, much of the motion on the Argand plot can be attributed simply to the thresholds, which analytically correspond to cuts rather than poles.

Kroll pointed out that the 1D_2 resonance appears to be nearly "purely conventional", a manifestation of this Ball-Fraser inelastic coupling mechanism. The situation for the 3F_3 , he feels, may be more interesting, since present conventional models do not seem quite able to reproduce all the features of the data. Perhaps this triplet resonance is "mixed", having both a conventional and an unconventional component. Kroll showed transparencies of how the addition of a dibaryon resonance term in this partial wave made dramatic improvements in his Deck model predictions of certain observables, such as $\Delta\sigma_L(pp \rightarrow NN\pi)$.

It is probably useful to be cautious about these conclusions. First, conventional models, especially for $NN \rightarrow NN\pi$, are few and limited in the physics they include. Also, unitarity is probably essential for understanding odd-tensor observables like A_{NO} , the scattering asymmetry with respect to the beam. I will show a figure illustrating this point later on. Kroll and his colleagues plan to apply their model to predict A_{NO} and other such quantities. Finally, besides comparing with all available $NN \rightarrow NN\pi$ data, future analyses should eventually also include information on the strongly-coupled $NN \rightarrow NN$, $NN \rightarrow \pi d$ (and its inverse), and $\pi d \rightarrow \pi d$ reactions.

A first step towards such a unified analysis has already been taken by an Osaka group, as reported by N. Hiroshige. They have extended the K-matrix approach to include $pp \rightarrow pp$, $pp \rightarrow \pi d$, and $\pi d \rightarrow \pi d$ amplitudes as determined by recent phase shift analyses for each of these reactions separately. Indeed, they find highly inelastic resonance poles in the 1D_2 and 3F_3 partial waves. A bit of caution is in order here as well. The Osaka group has not (yet) included any $pp \rightarrow NN\pi$ information in their analysis, and that is the major inelasticity in this region. Moreover, one can sometimes be fooled by the K-matrix approach. Some "experiments" have been done /2/ in which theoretical models are used to generate "data", which in turn are used for a K-matrix fit. The resulting output amplitudes do not always resemble the input amplitudes.

Are there any hints of new dibaryons, i.e. resonances other than the $I = 1$ 1D_2 and 3F_3 ? Several suggestions came up during the workshop, although none of the following needs to be taken seriously yet. First, there was nothing said about $I = 0$ dibaryons. In fact, the data and theoretical curves shown by Meyer for $\gamma d \rightarrow pn$ can be taken as evidence that the Kamae data should not be interpreted in terms of a resonance.

Things were more exciting in $I = 1$. Lomon pointed out, on the basis of a vague wiggle in the Saclay-Genève phase shift analysis, that there might be something resonating in the 3P_0 amplitude near 575 MeV. I was very surprised not to hear a discussion, in the Workshop, of the three narrow bumps found by an Orsay-Saclay group /3/ in the reaction $^3\text{He}(p,d) X$. As reported at the PANIC, the invariant mass of X peaks at 2124, 2189 and 2243 MeV, with widths of the order of 20 MeV. A. Masaike told us about a KEK polarized-target experiment on elastic πd scattering at 740 MeV, which shows no trace of a previously reported 1G_4 resonance. And finally, not at the Workshop but yesterday, K. Locher told us of another narrow bump at $M = 2020$ MeV in the photoproduction of pions from deuterons.

VI - MODEL DEPENDENCE OF INELASTICITY PREDICTIONS

The spin dependence of total inelastic cross sections has recently become available for the first time, and it is telling us a great deal about the dynamics of the inelasticity. Figure 1 shows $\Delta\sigma_1(pp \rightarrow NN\pi)$, another version of which appeared several times during the Workshop (and the week since). The three conventional model calculations miss entirely the shape and sign of the experimental data (which comes from Genève and ANL). Since it is the triplet (spins parallel) cross section that has the negative sign, the lesson to be drawn from Fig.1 is that there is triplet inelasticity missing from conventional model descriptions.

One way of including more triplet inelasticity, exploited by Kroll and his colleagues, is to add an explicit 3F_3 dibaryon resonance to the model. This gives, after fitting a few parameters, the dashed curve shown in the figure. The fit to the data is much improved.

