RESPONSE TO COMMENTS
TO THE CLAS-ANALYSIS 2007-117

COMMITTEE: ANGELA BISELLI (CHAIR), VALERY KUBAROVSKY, AND CARLOS SALGADO

1. General comments

(1) The note needs much more explanations on the basic physics behind the analysis. e.g. Feymann diagrams, exchanges expected in the basic reactions, theoretical predictions for cross section, masses, branching ratios. Is the analysis checking any model in particular? In summary the introduction is too short and incomplete. How good is the mass value for the Φ^{--}? Should the search be done in a larger range?

Done. The analysis note introduction has been improved.

(2) The descriptions of the data reduction, PID, energy loss and momentum corrections are extremely short and incomplete. There are only 8 pictures with absolutely no details about the quality of the data analysis. I believe that the experimental group made a great job and made analysis very carefully. However it was not reflected in the written analysis note.

Done. The analysis note contains plots and descriptions for PID, momentum corrections.

(3) The committee thinks that to better believe upper limit for the Φ^{--} cross section, the authors must cross-check their analysis with a standard resonance. e.g. measure a cross section of a resonance among their data (using their analysis, acceptance corrections...)and compare it with previous measurements.

This is still in progress, but nearly complete. We still need to finish comparing cross sections from eg3 to g11 and g13 data. A separate note is being written.

(4) The committee would like to see more explanations about the basic event structure of the initial sample and biases (or not) introduced by basic cuts and triggers. e.g. what are the event multiplicities, kinematic distributions, and how these compare to the simulations distributions

Added more plots to the analysis note.

(5) Is there any significant enhancement about 1.92 GeV in figure 13b?

The significance of the peak at M=1.92 GeV is less than 95%. In the absence of any independent data supporting such a peak it is not usefull to dwell on such a "peak".

2. Specific comments

(1) abstract: "do not show any statistically significant enhancement" Was there any statistical test done to test the hypothesis of an enhancement? I guess it could be one below your 200 pb limit? Re-phrase.

Rephrased in the text. The upper limit us large now.

(2) Page 4, paragraph 1, line 13 Trigger bit 6 had very complicated structure as described in the note. I checked with Sergey the eg3 trigger file and found that in fact the trigger demanded just 3 sectors in coincidence. This may lead to the significance problem in the analysis due to the incorrectly calculated acceptance. It means that all cross section upper limits are wrong. This issue has to be carefully tested.

We checked with Sergey, and he told us that, indeed, the 2-sector part of the Level-1 trigger was removed from the trigger file. We performed the analysis assuming only 3-sector coincidence trigger. For both the real events and simulated events we required that there be three different sectors with the time-based tracks matching the TOF hits. The Start Counter hits were never used in the offline analysis, although the Start Counter was in the trigger. But we checked that for simulated events with at least 4 TOF matched time-based tracks in the fiducial regions of CLAS only $\sim 1\%$ *of the events do not have the threes Start Counter hits matching the track in the corresponding sector. This uncertainty is negligible with respect to the overall systemtic uncertainty, and therefore is ignored.*

(3) Page 4: The triggers seems to be highly selective. Have they been simulated? Do we understand their biases?

See the answer to the previous question. The three-sector requirement was applied to both real data and the simulated data. Therefore, the trigger selectivity is included in the acceptance corrections.

(4) Page 5: Why the cut on the Λ is done on both M_c and M_u (energy loss corrected and uncorrected invariant masses) and not just the corrected value?

The cuts discussed here are to enhance the efficiency for the skim. We tried to include as many candidates as possible into the skim without having really large skimmed files. These are not the final cuts.

(5) Page 6, paragraph 1, line 6 The cut +/- 1 ns for the proton and pion vertex time is very stringent. It is true that in average the TOF time resolution is about 200 ps (for protons closer to 300 ps). However it degrades significantly (up to 1 ns) with the increasing paddle number especially for protons. More over the average difference between TOF and RF moves from the zero value as well. It will be nice to produce the proton-pion time difference for individual proton paddles. It has to be investigated.

