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LA-UR--88-3320

DE89 002047

TITLE INTERMEDIATE-ENERGY HADRON INTERACTIONS, II

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SUBMITTED TO Summary Talk at Spin and High Energy Physics Symposium,
Minneapolis, MN, September 16, 1988

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INTERMEDIATE-ENERGY HADRON INTERACTIONS, II

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INTRODUCTION

I will summarize here the second parallel session on intermediate-energy hadron reactions, held yesterday afternoon. There was already such a summary given Tuesday morning by Mike Moravcsik for a similar session held on Monday afternoon. In the session yesterday there were nine talks in all, six of them by experimentalists and three by theoreticians. Both parallel sessions were organized (in parallel) by Hal Spinka and Gianni Pauletta, and there was some overlap between them in topics. In the interest of maintaining a perspective, therefore, I will not restrict my remarks only to yesterday's talks.

The topics to be covered are as follows. I'll begin with new developments in $NN \rightarrow NN\pi$ reactions. This will provide a natural lead-in to the main topic of this talk, which is dibaryons. This will be followed by discussion of elastic proton-deuteron and inelastic proton-alpha scattering. Then there will be a brief mention of two technical developments. Finally, I'll close by giving short remarks about two peculiarities that were found by theorists looking at strong interaction amplitudes.

NEW $NN \rightarrow NN\pi$ DATA AND ANALYSIS

Two experimental contributions described new results in intermediate-energy single-pion production. There has been an enormous increase in the last five years of data for exclusive-kinematics spin observables for this process.¹⁻³ The two experiments reported here will in themselves constitute a large fraction of the expanded $NN \rightarrow NN\pi$ data base.

Peter Riley described work at LAMPF which measured spin correlations A_{1j} in $pp \rightarrow pn\pi^+$ at five energies between 500 and 800 MeV. Seven spin observables were measured. Because of the polarized proton target, all three final state particles were detected to extract a very clean sample of events of about a million events, distributed evenly over the energy range. A short report of this experiment has already appeared.⁴

There has also been recent work performed at PSI by the Geneva group, described by Philippe Sormani, in which scattering asymmetries and spin transfer (Wolfenstein) parameters were measured at 517 and 580 MeV. In this case six observables were measured. The double scattering experiment improved its statistics by selecting events which required a scattering in the recoil polarimeter block. Again, a very clean signal was observed.

I had originally hoped to present here a nice figure showing some of the Geneva data, but unfortunately the experimentalists have not (yet) collected

their data in such a form. What is available are (rather dense) tables of their results. This illustrates one of the difficulties of presenting results for a three-body final state; there is a lot of phase space covered by such data. Sornani gave a warning, and it is one that I agree with, that presenting one-dimensional projections of such data can hide a lot (or too much) of the physics present in the exclusive data.

With regard to the analysis of the $NN \rightarrow N\Delta$ data, I am happy to tell you that we have now entered a new era. Riley also reported the first partial wave analysis for a $NN \rightarrow NN\pi$ reaction.⁴ The major credit for this achievement goes to Rick Shypit and David Bugg.

What Shypit and Bugg did can be described briefly as follows. They performed a least squares fit for the magnitudes and phases of the low- L $NN \rightarrow N\Delta$ partial wave amplitudes. The high- L amplitudes were taken from theory, given essentially by the one-pion-exchange Born approximation, and were held fixed in the fitting procedure. The model takes account of the isobars Δ^{++} , Δ^+ , and S (a mixture of the S_{11} and S_{31} πN interactions), all mutually interfering and all varying appropriately as their invariant masses change. The fit was done, at each energy, in terms of Gottfried-Jackson variables with the experimental data sorted into about 300 bins.

The extracted phases can, in a weak-coupling limit and after some manipulation, be interpreted as scattering phase shifts for elastic $N\Delta \rightarrow N\Delta$ scattering. Such information has not heretofore been available. Moreover, the fitted magnitudes, in turn, will soon be used to determine the NN inelasticity parameters, η_{LSJ} . The latter quantities should be much more precise than those obtained from elastic NN phase shift analyses, simply because much more detail of the inelastic reaction (especially regarding angular distributions) goes into their determination.

The first partial wave analysis for $N\Delta \rightarrow N\Delta$ is obviously not the last one. In the future such analyses should be based on an expanded database which includes all the available data, not just that from one experiment. Eventually it will be important to do a *unified* analysis of all the relevant reactions, $NN \rightarrow NN$, $NN \rightarrow \pi d$, as well as $NN \rightarrow NN\pi$. Finally, to get an idea of the model dependence of the results, it will be very useful to see such analyses done by more than one investigator.

