

Dear Committee Members,

Thank you for your comments and questions. I apologize for the delayed answer. During this period I have changed the background subtraction method and used a different MC based on Titov-Lee theoretical model to calculate the acceptances. Also, some cuts and corrections applied to data were modified. After all, we obtained a better agreement between CMU's and current analysis results.

In the current version of the note we present the preliminary results of the Phi meson cross sections in photon energy interval 1.6 -2.6 GeV. The energy range is limited because of the MC generation range. We have some problems related to CLAS RECSIS package that must be solved, and the analysis energy range will be extended to 3.6 GeV.

I will send the current results comparison to world data plots during the following week. The systematic errors will also be overlooked and modified, as the analysis results will be extended in energy range.

General comments on both analysis:

The investigation of phi photoproduction via its neutral decay is a very interesting and important subject. The fact that CLAS now has very detailed data on phi photoproduction via both charged and neutral decay modes, with the latter being investigated by two groups independently, is a strong confidence builder towards a future publication.

it is imperative that the two groups from ODU and CMU work together to produce comparison plots to check the consistency, for both the differential cross section (which as I understand was presented in June this year) and the Spin Density Matrix Elements, and incorporate it into the analysis note. We leave it to the analysis groups to work out the details on how to proceed. Of course, we expect to hear from you earlier than it took us to get back to you.

General comments on the ODU analysis:

The committee believes that the analysis note presented to us is quite concise, and frankly did not contain enough material. We find some of the analysis techniques that were used to be questionable. In particular, the side-band subtraction method used for the phi yields seems too simplistic. The estimation of the systematic errors also is too simple, and incomplete. Other methods of extracting the yields should be tried. The committee would also like to see the comparison between other world (published) data than leps, in particular g6a. There is also no discussion on whether the simulation that was used to derive the detector acceptance can reasonably reproduce the data.

## Comments from Raffaella De Vita

### Comments on the ODU analysis:

- The analysis note is quite concise and there are many point that I find quite obscure. I have tried to put down the most relevant question but I may have more to ask after this first round.
- The points of this analysis that look more crucial to me are the following:
  - the yield extraction and background subtraction: I don't think the technique they adopted is good enough.
  - the estimate of systematic errors is too simplistic.
  - the comparison to world data is limited to leps only while they should compare with the CLAS published data at least.

### section 3.1:

- you mention the PART bank was used in the analysis and you mention also that GPART was set equal to 3. The PART bank is the output of the PID particle-identification scheme, but GPART is the variable in ntuple 10, which tells how many particle are in the EVNT bank, which is the output of the SEB particle-identification scheme. In conclusion which PiD did you use? PID or SEB?
- which files were used in the analysis: bos files or ntuples? if bos files, which ones?
- how were the minimum momentum cuts for pion and proton determined?

I apologize for this. Yes, the particles were selected from the data according to the particle ID assigned by SEB. I have used the ntuples.

The minimum momentum for the protons was selected arbitrarily in the cut to eliminate CLAS detection problems of low momentum protons ( this cut removes about 3 % of protons).

The minimum momentum cut for the pions eliminates a very small fraction of very low momentum pions only ( about 0.4%).

### section 3.2.1:

- can you say which set of tagger energy corrections were used? Mike Williams' or Valery's? I suggest to add anyway a reference.
- pg. 25 line 3, I think the reaction used to derive the correction was  $\gamma p \rightarrow p \pi^+ \pi^-$  with all final state particles detected.
- from this section I have the impression that Mike Williams tagger energy correction and Valery's run dependent correction were used: is this true? this should be anyway stated very clearly, which is not the case right now.

Both the tagger energy corrections and the momentum corrections are Valery's. I have used his description for the corrections from his analysis note: Experiment g11: Search for the  $\gamma p \rightarrow K^0 K^+ n$  (2005), which I found on his g11 Participants page:

[http://www.jlab.org/Hall-B/secure/g11/valery/note/valery\\_note.ps](http://www.jlab.org/Hall-B/secure/g11/valery/note/valery_note.ps)

section 3.2.2:

- I don't understand the last sentence of this section: what are you referring too?