Even more recently we have begun to see $\Delta\sigma_T(pp \rightarrow NN\pi)$ data. R. Hess showed us Genève data for this quantity up to 580 MeV, and it turns out to be positive. Conventional model predictions for $\Delta\sigma_T^{\text{inel}}$ are also positive and rising in this energy region. Moreover, K. Imai earlier this year at a conference in Japan showed his extracted values of $\Delta\sigma_T^{\text{inel}}$ at higher energies and has claimed them to be in "reasonable agreement" with the Klotz-Silbar prediction. Thus we can draw an important conclusion:

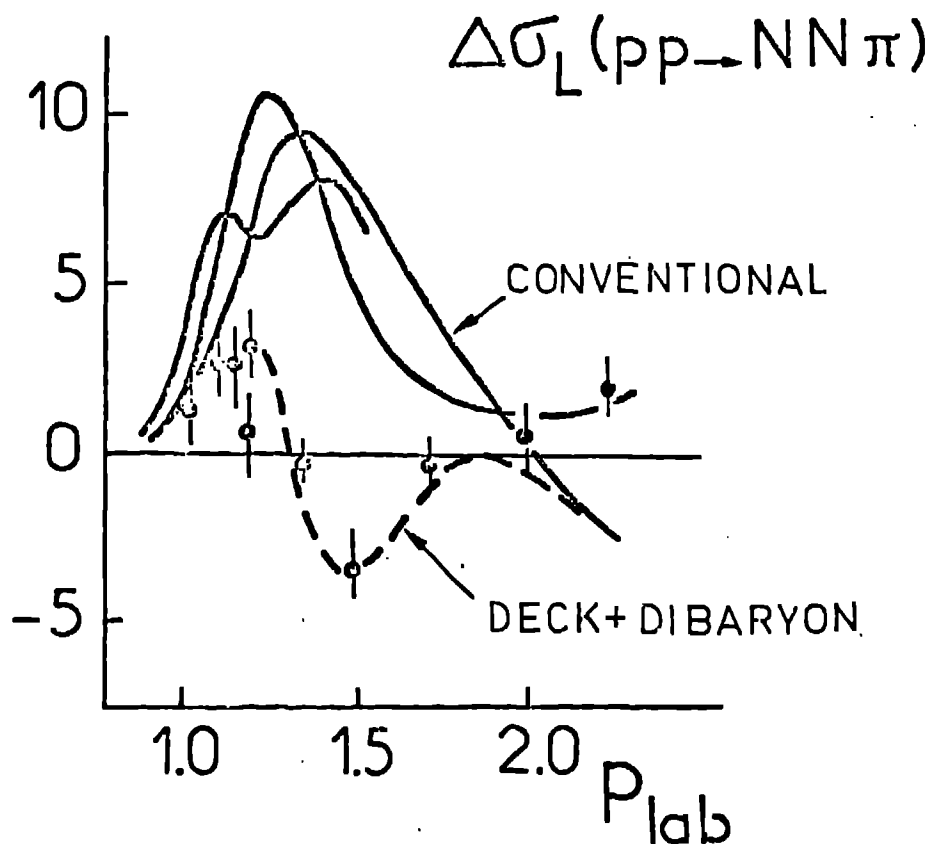


Fig.1 - Longitudinal spin dependence of the $pp \rightarrow NN\pi$ total cross section.

if $\Delta\sigma_1^{inel}$ is bad (compared with experiment) and $\Delta\sigma_T^{inel}$ is good, then the triplet inelasticity missing in the models has $L = J$.

At a finer level than total inelastic cross sections, one can study the inelasticity parameters η_{LSJ} in specific NN partial waves. Kroll showed a plot of $\eta(^3F_3)$ versus T_{lab} , comparing three conventional models, plus his Deck model with the added dibaryon, with the values extracted from elastic phase shift analyses. In general, the conventional models all underpredict the amount of inelasticity in this $L=J$ triplet wave. (The model with the fitted dibaryon, of course, does rather well.) This is quite consistent with the conclusion drawn in the last paragraph.

It is not obvious, however, that adding in a dibaryon is the only way to get the additional inelasticity here. First note that the 3F_3 partial wave is not a peripheral wave, since the NN state can couple to an $N\Delta$ state in a relative p-wave (with channel spin coupled to $S=2$). However, almost all the conventional models that have predicted η_{313} are of the one-pion-exchange type (including others not shown by Kroll). It might be that short-range forces not so far in the models (such as ρ -exchange) are responsible for the missing inelasticity here; that needs to be investigated.

In regard to the model predictions of $\eta(^3F_3)$, it is also worth noting that there are big differences between them. For example, at 800 MeV the Deck model η is 0.9, while Lomon's η is 0.73. Since the inelastic cross section in a partial wave goes like $1 - \eta^2$, that means the two models differ by almost a factor of three. In a non-peripheral partial wave, like 3F_3 , this sort of model dependence is not so surprising, even if annoying.