DIBARYONS AGAIN

In some people's minds, the trouble with dibaryons is that they never die. From what has happened at this conference, it seems that they are not even fading away. To be honest, two weeks ago when I thought about what I might be saying here, I concluded that there wouldn't be much about dibaryons. The events of this conference have caused me to make them the main point of this summary.

First, a social physics comment. One speaker on Monday, George Glass, didn't actually say anything much about them other than to state that

“dibaryon” is a dirty word.” He didn’t say why. Let me point out that if that is true, then it is the first eight-letter dirty word anyone ever taught me. Perhaps that means it is twice as potent as the usual such words.

To make some sense of this 12 year-old subject, it is useful to point out that there is no real consensus as to what a dibaryon is. This week we have heard the following different working definitions:

- Bugg: “A broad dibaryon corresponds to a phase shift in the $N\Delta \rightarrow N\Delta$ channel which rises through 90° .”
- Hoshizaki: “It is a pole in the s -plane.”
- Lomon: “An exotic dibaryon is one whose structure depends explicitly on quark or gluon degrees of freedom.”

Please bear these semantic ambiguities in mind as I go through what has happened this week.

To begin, some classification. I’ll start the discussion with what is new regarding the broad dibaryon candidates. These are the “old” ones that are seen as loops in the Argand diagrams for the 1D_2 and 3F_3 $NN \rightarrow NN$ partial waves (and perhaps others). Then I’ll go into the “newer” narrow dibaryon candidates, where by “narrow” I mean something with a width of 30 MeV or less. These seem to be falling into two groups, corresponding to center-of-mass energies W near 2.1 and 2.7 GeV.

The broad dibaryon flap at this conference arose because the Shypit-Bugg partial wave analysis for $NN \rightarrow N\Delta$ mentioned in the last section was published with the title “Evidence Against Broad Dibaryons”. The extracted elastic $N\Delta \rightarrow N\Delta$ phase shifts were all rather small (as for the 3P_2 or 3P_3 waves) or falling as a function of energy (as for the 5S_2 wave). Neither case looks like a rising phase shift associated with a resonance or bound-state pole in the $NN \rightarrow N\Delta$ channel, which is the dominant channel. In fact, the falling behavior of the 5S_2 phase shift fits in very nicely with the interpretation of the 1D_2 “dibaryon” as an effect due to the cut from the $N\Delta$ threshold.⁵

Riley’s talk was immediately followed by a contribution from N. Hoshizaki, communicated to this meeting by his colleague, A. Masaike. Hoshizaki’s main point is that the result for the 5S_2 phase shift by Shypit and Bugg could in fact be better interpreted as evidence *for* a broad dibaryon, not against. He considered two models, one involving an inelastic virtual state pole and the other involving an unstable bound state pole. (By way of analogy, recall the natures of the 1S_0 and 3S_1 NN amplitudes near the NN threshold.) Hoshizaki claims the bound-state-pole model looks more reasonable when compared with the data.

So, that reopens what might have been considered a closed controversy. There are clearly things here that need sorting out. Among them are the semantic differences noted at the beginning of this section, along with what might be some misunderstandings at the technical level of how the fitting was done.

Turning now to the narrow dibaryons, the situation at an invariant mass of $W \approx 2.1$ GeV has become (experimentally) clarified. For many years now, an

Orsay group⁶ has been seeing three narrow bumps (on top of a large background) in the differential cross section for the reaction ${}^3\text{He}(p,d)X$. (The isospin of X must necessarily be 1.) I recall asking at the Marseilles conference four years ago why no one from the Orsay group spoke about these interesting data. The answer was, "But this is a *spin* conference and we didn't measure any polarizations."

This week Magid Gazzaly reported that a Minnesota-Udine-Texas-LAMPF group has repeated the Orsay experiment, but with a polarized proton beam. They confirm the existence of the narrow bumps. In fact, the asymmetry, A_N , which Moravcsik showed you on Tuesday, is striking. It shows not only the three Orsay bumps but two more at lower energies:

LAMPF	Saturne
2.016±0.005	
2.054±0.004	
2.125±0.003	2.124±0.003
2.152±0.004	2.155± ?
2.181±0.005	2.192±0.003

All widths in both experiments are ≤ 35 MeV.