This cut has been modified. The new cut on proton-pion timing is described in the note. It is wider for slower and more backward-going protons.

(6) Page 6, paragraph 1 DOCA cut is used very extensively in this analysis. I think that it is necessary to investigate the DOCA distribution in different regions of the

phase space, for example as a function of the θ angle. Usually this distribution has long tails that may be difficult to reproduce in the MC program. it would be nice to make an analysis with MC events as well to make sure that the systematics is under control.

The sensitivity of the results were performed. The upper limit was estimated for different DOCA cuts keeping everything else the same. The study was documented on the eg3 wiki-page :

http://clasweb.jlab.org/rungroups/eg3/wiki/index.php/Study_of_the_dependence_of_the_results_on_DOCA_cuts

. The study shows very small sensitivity of the results when changing the cuts to 10 cm.

(7) Is it possible to have more details on how DOCA is calculated and defined?

DOCA is in common usage and adequately described in the text.

(8) Page 6, Equation (2): It is not clear how the parameters in the denominator of equation (2) were chosen (0.0017, 0.45 and 1.5). Are these distributions really have constant resolution in the total CLAS phase space? I don't think so. If you want to use equation (2) with constant resolution for all events you have to prove it. Usually it is not the case for CLAS. Are the terms of the χ^2 uncorrelated?

We chose what we considered reasonable values for these parameters based on the resolutions for these quantities. The sensitivity of the final results with respect to these parameters were studied, and the results are documented at:

http://clasweb.jlab.org/rungroups/eg3/wiki/index.php/Study_of_the_dependence_of_the_results_on_the_Lambda_Selection

. The study shows very small sensitivity of the results.

(9) Page 7, paragraph 2, Equation (3): T_0^{SEB} and T_{π^-} are calculated in the different vertex points. In principle it is necessary to translate T_0^{SEB} to the π^- vertex and only after that to make a correction. It is too wide from my point of view.

The cuts for PID are wide to account for this difference. See Fig 5 in the new analysis note and point # 10 below.

(10) Page 7-8 Figure 3: it seems that all the work done in adjusting the start time does not affect the pi selection cut at all? The cut seems really loose, what is the criterium for determine that cut value of $\pm$ 0.5?

This point seem to somewhat contradict the previous one. What we did was we made more narrow PID cuts. We did a systemtic study to check the sensitivity of the results on the PID cuts. The change in the final result ($M_{\Xi\pi}$ spectrum) when comparing the old very wide PID cuts and the new relatively tighter PID cuts is negligible.

(11) Page 8, paragraph 2: Two methods for the *eloss* corrections are used in CLAS data analyses: MC and eloss procedure [20]. Did you check that these two methods give close results?

We have only used the most common package "eloss" which has been checked extensively.

(12) Page 9, paragraph 2 I understand that the momentum correction procedure is described in the master thesis [21]. However it will be useful to include the short description and main results in the note as well.

A paragraph has been added to describe the procedure.

(13) Page 10, Equation (4). Same question as for Equation (2): how are the parameters at the denominator are chosen? Selection criteria includes DOCA and it's error 2.25 cm. Where is this number 2.25 coming from? Is it independent on the kinematics (for example the Λ or π^- momenta/angles)?

We chose a set of parameters consistent with the resolution of the detectors. Typically, we do not see much change in the result when these parameters are varied within reasonable limits. The choice of these parameters does not affect the final result on the upper limit at all, because these numbers are not used in the "conservative" event selection.

(14) Page 10, paragraph 3 I don't think that $\Sigma(1385)$ creates significant bgrd under the $\Xi(1321)$ peak. The mass difference is 66 MeV and the FULL width is only 40 MeV.