In fact, however, even the peripheral partial waves show a wide variation in predicted inelasticities. This point was not really discussed at the Workshop, I know of it

body) unitarity in the calculation. The πN input is relatively simple, but leads to a model with iterated pion-exchange forces with no free parameter. The Wuppertal group, as we have seen, can add in explicit dibaryon terms, which, though a non-unitary procedure, doesn't hurt them since their model isn't unitary to start with. We, on the other hand, cannot add such terms (very easily), and have not yet done so.

Actually, the unitary OPE model does quite well in describing a large $pp \rightarrow np\pi^+$ data base. A "typical" comparison with the extensive 800 MeV Rice-Houston data /6/ for an exclusive differential cross section and the corresponding asymmetry parameter, A_{NO} , is shown in Fig.2. For this forward-angle proton case, the predicted cross-section is a bit low, but at larger proton angles it is high. This reflects the overprediction, in this model, of the $NN(^1D_2) \rightarrow N\Delta(^5S_2)$ inelasticity, which in turn means a too isotropic $N\Delta$ final state. The peak in the cross section around outgoing proton momentum of 600 MeV/c is due to an NN final state interaction in the 1S_0 or 3S_1 states. Our model does not contain any such dynamics (yet), whence it misses this peak. The unitary prediction for A_{NO} , shown as a solid curve, has the right shape but is displaced downwards from the data. The dashed curve on that graph is our Born approximation calculation of A_{NO} , and the difference between the two curves illustrates my earlier point about the importance of unitarity for understanding such observables. For other proton-pion angle pairs, the agreement between model and data is sometimes better and sometimes worse than shown.

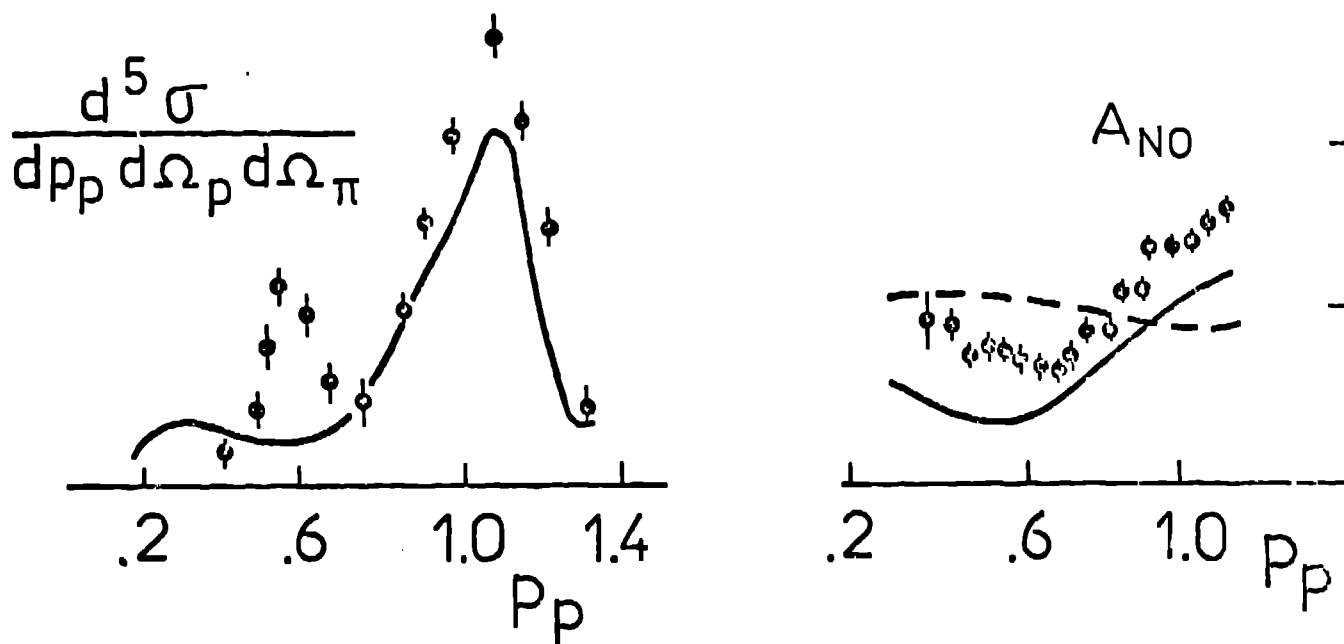


Fig.2 - Exclusive differential cross section and asymmetry for $pp \rightarrow np\pi^+$ at 800 MeV, $\theta_p = 14^\circ$ and $\theta_\pi = 42^\circ$ (lab).