What can be said at this point about these narrow peaks? In the first place you should be aware that as yet there is *no* spin-parity information about any of these peaks. Nonetheless, as many theorists have conjectured, the above energy levels *look* like they constitute a rotational band, $W = M_0 + M_1 J(J+1)$. Ten years ago in a calculation based on a stretched six-quark bag model, Mulders, Aertz, and deSwart⁷ found just such a formula. In the time since then, however, Mulders has argued that the states they predicted might not be observable.⁸

In a paper presented just after Gazzaly spoke, H. Konno reported a calculation done with Nakamura and Noya based on a cluster model with three diquarks. Such a model also gives five rotational-like states, and it is interesting that these authors were aware of the Orsay peaks but not the two lower peaks that had just been reported by Gazzaly! One should also note that K. Althoff has just reminded us, earlier this morning, that one of these lower peaks was probably seen at Bonn some years ago in the invariant pp mass in the reaction $\gamma d \rightarrow pp\pi$.

A note of caution: the subject under discussion is a complicated four-body reaction. It could be that these peaks are not dibaryons at all but reflect some complicated few-body final state interaction. There is no theoretical work that I know of, however, that makes such a claim. If the Bonn peak mentioned by Althoff is indeed related to the ${}^3\text{He}(p,d)X$ peaks, this might argue against an F.S.I. interpretation.

There are now also strong candidates for at least one narrow dibaryon near $W = 2.7$ GeV. Actually, this is something that Earle Lomon has been telling us for years, and the subject of his talk here was where and how to find such exotic states. In a formalism that combines the R matrix techniques with a cloudy bag quark model, Lomon predicts narrow states in the NN system as follows:

W (GeV)	Γ (MeV)	J^π
2.63	50	1^+
2.70	50	0^+
2.88	100	2^+
2.90	100	3^+

Some of these are $I = 0$ objects and some are $I = 1$. Lomon warns us that the structures associated with these states that can be seen in spin observables might only show up as small excursions from the smooth background energy dependence. Incidentally, Lomon also discussed predictions for strange dibaryon exotics and Z^* 's. However, nothing was said about the experimental status of such exotics in the intermediate-energy parallel sessions of this conference.

There *have* been some hints that such NN exotics may exist in this energy region. There is a bump in the $\Delta\sigma_L$ and A_{LL} observables measured long ago at the ZGS,⁹ and more recently, at Saturne, people have seen structure near 2.7 GeV in the asymmetry for $pp \rightarrow d\pi^+$.¹⁰ More convincing evidence has emerged during this conference, as reported by A. de Lesquen for the Saclay NN group headed by François Lehar. For some time now this group has been measuring three-spin correlations in $\bar{p}\bar{p} \rightarrow \bar{p}\bar{p}$ scattering. With about 15 such spin observables at 11 energies for T_{lab} from 0.853 to 2.7 GeV, they have recently¹¹ been able to make a direct reconstruction of the five (Wolfenstein) spin amplitudes. What is very interesting about these amplitudes is that three of the four relative phases show a very large dip or jump at one, and only one, energy, $T_{lab} = 2.1$ GeV (which corresponds to $W = 2.74$ GeV). Unfortunately, the energy spacing in this region is somewhat coarse; thus one cannot say whether this is actually a narrow structure.

The Saclay group has since measured the scattering asymmetry A_N at finer energy intervals:

Fig. 1. A_{NN} versus T_{lab} from the Saclay group (preliminary data).

Their results, at three different angles, all show a bump of more than 0.1 units at the same energy. The CM width of the peak seen here is less than 50 MeV.

Again, there is no information as to the spin and parity of this resonance, if it is truly a resonance.

To summarize the present status of dibaryons:

- 1) The broad dibaryon candidates are still in an unsettled state. The partial wave analysis that led to "evidence against" clearly needs refinement and an expanded database.
- 2) The narrow bumps seen in ${}^3\text{He}(p, d)X$ have been confirmed. There are now five such structures.
- 3) There is new and pretty convincing evidence for at least one narrow dibaryon near $W = 2.7$ GeV.

There will certainly be more discussion of dibaryons at Spin '90!

pd AND *pn* SCATTERING

For some time now there has been an extensive program at the ZGS, LAMPF, KEK, and Saturne to measure spin observables for elastic *pd* scattering. The leader of this effort has been George Igo, who reported here on the Saturne measurements, done in collaboration with the Lehar group. Here they make good use of one of Saturne's technical advantages by working with a vector- and tensor-polarized deuteron beam.

The data Igo presented was taken at a deuteron beam energy of 1600 MeV, corresponding to an equivalent proton beam energy of 800 MeV, thereby complementing the LAMPF data set. By now there exists at this energy an "overcomplete" set consisting of about 30 different spin observables. An example of their results is:

Fig. 2. The correlation for $p(d, p)d$, all spins normal.