This is probably very subjective point. The applicability of the term "significant background" depends on the relative heights of the peaks as well as on the resolutions. Without applying the cuts for enhancing the $\Xi(1321)$ peak the number of events from $\Sigma(1385)$ is larger than the number of $\Xi(1321)$. But this is not a crucial point for this analysis, and the word "significant" was removed from the text.

(15) Page 12, paragraph 2 How many Ξ events do you have in the Fig.7? What is the main source for the event's loss? How did you choose the 1.5 cm cut? Where is it coming from?

The number of events added in the note, see Table 4.

(16) Page 12, paragraph 2: I am not sure I understand what it means to include all the possible pairs in the invariant mass. Doesn't that mean that you are double counting?

There are three π^- in the event. The best match of p,π^- for a Λ is selected using Eq 2. That leaves two π^- which can be matched with the Λ. "All combinations" means two combinations are made and entered into the histogram. Note that there is only one correct combination for the cascade and corresponds to the entries in the peak. The "wrong" combination contributes to the overall background in addition to true phase space events.

(17) Page 16, paragraph 2,line 4: The timing window 0.8 ns can be too narrow for the event's selection. This cut has to be investigated in details to make sure that there is no hidden loss. I remember from the g11 analysis that you need to apply about 20% correction factor if you want to use such s strong cut.

This cut has been studied and, indeed, was too narrow. The software timing window in this analysis between the tagger and the CLAS has been changed to ± 3.2 ns to be able not to do any corrections in the acceptance calculations.

(18) Page 17: $N_{sim} = N_{gen}$

done.

(19) Page 19: is the value of the acceptance for $\Phi^{--}(1.860)$ at 0.4%? Can we make any measurements with such a small acceptance!!! If I understand correctly the acceptance some of the other possible kinematics of the Φ^{--} are even lower?

he CLAS acceptance for multi track events is typically a fraction of a percent. See for example electro production of omegas (detecting e, p, pi+ pi- in fig 9.EPJA 24, 445 (2005)). And yes... we can make measurements at this level.

(20) Page 19, Fig. 10 It would be useful to present not only invariant mass distributions but also other kinematical variables (momenta, angles...) and compare data and MC.

We have included various kinematic distributions for reference.

(21) Page 21, table 1: What is the average acceptance? It is not clear why the acceptance for row #1 and for row # 4 are so different? As I understand this is the same reaction. The three-body phase space and four-body phase-space with a fixed-mass 1.862 are identical. Isn't it? It looks like a bug that goes to the estimation of the systematics.

There was indeed was a mistake. The acceptance in raw #1 and row #4 were different because of the way the acceptance in raw #1 was calculated. In the simulated channels where the $\Xi^-\pi^-$ originated from the $\Phi^-(1862)$ with a fixed mass we applied a cut around the mass of the reconstructed $\Xi^-\pi^-$ system. This cut was not applied in the case of the continuous mass distribution for $\Xi^-\pi^-$ system simulated flat in the phase space. Such a treatment was an incorrect one, because we are estimating cross sections (that upper limits) winthin a specific mass window of 20 MeV. The mass cut has been applied in the continuous mass spectrum simulation as well (raw #1 in Table 1 (Table 5 in the new version). The cut is on the difference between simulated and reconstructed mass of the $\Xi^-\pi^-$ system $|\Delta M| < \pm 10$ MeV. Applying the mass cut reduced the acceptance value at $M_{\Xi}^-\pi^- = 1.862$ GeV from 0.4% to 0.3%. It also reduced the slope of the acceptance dependence of the mass. As a result of this change, combined with other improvements in the analysis, the upper limit for the cross section increased to ~ 450 pb.

(22) Page 22: Could you provide the total integrated luminosity (e.g. in pb^{-1})?

The total photon-nucleon luminosity in the tagged photon energy range for the data used in this analysis is $\sim 50\ pb^{-1}$.

(23) Page 22, equation (11) I believe that you have to combine Equations (11) and (12) from the very beginning. The equation (11) looks very strange with the definition of "S" as the effective target surface. It is not clear what is effective target surface.