My sentence indeed is not organized well. I mean that eloss includes only correction for the energy loss of the particles in the target material, and the corrections for other factors changing the momentum of the particles are taken care of with momentum corrections. I will remove the sentence.

section 3.3:

- this section explain how single-particle efficiencies were derived. However it is not clear what was the purpose of evaluating SPEs: were they used to determine fiducial cuts or were they actually used to correct for detection inefficiency?

It is used for both. First, it is used to correct for the detection inefficiencies. Second, it is used to place fiducial cuts on very inefficient regions of the detector as a 40% efficiency cut on events (both to data and reconstructed events in the acceptance calculation) and broken scintillator paddles removal.

section 3.4:

- the number of bad scintillator paddles is quite small, definitely smaller than the number of problematic paddles that were identified during the calibration that are indicated in the calibration database (the list used in the CMU analysis is more consistent with the original g11 one).

All the bad paddles are removed in the new version of the analysis.

section 3.5:

- can you show distribution for data and MC?

Included.

figure 28:

- can you include a fir of the peak and show on the plot the cut range? The same for M\_KL

Done.

section 3.7:

the background subtraction method that is used looks to simplistic to me. Because of the proximity to the KK threshold the background has a complicated shape and the assumption of estimating it using half a side band is arbitrary as arbitrary is they way used to estimate the systematic on it.

The background subtraction method is changed. The new method is described in the new note in detail.

section 3.8.1:

- how many iterations were performed to finalize the acceptance? can you show comparisons of data and reconstructed MC spectra? was gpp used? if not, why?
- what kind of kinematic dependence was chosen for the event generator? (for example was the  $t$  dependence generated assuming an exponential slope?)

GPP was not used due to the fact that we are extracting the efficiencies of the detectors from the data directly.

Those efficiencies also take into account the bin migration.

section 3.8.2:

- can you show plots of momentum vs.  $\theta$  and  $\theta$  vs  $\phi$  for the different particle type indicating the position of the fiducial cuts that were used? was any minimum momentum or minimum angle cut applied?
- was the acceptance estimated as a function of  $E_\gamma$ ,  $t$  and  $\cos\theta$ , or was it integrated over some variables? it is not clear from the text.

The fiducial cuts were applied as a combination of 40% efficiency cut both for the acceptance calculation and for the yield extraction, and bad scintillator paddles cut. The efficiencies were obtained for different momenta and for every  $\theta$  and  $\phi$  angles and our fiducial cut is based on efficiencies, therefore it is dependent on the momenta of the particles.

The acceptances were individually obtained for each distribution that was measured (for  $t_{\min}$ ,  $\cos\theta_{\text{hel}}$ ,  $\cos\theta_{\text{gj}}$ ,  $\cos\theta_{\text{cm}}$  for different energy bins).

section 3.8.3:

- if I understand correctly, momentum corrections for the proton were evaluated for the MC data. Did they apply  $e_{\text{loss}}$  too or not? the kind of shift they observed looks like a problem of energy loss and as far as I know in g11 this can be easily fixed applying the standard  $e_{\text{loss}}$  correction.

Yes, it is basically a correction for the energy loss. Only, I had obtained the corrections myself here, instead of using the  $e_{\text{loss}}$ .

In the newer version of the analysis the standard energy loss ( $e_{\text{loss}}$ ) corrections are used.

section 3.8.4:

- where the variance in the target density comes from? was the fluctuation in density included in the systematics (it is negligible but they should say it anyway)?

This is the variance from run to run, because the pressure and the temperature used to find the density changed during the runs.

section 3.8.5:

- I know that they applied trigger efficiency correction as everyone else in g11 but there's nothing about it in the note. In addition since they use a timing cut between the target and start time they should also apply multiple hit corrections.

In the new version we applied the multiple hits correction due to the 2ns timing cut: we are losing about 3% of events because of this cut.

section 4.1:

- pg. 69, third line: are they cutting bins with efficiency less than 0.1%? is that what they mean here?

The cut is for the events with acceptances less than 0.1% total (not the acceptances of the protons or pions, but the final acceptance calculated for the particular observable, as  $t-t_0$ ,  $\cos\theta$ , etc.). 0.1% is for the  $ACC_{proton} * ACC_{pion+} * ACC_{pion-}$ . (10% for each particle).

figure 47 and following ones:

- are the error bars statistical only?  
- they should include the comparison with the world data for phi photoproduction not necessarily only for the neutral channel decay.

