C12-19-001:
Strange Hadron Spectroscopy with Secondary K_L Beam in Hall D

J. Dudek, R. Briceño

This is the latest iteration of the proposal for a new K_L beam to be delivered to Hall D, making use of the present GlueX spectrometer. The motivation is twofold: a dedicated hyperon spectroscopy program, and a new measurement of $K\pi$ scattering, with the goal of constraining the low-lying kaonic resonances. The proposal was conditionally approved in the last PAC.

I. ON THE HYPERON SPECTROSCOPY PROGRAM

First, we discuss the proposed hyperon spectroscopy program. The comments raised here are nearly identical to the previous year's comments, which were largely ignored.

Within this respect, among the major points of impact are Heavy Quark Symmetry (HQS), connection to LQCD, and resolving outstanding puzzles. HQS is very well stablished and understood at this point, both formally and numerically via LQCD. This is a symmetry that works rather well in the bottom sector, reasonably well in the charmed sector, and questionably well in the strange sector. The PI's claim that "*The spectrum of Ξ hyperons also clearly has significant discovery potential with implications for heavy quark symmetry and relationships to mass splittings in charm and beauty hyperons*" It is not clear what further insight would be gained with regard to this symmetry from these observations. This point is further muddled by the fact that in other parts of the proposal, the PIs argue that the experimental data may put constraints on SU(3) UChiPT, which starts from the assumption that the strange quark is massless and therefore chiral. The strange quarks are interesting in part due to the fact that they are neither approximately massless nor infinitely heavy. As a result, they might provide insight into the transition from these two regimes where reasonable theoretical approximations may be implemented. Unfortunately, this nuanced view is not adopted by the PIs

The $\Lambda(1405)$ is highlighted as one of the primary state(s) that may be further constrained from the K_LN scattering data. This dynamical enhancement is indeed puzzling and could use clarification. The puzzle, as explained in the proposal is the fact that what was previously thought to be a single resonant pole is now believed to be two poles. One is just below the KN threshold, and the other one is far below and deeper into the complex plane. The real part of these poles lie outside the kinematic region that will be access by the proposed experiment, although the pole near threshold is expected to manifest itself above threshold. This state is fairly well constrained, and as the PIs point out the pole that is less well understood may be accessed via photoproduction data on $\pi\Sigma$ by CLAS. Finally, the PIs claim that systematics associated with model chosen for extrapolated into the complex plane "*can be traced back to the fact that the experimental data used to fix the parameters of the models is rather old and imprecise.*". In Fig. 6 they compare predicted experimental data that may be accessed using the K_L beam ($K_Ln \to K^-p, \bar{K}n, \pi\Lambda, \pi\Sigma$) using two different examples of models used to determine the aforementioned pole. Given the models chosen at this point for extrapolating experimental data into the complex plane, it is not clear that the systematic error, which is currently the dominant one, will be reduced by putting further constraints on the models. To convincingly show this, the PIs can "*tune*" Model-B2 in Fig. 6 to describe Model-B4 (or vice versa) and the available experimental data, and then proceed to determine the two $\Lambda(1405)$ poles.

In the discussion on the $\Lambda(1405)$, the PI's make reference to LQCD calculations in "*Modern LQCD calculations also support the view that this its structure is a* $\overline{K}N$ *state*" and again in "*Lattice calculations based on the sequential Bayesian do, however, indicate that the multiplet may have a mainly 3-quark structure*". These so-called *modern LQCD calculations* use outdated techniques that make use of uncontrolled approximations. This is to say that although the analytic and structural nature of the $\Lambda(1405)$ is interesting, these calculations do not give any systematic insight into what QCD may or may not support. It should be self-evident that these discrepancies are due to "user-error" in the application of Lattice QCD, given that they are both using the same underlying theory to reach dramatically different conclusions.

II. ON THE $K\pi$-SCATTERING PROGRAM

The other major area of the proposed experiment is the spectroscopy of excited kaons in the $K\pi$ final state, and in particular the determination of elastic scattering phase-shifts, which can be obtained by studying the $|t|$ dependence of $K\pi$ production processes, and attempting to isolate the pion exchange. Collecting new data on these phase-shifts is warranted given the rather limited database obtained in experiments at SLAC, and the difficulty that has been found in pinning down the pole position of the scalar κ meson using these data. As pointed out in previous theory reviews of earlier PAC proposals, the extraction of phase-shifts is quite challenging, and the previous proposals did not provide sufficient detail to understand how it would be done.