Done.

(24) Page 23, paragraph 4 It is not clear what is the reason for the reduction by 20% the four-track events when you put the start counter in coincidence with tagger.

I don't agree that uncertainty in CLAS are typically under 10%. You have to prove this statement. For example (from g11 experience):

- Rate dependence for the 3 prongs events - 19%
- Multiple hits in the tagger - 18%
- Trigger inefficiency - 15%
- 2 (!) ns tagger cut - 6%

This is 70% correction factor in total. There are no such a study in this analysis note.

The only way to check the systematics in such an extreme experiment (high current, 4-prongs events) is to find the reference reaction with known cross section and estimate this cross section from your own data. I don't think that we can release the cross section (or upper limit of the cross section) without such a test.

We performed checks with other reactions. The preliminary cross sections are consistent within 25%-30%. A separate write up is being prepared for this issue. We do not understand the source of 20% change either. We do this corrections assuming that there is no inefficiency when the start counter is not in coincidence with Master OR from the tagger.

(25) I would like to see more details and plots of all the systematics studies that were done (details about how the cuts were modified, comparison of spectra etc). I would like to see some plots that show the studies done regarding the acceptance and I would like to know if the authors have any insight on why the acceptance is so different for different events configurations.

If this refers to different models, it is quite clear that forward going particles (proton in particular) have low acceptance, and has a large effect depending on the production model for the events. With little theoretical (or experimental) guidance on the production model, this uncertainty must be included into the systematics.

(26) Page 24: the Figures showing the final spectra (e.g. Fig 13) should have a line or an arrow, indicating where the enhancement should be

Can be added for publication.

(27) Page 24: Fig. 13 and 14. Is it $\Lambda\pi^-\pi^-$ or $\Xi^-\pi^-$ invariant mass spectra? How does $\Lambda\pi^-\pi^-$ spectrum look like?

....Does Fig. 13 have a cut around the cascade?

Figure 14 a) (now Fig. 20) selects cascade peak. b) selects sidebands c) represents cascade π^- spectrum. Should be clear from the caption.

(28) Page 25, paragraph 1 What is the the tight cut applied to select the Ξ?

The values for the cuts are added to the text (see Tables 2 and 3 in the note).

(29) Page 25, paragraph 2,line 4 What is the definition of the sidebands?

The sideband limits are given in the text.

(30) Page 25: Did you fit the cascade mass spectrum? What are the values you got? Can you show the sideband selection in the cascade mass spectrum? Could you calculate a cascade cross section with the sideband subtraction and compare with previous measurements?

There is a wiki page describing the parameters in this procedure.

http://clasweb.jlab.org/rungroups/eg3/wiki/index.php/New_maximum_likelihood_fit_of_the_XiPi_signal.

The wiki-page for the comparison of the two methods is at:

http://clasweb.jlab.org/rungroups/eg3/wiki/index.php/Subtracting_background_in_two_ways.

But this procedure was used only as a cross-check for the more reliable sideband subtraction method, which is the method used in the further analysis of the data.

(31) Page 25, paragraph 2 Where are the results with the different cuts on $c\tau$?

We also performed a study to check the dependency of the results on the $c\tau$ cuts. We came to conclusion that the simulation does not reproduce the same distributions, and for that reason we removed the $c\tau$ from event selection for calculating the upper limits of the cross sections. This obviously increases the background, thus reducing the sensitivity of the experiment and increasing the upper limits we can quote. This is one of the reasons for the increase of the upper limits for the cross sections.

(32) Page 26, paragraph 3 The described procedure looks very complicated with absolute no supporting pictures. Can we look for example to the reconstructed Ξ live time? Does it come out right? I am not sure that the background was chosen correctly. Maximum likelihood method doesn't demand "reasonable" population in each mass bin. It works with any population. So it is not clear what is the reason for the different mass bin in two methods.