The error bars do not include the statistical errors only; they also include the acceptance errors, flux errors and the efficiency errors.

The comparison plots are included in the new version.

section 4.3:

- the estimate of systematic uncertainty is far from complete. I don't think the estimate of the error on the yield extraction makes sense and there's no error on the normalization.

The statistical errors may be overestimated. They will be overlooked and corrected.

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Comments from Valery Kubarovsky

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Figure 3. Where is Pomeron exchange? Where is  $K^+$  exchange?

Corrections included.

Figure 4 looks differently.

Figure 3 is intended to show the two different reactions with the same final decay products. Figure 4 shows the possible mechanisms of Phi meson photoproduction.

Page 22. Do you take into account the KS time of flight? The same question to CMU.

KS may escape the SC array.

Yes. It is included in the MC.

Page 24. There was also a requirement of NO neutral particles in the neutral event set.

This is very dangerous cut. There is KL meson in the event. It is neutral and interacts with EC with high probability. I am not sure that MC will reproduce this effect correctly.  
You have to remove this cut from the analysis.

The cut is removed.

Page 33. Detection efficiency

I am not sure that this method of calculating the efficiency is correct. For example it doesn't take into account the finite resolution of CLAS detector. For example you calculate the missing particle inside the fiducial volume, on fact it may be out of the fiducial volume. It is important to compare the data and MC efficiency and apply as a correction the ratio of  $\text{eff}(\text{DATA})/\text{eff}(\text{MC})$ . This was done in the CMU analysis I believe.

Our efficiency calculation is accounting for CLAS resolution. The reconstructed particle must be located within some angle range from the predicted particle. If the particle is missing in the non-fiducial region due to the resolution, that affects directly on the efficiency and if that effect is too big, it will be cut out in the acceptance due to the efficiency cut.

The MC efficiency does not 100% correspond to the real CLAS efficiency. It is based on some assumptions and GPP approximations.

Page 48. Background separation.

It is VERY strange estimation of background. And it is very strange calculation of the systematic error due this factor (page 83). The correct way to estimate this factor is to fit the distribution by right BW (see CMU note) and from this fit determine the factor.

We have changed the background estimation and subtraction method in the new version. The systematic uncertainties must be revisited.

Page 49. Formula 59.

What is  $\epsilon$ ? If  $t = t_{\min}$  the function goes to infinity.

I apologize for the mistake. The formula is corrected.

Formula 60. What is  $x$ ?

$x$  is the energy.

In the new version of the analysis we use different MC. The description will be included in the note.

Page 3.8.2 Again do you have correction due to finite live of time of KS?

Yes.

Page 83. What is the systematic error due to target proton vertex? I don't think that in the exclusive reaction we have contamination from the target walls.

The cut is removed in the new version.

Suggest to analyze the empty target run and show the  $z$  distribution for the events after ALL cuts. This systematic error 3.5% may be incorrect.

These statistic errors may be overestimated. They will be overlooked and corrected.

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The systematic errors will be overviewed.

Comments from Michael Dugger

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Page 1, paragraph 1, line 11

Comment:

States that almost all measurements show local enhancement between 1.8 and 2.4 GeV. The only data set that shows an enhancement is LEPS. The Saphire data looks flat with respect to energy. The BONN data is a single point.

Yes, that is true, the word "Almost All Measurements" is not appropriate in this case. I actually was considering David Tedeschi's previous analysis (that I was referring to before) and the recent CMU results, but I guess I cannot refer to them at this point. It is difficult to do good comparison with limited available data.

Page 24, paragraph 1, line 3-4

Comment:

The requirement of "no neutral particles in the final events set" has the potential to lower the cross section due to accidentals.

The cut is removed.

Page 24, section 3.2

Comment:

States that "standard g1 energy and momentum correction



packages" were applied. The energy correction used is not "standard". The energy corrections applied to the g1c, g8b, and the CMU g11 data, removes the three bump structure seen in Fig. 16. The ODU approach does not eliminate the three bump structure.

The applied corrections were obtained by Valery Kubarovsky and were used in some published CLAS analysis. If you look at the scale, the structure varies within 0.4%. It should be a very small effect.

Page 28, paragraph 1, line 7-8

Comment:

The statement,

"This correction does not include the energy loss due to the flight of the particle in magnetic field", should be

removed. There is no expectation for there to be any energy

loss due to the flight through the magnetic field.