One notable item of progress since the last proposal, is the proposed use of Δ^{++} final states. This has the advantage over a recoiling proton, that the t-channel exchange is charged rather than neutral, potentially simplifying the modelling. In addition it proves possible to perform isospin separation ($K\pi$ can be produced in both $I = 1/2$ and $3/2$) by measuring $K_L p \to K^- \pi^0 \Delta^{++}$ and $K_L p \to (K_L)\pi^- \Delta^{++}$. Of course the use of the Δ^{++} is not without its challenges, see below for comments regarding how low in $|t|$ one can go (and hence how close to the pion pole), and the quality of resolution in the required process $K_L p \to (K_L)\pi^- \Delta^{++}$ where the final state K_L is not detected.

An improved determination of the κ pole position remains a major claim of the proposal, and this is meant to be supported by Fig 12, where proposed data lying along the phase-shift curves coming from Rodas' dispersive $K\pi \to K\pi$ amplitude are shown. In previous versions of the proposal the analogous plot contained a simple scaling of the statistical error according to the expected number of events in KLF, which failed to take into account the many steps needed to get from raw events to a phase-shift, and the 'systematic error' that could be introduced in that journey. In the current Fig 12 what would appear to be an *incredibly crude* estimate of systematic error appears to have been introduced, simply an energy independent 5% systematic error added on each phase-shift point (which totally dominates the statistical error), with no suggestion as to how that originated, or if is conservative or optimistic.

Furthermore, the fit (in orange) to this data with 'systematic errors', does not account for the fact that clearly the uncertainties on these points at different $m(\pi K)$ shown in Figure 12 are *completely correlated*, and as such the fit returns a very small uncertainty which does not reflect the (correlated) uncertainty on the data points.

In addition, if Fig 12 is supposed to reflect what the final result of the experiment will look like, the binning (which seems to be in 10 MeV bins in $m(\pi K)$) seems unrealistic – the KLF meson scattering analysis note (on the collaboration website) suggests a $\Delta m/m \sim 5\%$ resolution on the πK invariant mass in the $K_L p \to (K_L)\pi^-\Delta^{++}$ process such that at $m(\pi K) \sim 1$ GeV, the bin size should be about 50 MeV. As this processes feeds into both isospins, presumably the bin size in each isospin will be limited by this process with the coarser binning (although in fact the $K_L p \to K^-\pi^0\Delta^{++}$ is shown as having a quite similar invariant mass resolution).

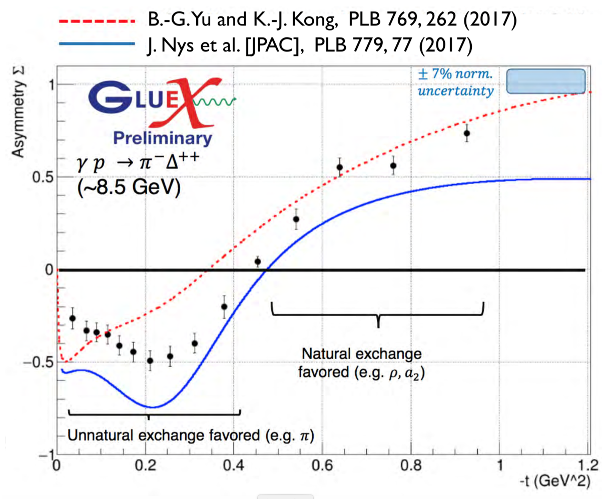
With the unrealistically small uncertainty on the fit as described above, the κ pole position resulting from the fit is slightly improved over the one obtained using LASS and other SLAC data (Fig 13). As pointed out in the text, the error on the pole position will not go below 3 MeV on the mass and 7 MeV on the half width even in the most optimistic projection for the KLF errors, as these uncertainties arise not from the πK data error, but from errors on the $\pi\pi \to K\overline{K}$ amplitude and the Regge parameterization of high energy scattering which enter into the dispersive analysis, neither of which will be improved in the KLF experiment. As such we can probably view the result in Figure 13 as the best case scenario for the experiment.