In fact there is more simple and direct check for the sideband subtraction method. You have to perform a one-dimensional fit of the $\Lambda\pi$ mass distribution for every $\Lambda\pi\pi$ mass bin with linear background. This is exactly corresponds to the sideband subtraction method. You can fix Ξ mass and width to make the fit more stable.

Added $c\tau$ distribution for Λ and Ξ, but in an earlier section. Note that for cascades, this distribution is dominated by background (signal/background 1/2). Therefore, the measured lifetime for this plot os smaller than one would expect for a clean Ξ^- event sample. We stress that this procedure is not used in the further analysis. It is simply used as a "sanity check" for the sideband subtraction method.

(33) Page 26, paragraph 3: Can we have more details about the fit? How many free parameters do you have? Are the fits stable? What is the quality of the fits?

See the previous point. ....

(34) Page 27, fig 15 It is hard to compare the red and black errors in this picture. Can you produce this picture with the same mass bin?

See the previous point.

(35) Page 28. Is it possible to have in the note a description of the Rolke Method in general and how the calculation was done in the analysis?

We show that similar results for upper limits are obtained using the Feldman-Cousins method. However, we added text describing the implementation of the Rolke method in this analysis.

(36) Page 28: 15.1 Did you use Fig 13- b)? Can you show your upper limit with the extra K? I cannot find what reference values did you use to calculate the upper limit of the Φ^{--} cross section. Which width and mass of expected signal in 15.1.2 (Gaussian above background) did you use? I do not understand why "for comparison here we ignore the acceptance and background uncertainties and assume 100% acceptance/efficiency"? I guess Feldman-Cousins can not handle this but you are using the Rolke et al. method at the end?

We did not estimate the upper limit with an extra kaon, the upper limits do not have to be the same. The Feldman-Cousin method by its nature did not include *uncertainties on the acceptance and efficiency extraction. Therefore, the comparison between the Rolke and Feldman-Cousins methods did not include these uncertainties. The systemtic uncertainties reduce the sensitivity of the experiment, and thus the limits obtained. Therefore, these uncertainties were included in the Rolke method, which was used for the final results of the paper, However,the effect is not large. Since the original analysis note we also implemented an approached based on the Feldman-Cousins construction which also includes the background and acceptance uncertainties. Comparison of the two methods gives us very consistent results.*

(37) Page 31: Fig 17 It is interesting that the upper limit gets better (more restricted) as the mass increases, and the data statistics decreases. I guess it is because of Fig 11 (acceptance). How this curve differ for the different kinematics of table 1?

This is because the acceptance typically must increase as the phase space increases. This is a general kinematical feature as we go above threshold. Also the statistical uncertainties decrease at larger mass values. But this trend for the acceptance was reduced after applying the cut on the difference between reconstructed and simulated masses for $\Xi{-}\pi^-$ *system (see item 21).*

(38) Page 32, conclusion There is only one comparison with the previously estimated upper limit (HERMES). Can we compare CLAS limit with other experiments? How does it look in the variety of the previously published data? Do we have any theoretical estimation of this cross section? Can we make any conclusion about the existence of this particle or its total width?

HERMES has the only result on photoproduction cross sections. There are some early theoretical estimates with large uncertainties (see the experimental proposal) which will be discussed in the publication. Conclusions on the existence is related to the quoted upper limit on cross section times branching ratio. It total width can be related to the cross section in a model, but this is not too interesting for a limit.

3. Grammar/typos comments

(1) Page 3, paragraph 2,line 2: willdenote → will denote
(2) Page 4, paragraph 1,line 5: were → where
(3) Page 4, paragraph 1,line 7: wide → in diameter
(4) Page 9, paragraph 2,line 2: remainsin → remains in
(5) Page 18, 5th line from the bottom: f → of
(6) Page 21, paragraph 1,line 1 give → given
(7) Page 23, last line assign → assigned
(8) Page 25, paragraph 1 evaluate → evaluated

All done. Check (5) should be sigma.