One interesting point about these data (and, for that matter, other data presented at this conference) is that some of these observables are three spin measurements, involving polarized beam, polarized target, and polarization determination of one of the final particles, including the one shown in Fig. 2. This

is the first Spin Conference, I believe, in which such three-spin data have been shown and discussed as common state-of-the-art results.

What is the motivation for this enormous experimental program? One of Igo's stated reasons is to use the overcomplete set of data to construct directly the 12 spin amplitudes for the $pd \rightarrow pd$ reaction. (This has not yet been done, even at 800 MeV where the data is most complete.) However, I do not find that a very compelling argument; the five elastic NN spin amplitudes are quite useful as inputs to nuclear physics, but there does not seem to be a comparable usefulness for pd spin amplitudes. Other people, such as Steve Wallace, feel these pd data provide a very useful testing ground for nucleon-nucleus scattering theories. In this case the spin-1 nature of the target is sensitive to details not coming into scattering on the more usual spin-0 nuclear targets. A potentially very important motivation for this $pd \rightarrow pd$ program is, I believe, the possibility of being able to use these data to get good, solid information about the off-shell $NN \rightarrow NN$ amplitudes. Recall that, despite great effort over twenty years, the proton-proton bremsstrahlung measurements have not given us much useful information.

But, can present-day (or near-future) theory confront these data? I am worried that it might not and that these beautiful data may have been taken in vain.

To estimate the amount of effort needed to extract useful NN information from the knowledge of $pd \rightarrow pd$ scattering, let me describe briefly a very recent calculation done by the Graz group.¹² They work at $T_{lab} = 2.5$ MeV, an energy below the break-up threshold, starting with a separable approximation (up to rank 6) for the Paris NN potential. At this low an energy they can get by with keeping only NN interactions in the 1S_0 , $^3S_1/{}^3S_1$, and all P partial waves. (This still means they have up to 16 coupled channels to deal with in any partial wave.) This low-energy problem is plagued by Coulomb complications, but they have learned how to get past this by using a shielded potential in a renormalization approach. The rather creditable results they get for about six observables required 600 CPU-hours of VAX-8700 time (their estimate). The theoretical effort, I would guess, must have cost about five man-years.

How would this scale up to a comparably serious effort at 800 MeV? First, there will be many more partial waves involved, which brings in a factor of 10 or so in CPU-time. The complications of break-up alone will probably cost a factor of 2 in CPU-time and double the coding effort. At this point it becomes clear this is not a problem for a VAX but for a supercomputer. The $10 \times 2 \times 600$ VAX hours might reduce to something like 600 hours on a scalar machine. Vectorization might save another factor of 10 in CPU-time, but it would also probably double the coding effort.

One big simplification at 800 MeV is that one can neglect the Coulomb interaction. However, at 800 MeV, this may be offset by the complication of pion production. In the much simpler $NN \rightarrow NN\pi$ case, Francesca Sammaruca just spent about three years getting to the point where she could believe

the results coming out of her three-body model code.¹³ Maybe, if we are lucky (because the two nucleons in the deuteron are so far apart and we require a pd final state), intermediate Δ 's will decay into (real) pions before they can convert back to nucleons in the next scattering. This possibility needs justification. Another possible simplification is that maybe one can work with a less pathological potential than the one the Graz people used. This too is probably balanced by a lack of knowledge (or consensus) about what the NN potential model at 800 MeV is. Or even what it ought to be; there are problems having to do with relativity that we haven't solved yet.

What the last two paragraphs show you is how hard it is to make a good estimate of how long it will take to do a big calculation. It appears that the computing demands are costly but not unreasonable – of the order of 100 super-computer hours. The manpower cost, on the other hand, is very large, perhaps 20 to 30 man-years. It is not just someone's Ph. D. thesis.

In the meantime, there may be simpler things that theoreticians *can* do to deal with this $pd \rightarrow pd$ data. Indeed, the curve in Fig. 2 is a calculation,¹⁴ albeit somewhat *ad hoc*, that doesn't do too badly. Perhaps it is even barking up the wrong tree for me to insist that the problem be done in a partial wave formalism with the full machinery of a three-body formalism.