The experiment also proposes to measure $K\pi$ in $K_L p \to K\pi(p)$ where the recoil proton is not detected, in order to collect events at smaller values of $|t|$, and with improved $m(\pi K)$ resolution, at the cost of needing now to model neutral exchange in which a more varied set of t-channel exchanges can be present. Further, moving away from exclusive reactions typically incurs an increases in background contributions, and this would seem particularly likely in this experiment, where the beam momentum is not known precisely – the possible backgrounds are not discussed in the proposal. The proposal does not describe how the recoil proton and recoil Δ^{++} datasets will be used together in the extraction of phase-shifts.

Unlike previous iterations, the proposal is now somewhat more honest about the challenges associated with extracting the phase-shift from the actual measured data and the fact that these may introduce a so-far unconsidered systematic uncertainty. In particular the separation of just the pion exchange when data comes at relatively large values of $|t|$. Obtaining small values of $|t|$ is actually made somewhat worse by using the recoil Δ^{++}, as the kinematic $t_{\rm min}$ value becomes quite large, especially at larger values of $m(\pi K)$ and at lower beam energies.

On the topic of beam energies, one issue that has not been discussed is the impact of a spread in beam energies in KLF. LASS and the earlier SLAC experiments used monochromatic beams, so their phase-shift extraction done by studying t-dependences was expressed in terms of reaction model parameters evaluated at a single beam energy. In the case of KLF, they will have a spread of beam energies, and will have to bin in $p_{\rm beam}$, and do separate phase-shift extractions in each bin, with model parameters that one assumes will be $p_{\rm beam}$ dependent. The bins will presumably have to be quite broad to have enough events in each bin, and more pressingly to handle the very poor resolution on $p_{\rm beam}$ in the $K_L p \to (K_L)\pi^-\Delta^{++}$ case where only TOF is being used to determine the beam momentum (see Fig 30 [central] where for a 4 GeV incident K_L, the resolution will be close to 1 GeV). Whether this can be turned to the advantage of the experiment by demonstrating consistent phase-shifts extracted from different $p_{\rm beam}$ bins remains to be seen, and no model predictions are suggested in the proposal.

The use of the Δ^{++} rather than the recoil proton in the final state also complicates the phase-shift extraction as now there are *multiple helicity amplitudes* to deal with in a situation where the modelling is already complicated. It is not simply the case that at low $|t|$ simple pion exchange is dominant and other processes can be neglected as demonstrated in the GlueX data on beam asymmetry in $\gamma p \to \pi^- \Delta^{++}$:



If pion exchange was truly dominant, this quantity would take value -1, whereas one can see that the data is not close to that value even at small $|t|$, which is conventionally interpreted as there being strong 'absorption' corrections, for which there is not a single established and reliable theoretical description. Significant amounts of modelling will be required to describe this, and it is not clear from the proposal how the KLF collaboration plans to achieve this.

If one tries to view this proposal as though this were the first time it had been submitted to the PAC, the relatively low level of sophistication in simulation and in understanding of the basic reaction processes could be overlooked, and it is certainly true that the idea of running an experiment with a relatively high flux of K_L and a modern detector is not a bad one, even if only to collect data to support the existing $K\pi$ database (which is not large), and hopefully to add data at lower $K\pi$ invariant masses. Unfortunately this is now the 4th full PAC submission, and the proposal continues to fall down by promising significant improvement in the state of the art for things like the κ pole, without the support of careful study of the reaction process and realistic simulation of the expected events in the detector, and plans for the extraction of the phase-shifts. The reports for PAC 46 and PAC 47 stated that *"Demonstration of the amplitude extraction, using a complete detector simulation, would be necessary for approval"* and *"Simulations addressing backgrounds and the low $|t|$ region are necessary"*. Reading the (apparently incomplete) analysis note provided on the KLF website, it would appear that there has been some effort in the direction of simulating events and putting them through the detector simulation, and finding from these the possible resolutions and efficiencies. As far as adding in possible backgrounds and performing PWA, Sections 7.1 "Partial wave analysis for neutral exchange" (one paragraph) and Section 8 "Systematic uncertainties" (one paragraph), the studies appear to be at a rather rudimentary stage of development. Presumably it is this that motivates the choice to simply put a flat 5% systematic error on phase-shift points in lieu of detailed simulation.

It is also surprising, given that the experiment will need expertise in amplitude analysis to extract the $K\pi$ phase-shifts, that the collaboration has not formed a consortium, including theorists, that could be approached for assistance in developing the required formalism. The meson analysis note has only two authors listed – it might be suggested that assigning more resources to this effort may be required, and given that over 100 experimentalists and nearly 50 theorists have promised their support, this should be perfectly possible.