This unitary model has also been compared, with about the same degree of success, to many other kinds of data on single-pion production from 420 to 800 MeV. These include spin-spin correlations /7/, spin-transfer coefficients /8/ and polarization asymmetry in $pp \rightarrow pp\pi^0$ /9/ and in inclusive $pp \rightarrow pX$ /10/.

So, what are the problems in the $NN \rightarrow NN\pi$ reactions? I have already discussed $\Delta\sigma$ ($pp \rightarrow NN\pi$) and the missing $L = J$ triplet inelasticity. Probably closely related to this is the wrong sign prediction for A_{LL} at 800 MeV, mentioned by Kroll. A perhaps entirely different problem is the spin-transfer coefficient measured for the inclusive $pp \rightarrow pX$ reaction at 0° at 800 MeV /11/. Figure 3 shows the K_{LL} coefficient, comparing with our unitary model. Again, the shape and magnitude are completely wrong. In this case it is not clear at all how more $L = J$ triplet inelasticity can improve

from a recent preprint by Kloet and Tjon /4/. For example, the 3F_4 partial wave inelasticity at 800 MeV also varies between models by a factor of three or more in the cross section. Kloet and Tjon attribute the differences to two sources : differences in the treatment of Δ propagators, and whether the cutoff functions in the models are functions of variables appropriate to a two-body coupled-channel approach or to a three-body Faddeev type approach. It is not obvious how the latter ambiguity can, or should, be resolved.

This model dependence of the inelasticity parameters is unfortunate, since we therefore cannot now use theory to fix the peripheral partial wave η 's, thereby reducing the number of parameters that must be fit in a phase shift analysis. The best that can be said is that people doing phase shift analyses ought to try searching on the η 's within an "error band" indicated by the various different model predictions.

VII - HOW GOOD ARE PRESENT DAY CONVENTIONAL MODELS ?

I now want to discuss how well today's first-generation conventional models compare with experiment. For the reactions $\pi d \rightarrow \pi d$ and $pp \rightarrow \pi d$ I will be brief, since these topics were very nicely covered in Locher's talk yesterday. On the other hand, the $pp \rightarrow NN\pi$ reaction has not so far been very extensively reviewed in this series of Spin Physics conferences. The basic question to bear in mind throughout this discussion is, to what extent do disagreements with data need to be modified by unconventional dynamics?

Regarding the T_{20} controversy in elastic pion-deuteron scattering, there is little to say theoretically. If the ETH data do turn out to be correct, however, then their narrow structures will almost certainly require an unconventional explanation. On the other hand, if the ANL data are the correct ones, then T_{20} can probably be understood with conventional models.

Note all problems in pion-deuteron scattering are discrepancies between experiments. Although he was not a participant at the Workshop, several people referred to calculations by H. Garcilazo /5/, which compare well with the it_{11} and (ANL) T_{20} data. Unpublished work by him also does a good job in representing the cross sections and vector polarizations for pion-deuteron breakup, $\pi d \rightarrow NN\pi$. The problem, however, is that Garcilazo has no absorption channel (i.e., $\pi d \rightarrow NN\pi d$) in his model. Is this, as Locher says, "a giant step backwards"? Or, are the usual treatments of the P_{11} πN input amplitude, which tend to involve a big on-shell cancellation between a pole-term and a background contribution, wrong in some unknown way. don't know, but the latter sounds at least plausible to me.

For the $pp \rightarrow \pi d$ reaction present theories are overwhelmed by very precise and numerous data. Conventional models only predict the trends of observables like A_{NO} correctly. The agreement between model and data, Locher showed us, can be much improved if one simply adds, in an ad hoc manner, some (non-resonant) triplet strength, which in his examples was in the 3P_1 and 3F_2 partial waves. Again, this is quite consistent with the conclusions drawn $\Delta\sigma_L$ and $\Delta\sigma_T$. As far as I know, there are no unconventional models proposed for this reaction (other than Rinat's cloudy bag essay), and that is probably because the data provide no compelling reason to propose them.