Turning to a subject more in nuclear physics, Sirish Nanda showed us the following picture:

Fig. 3. Spin flip probabilities for inelastic proton scattering

This is an unexpected result for another few-nucleon reaction, ${}^4\text{He}(\vec{p}, p'){}^4\text{He}^*$ at 500 MeV and at a momentum transfer of about 350 MeV/c. The Minnesota-CEBAF-Rutgers-LAMPF group measured the spin-flip probability, $S_{NN} = \frac{1}{2}(1 - D_{NN})$, with an eye to learning the $\Delta S = 1$ response of the ${}^4\text{He}$ nucleus. (Here $\Delta S = 1$ refers to spin-flip, not to a strangeness-changing decay.) The S_{NN} observable had previously been measured by another group for the nuclear targets ${}^2\text{H}$, ${}^{40}\text{Ca}$, and ${}^{208}\text{Pb}$. What is found (Fig. 3) is that once again ${}^4\text{He}$ is different; this time its S_{NN} is about twice the size of that for a "normal" nucleus. As far as I know, there is no explanation for this difference.

TWO TECHNICAL DEVELOPMENTS

Gerald Gaillard of Geneva described progress in the development of a polarized neutron beam at PSI. They get longitudinally polarized neutrons from longitudinally polarized protons using the ${}^{12}\text{C}(\vec{p}, \vec{n})X$ reaction at 580 MeV. The neutron polarizations range from 30 to 45% and their energies range from 300 to 580 MeV. The project has recently been funded to move to a more permanent home on the PSI experimental floor. At the time the project comes to completion (1990), the polarized neutron beam there will have an intensity about fifty times that at LAMPF.

The Geneva group will measure spin observables for elastic np scattering. They have already taken data for asymmetries A_N and spin-spin correlations A_{ij} . After recommissioning, they plan to measure three-spin observables.

At the synchrophasotron at Dubna, as those of you who were at the Serpukhov conference already know, it is now possible to accelerate polarized deuteron beams up to about 10 GeV/c. P. V. Nomokonov described two experiments done with this beam. (I characterize these experiments as being under the heading of technical development, although the experiments themselves did have some physics motivation.) In both experiments the beam of 0.5×10^9 deuterons per cycle with a polarization on the order of 30% impinges on a CH_2 internal target. The quantities which were measured were both for the reaction ${}^{12}\text{C}(d, p)X$: A_N at $T_{\text{Lab}} = 1.6$ GeV and T_{20} at $p_{\text{inc}} = 9$ GeV/c. The motivation for the 1.6 GeV data was the series of very backward (inclusive) scattering experiments done 10 years ago at LAMPF.¹⁵

TWO PHENOMENOLOGICAL ODDITIES

On the theoretical side of the session, there were, in addition to Loman's talk on exotics, two presentations. The first of these was by Alex Gersten and, in a sense, was a continuation of some things he already told us at Marseilles. Gersten has found that intermediate-energy $NN \rightarrow NN$ double-spin flip helicity amplitudes show an astonishing scaling property. Earlier it was a particular combination of isospins for the amplitude $(+ + |T| - -)$ that scaled (from 400 to 800 MeV), looking very much like a one-pion exchange Born approximation prediction. Now he has found that a similar scaling applies to the $np \rightarrow pn$

amplitude $(+ - |T| - +)$. He is then able to fit these amplitudes in a very simple "no exchange model", that is, by using OPE scattering wave functions outside some radius R (which is ≈ 1 fm) and setting the wave functions to zero inside R . This seems to imply that these double-spin-flip amplitudes do not involve any short-range forces!

It would be very interesting to see if this scaling behavior also holds at higher energies. The combinations Gersten forms all require knowing the np as well as the pp amplitudes, and the former are a bit of a problem right now. As mentioned earlier, there are 11 higher energies for which the Saclay NN group have determined the $I = 1$ pp spin amplitudes. Earlier measurements at the ZGS, at 6 GeV/c, also determine a complete set of pp amplitudes.

In a somewhat different vein, but in much the same spirit, Mike Moravcsik described a phenomenon which he and Gary Goldstein have found when strong interaction amplitudes are recast into the so-called "planar-transverse" representation. What they find is that these amplitudes have a strong tendency to be all purely real or purely imaginary, relative to one another. This has been found to be true for elastic $NN \rightarrow NN$ scattering (where the effect is more pronounced above 300 MeV), for $\pi N \rightarrow \pi N$ (for energies up to 40 GeV), and (with some theoretical input) for $\pi d \rightarrow \pi d$.

Both these talks point out some very peculiar properties of strong interaction amplitudes. I don't think anyone, at this point, knows the reasons for, or the implications of, these phenomena. Perhaps one or another of these oddities will provide an important clue that leads to new physics.

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