I mean that eloss includes only correction for the energy loss of the particles in the target material, and the corrections for other factors changing the momentum of the particles are taken care of with momentum corrections. I removed the sentence.

Page 28, paragraph 2, line 5-6

"In this case the corrections for the theta and P variables

were very small and could be neglected." This needs to be

shown.

I have not obtained the corrections myself and have used Valery Kubarovsky's corrections and his description about this corrections in the note - Experiment g11: Search for the  $\gamma p \rightarrow K^0 K^+ n$  (2005), which I found on his g11 participants page:  
[http://www.jlab.org/Hall-B/secure/g11/valery/note/valery\\_note.ps](http://www.jlab.org/Hall-B/secure/g11/valery/note/valery_note.ps)

Page 31, Fig. 20

The figure needs to be better labeled, so that the reactions are clearly shown. Also the plots should include which plot is "before" and which is "after".

Modified.

Page 32, Fig. 21

The text on the plots is not explained (e.g. " $n = 0.4 \text{ MeV}$ "?).

Explanation is included.

Page 38, Section 3.4

Comment:

"For this reason an efficiency cut of 40% was applied both to data and generated events." does not show in Fig. 25.

Figures 25, 26 and 27 belong to subsection 3.3 "Detector Efficiency Corrections". Just I am not able to fix them in the correct place (some LaTeX problem). I apologize for it. I will try to fix that.

Page 42, Section 3.5

Comment:

"So only the region  $Z \in [-8 : 28]$  of the interaction vertex was kept in the analysis. This cut was applied also to the generated events during the acceptance calculation."

We need to see that the Monte Carlo z-vertex distribution (before the cut) matches the z-vertex distribution of the actual data. This cut has the potential of causing more problems than it solves.

The plot will be included in the section of MC description. The cut is removed in the new version.

Page 42, Section 3.6, Paragraph 2, line 2-3

Comment:

"...peak with some small background." The background under the peak in Fig 28. is not a "small" fraction of the events shown in the figure. Under the center of the peak, there might be 10% background. Away from the center, the background becomes a larger fraction. This is probably just an issue of how the relative term "small" is being used. It might be better to use more precise language.

Modified.

Page 43, Fig 28

Comment:

Axis label does not make sense.

Corrected.

Page 44, Fig 29

Comment:

" $M(\pi^+ \pi^-) < 0.015 \text{ GeV}/c^2$ " is not possible. The minimum possible mass of  $M(\pi^+ \pi^-)$  is  $M(\pi^+) + M(\pi^-)$ .

Corrected. It should be  $|M(\pi^+ \pi^-) - 0.49765| < 0.015 \text{ GeV}/c^2$ .

Comment:

the superscript "0" on eta and omega is not needed.

Removed.

Page 48, Section 3.7

Comment:

Performing a side-band subtraction when there is only one side, is a dangerous thing to do. It would be better to fit the distribution with a function = peak + background, and then remove the background.

The background removal method is changed in the new version. The description is included in the note.

Page 49, Section 3.8.1

Comment:

"Also, during the cooking some events can be lost." You need to be careful here. It is probably best if you just remove this statement. Otherwise you need to talk about how gflux compensates for the lost events.

Removed the sentence.

Page 50, Section 3.8.2,

Comment:

"Finally, RECSIS reconstruction program was used to cook GSIM output, like experimental data was cooked. Some cuts and corrections were applied to the reconstructed events:

" Energy loss momentum corrections,"

The momentum corrections used for the actual data (post-eloss) should not be applied to the Monte Carlo data. Please be clear about what corrections are the same, or different, for Monte Carlo vs. actual data.

The Momentum corrections applied to MC are actually for the energy loss ( eloss corrections). In the new version they are changed to standard CLAS eloss package corrections.

Page 51, Paragraph 2:

"To eliminate the bin migration problems, the number of energy bins and  $t_{\text{min}}$  bins to calculate the acceptances for the cross section  $t_{\text{min}}$  distributions were chosen to be twice the number of bins used for the data. For  $\cos\theta$  distributions the number of energy bins was also taken two times greater than the number of

bins for the data, and the number of angle bins was the same as for the data. For azimuthal angle distributions the number of energy bins and angle bins were the same for acceptances and for data."