For calculations of the reaction where most of the inelasticity occurs, $NN \rightarrow NN\pi$, there are only two active groups. The theoretical basis for all models in this energy region is the isobar model, proposed in 1958 by Lindebaum and Sternheimer and by Mandelstam. Here the initial NN state makes a transition to a (spectator) nucleon and an isobar, which then propagates some distance and then decays into a nucleon and a pion. The most important isobar is the $\Delta(3,3)$, for which the propagator is like a Breit-Wigner resonance factor. The Wuppertal group, headed by Kroll, uses a form of this model called a Deck model (or, a Ferrari-Selleri model), in which the isobar production amplitude is given by single-pion exchange and the πN input information is taken from the Karlsruhe-Helsinki analysis. Our group /5/ solves coupled-channel three-body equations for that production amplitude, thus maintaining (two- and three-

the situation. Nor is it clear whether the trouble comes from the lack of short-range forces in the model.

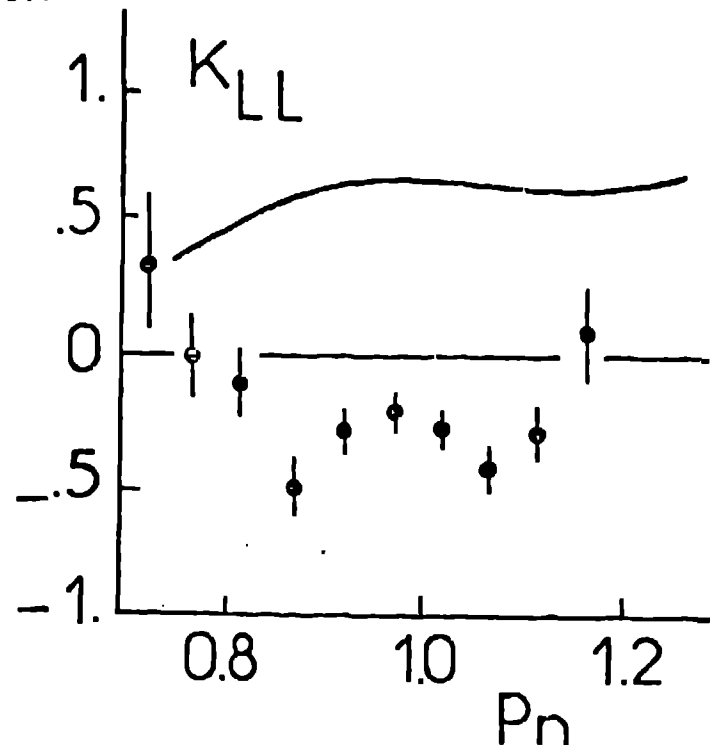


Fig.3 - Spin transfer coefficient K_{LL} for $pp \rightarrow nX$ at 800 MeV, $\theta_n = 0^\circ$.

What needs to be done to improve today's $NN \rightarrow NN\pi$ models? First, other unified-model builders should turn their attention to the unbound three-body final state. Kroll and company and Dubach, Kloet and Silbar badly need some competition! It is very hard to know what the model dependence of certain predictions is if there are only two models.

A second major improvement would be to go beyond one-pion-exchange forces. To put shorter-range forces into these models, one could use phenomenological potentials or, perhaps more satisfying, the exchange of heavier mesons. The nucleon-nucleon final state interactions are, as we have seen, sometimes important, and these should also be brought into the models. As mentioned earlier, it might eventually be necessary to include explicit dibaryon resonances in the models, and one would hope that this can be done in way that does not vitiate unitarity.

In the farther off future, say, in about five years, we might expect to see unified amplitude analyses of all the $I = 1$ data in the single-pion-production region. This would treat $NN \rightarrow NN$, $NN \rightarrow \pi d$, and $NN \rightarrow N\pi$ data simultaneously, using theory to constrain or fix the high- L partial waves.

VIII - CONCLUSION

In general, theoretical models of the inelastic NN region are doing as well as (or better than) could have been expected. This has been no easy task in view of the recent flood of new experimental results. I see three major problems that theorists must now face: the model dependence of the peripheral partial wave inelasticities, the missing $S = 1$, $L = J$ inelasticity, and the K_{LL} problem in forward $pp \rightarrow nX$. Apart from these problems, which probably are soluble "in the usual way", my basic conclusion as a spectator at the NN Workshop is that there is no clear evidence today in the NN system at intermediate energies for unconventional dibaryon resonances.

In closing, I would like to thank C. Lechanoine-Leluc for doing essentially all of the organizational work for this Workshop. J. Soffer and F. Lehar were very helpful with the arrangements, support, and good advice. And M. Moravcsik as chairman did a good job of keeping the Workshop on track, in spite of a very crowded schedule.

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