Analysis Review

"Study of Vector-Meson Photoproduction decaying to Multitrack-Final States using CLAS-g12 Data" (Z. Akbar *et al.*)

John Price (chair)
Susan Schadmand
Eugene Pasyuk

This paper reports on analysis done with the g12 dataset on several processes on the photoproduction of vector mesons. At this point, the analysis document is incomplete, but some preliminary comments can be made. The states quoted in the introduction are: $\gamma p \to p\varphi$, $\gamma p \to p\omega$, $\gamma p \to p\eta$, and $\gamma p \to K^0\Sigma^+$. Note that the latter two processes do not include a vector meson.

In this context, the observables claimed to have been studied are: the beam-helicity asymmetry $I^{\odot}$ for the process $\gamma p \to p\pi^+\pi^-$; the cross sections for the processes $\gamma p \to p\pi^+\pi^-(\pi^0)$ and $\gamma p \to pK^+K^-$; and the spin-density matrix elements for the ω and φ.

It should be noted that this document is not complete. The document is missing results for the φ cross section and for the $\omega \to \pi^+\pi^-\pi^0$ Dalitz plot. The latter is reportedly ready for inclusion in the next round of review. For the time being, we will work with the document we have, and assume that all of the items in blue text in the analysis note will be rectified in the next version.

The choice of event topologies on page 8 could be better motivated. Was any consideration given to the possibility of combinatoric background?

On page 7, it states that "Events were pre-selected based on the particles' identification number (PID), which was deterined during the cooking process." Does this mean that you accepted the particle ID supplied by the PART bank, and did not attempt to verify it?

On page 10, it appears that the method for dealing with tagger accidental background is to ignore any events which appear to have multiple tagger hits. Is this the best approach?

In the histograms on pages 16 and 18, the pull confidence levels seem to come from an old version of the kinematic fitter. Specifically, the rise in the CL distribution toward 1 is troubling.

The discussion of the comparison between the simulation and the data in section 3.7 has some issues. The comparison seen in the figures does not seem to be as good as the authors claim in the text; perhaps the trigger efficiency study results were not applied to the simulation?

In the discussion of the trigger simulation on page 28, what effect did this have compared to the effect of the fiducial cuts and knockouts due to bad detector elements?

On pages 45 through 48 are several invariant mass distributions for the $\pi^+\pi^-\pi^0$ system, but only every sixth distribution is being shown. While it is understandable that the authors do not want to inundate us with data, that is precisely what this level of review is for. All data planned for publication should be shown here.

The plots on pp. 84-89 are troubling. Note that the systematic uncertainty for the $p\omega$ analysis, shown in Table 20 on page 83, shows numbers around 5.9% for the sector-sector variation, and 2.4% for the fiducial selection. Figure 49 appears to be an attempt to quantify the discrepancy between g12 and g11 with a pseudo-Gaussian (the authors correctly state that this should not necessarily be a Gaussian), but the width of the peak is far greater than would be expected for the roughly 8% systematic indicated in the note. Additionally, it appears that several of the energy bins have marked discrepancies throughout the entire angular range, which needs to be addressed. Note, for instance, that in the energy bin 1.94-1.95 GeV, g12 is about 50% higher than g11 for all backward angles.

A similar effect is seen in the $p\eta$ cross sections. Several of the energy bins in Figs. 73-76 show good agreement between the g12 and g11 data sets; others have the entire angular range off by 30% between the two. This is argued away by Fig. 72, which shows that, on average, the agreement between g12 and g11 is "good". Because the two experiments have comparable statistics, more effort should be made to understand the discrepancy. Is it possible that the above-mentioned concerns with the simulation could be affecting this comparison?