Comment: I think there might be a communication problem here.

As written, the bin migration problems will be the same regardless of the finer acceptance binning. I must be missing something here. Was the acceptance parametrized in some way?

If the cross section changes fast, then the effect of bin migration can differ depending on the slope of the distribution. The bin migration effect can differ for different bins. As the  $t$  distribution is a rapidly decreasing function at low  $t$  and is becoming flat at some value, the bin migration in the slope-changing region may have some effects. To minimize the effect and be on a safe side we decreased the bin size.

Page 52, Section 3.8.3

Comment: It looks like the energy and momentum corrections applied to the Monte Carlo are different than that applied to the actual data.

Question: Is the corrections discussed in this section the only corrections placed on the Monte Carlo data?

Comment: You need to show that these corrections are not needed for the actual data. For the actual data you only have a correction in the phi variable. You have no momentum dependence. The Monte Carlo study says that there can be a large momentum dependence in the correction.

Comment: If GSIM is an accurate description of CLAS, and there were no momentum corrections beyond  $\epsilon_{loss}$  for the actual data, then the Monte Carlo (without any corrections) would mitigate the bin migration. You simply would take the acceptance as  $N_{seen}/N_{thrown}$ , where  $N_{thrown}$  uses only generator kinematics and  $N_{seen}$  only uses reconstructed kinematic information. By making additional corrections to the Monte Carlo reconstructed kinematics you do not get the benefit of this "self-correcting" behaviour.

These corrections in Monte Carlo are changed with standard CLAS  $\epsilon_{loss}$  corrections. There are no momentum corrections applied to MC.

Page 53-60, Fig. 34-41

Comment:

When the acceptance is low, change the scale of the plot.

There are times when you plot acceptance that has a maximum

of about 0.01 but the scale goes all the way up to 0.12. This "squishes" what is seen, and makes the acceptance appear much more smooth than it actually is.

Done

Page 62-64, Fig. 43-45

Comment:

Need axis titles and tick marks.

Done

Page 65, Section 3.8.5

Comment:

Need to discuss the Flux correction the Genova group derived.

Page 69, Last Paragraph

"The differential cross section still has some local structure at about 1.8-2.4 GeV as the one observed in , "Charged" decay mode, although in this case the peak is smaller."

Comment:

We need to see a comparison plot.



Page 83, Paragraph 1

Comment:

How is the systematic error of 1.3% found?

Page 83, Paragraph 2

Comment:

How is the systematic error of 3.5% found?

Page 83, Paragraph 3

Comment:

How is the systematic error of 5.7% found?

The systematic errors will be reviewed.

Minor issues:

- \* There too many spelling errors to list them all.
- \* The figures are out of order (e.g. Fig 3. comes before Fig 2.).

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Comments from Lei Guo:

The note seems to be put together hastily (or maybe you have a new version now?), and I find the yield extraction to be unjustified, the systematic errors inadequately addressed. Whether the acceptance is derived correctly is questionable, at least judging from the way it is described in note in its current stage.

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1. Page 1 Para 1, last sentence : "Lambda and phi". It seems a lot of times when Lambda is used, the author really meant Lambda(1520). This occurs throughout the note. Please clarify one way or the other.

Done.

2. Fig. 2. What is ref[11] standing for? What energy is the plot generated for? I have taken this plot from Gagik Gavalian.

I have changed the plot. It is for all energies. There is no energy cut. The reference is included.

3. Please show the 1d plot of pKs as well as pKL invariant mass spectra. Although there is no Lambda(1520) in the neutral mode. What about the other excited hyperons (Sigma\*'s)

The intention of this analysis was not the missing hyperons or other particles identification but purely the Phi cross sections extraction for the 'neutral' mode. Even if there are some other coexisting particles that can interfere with Phi, they should not be as prominent as Lambda(1520) in 'charged' mode.

I will include the 1d plots of pKs and pKL invariant mass spectra.

3. Fig.3. These diagrams do not seem to be referenced/ Besides, where is pomeron exchange in the left plot? There also should be excited Sigma\*'s on the right plot.

Corrected.

4. Page 21, para 1, last sentence "still open in this topic ...": could you be more specific?

The sentence is modified.

5. Page 24, sec 3.1 "no neutral particles ...": how could this be correct? In addition to non-decaying Klong, the Klong also decay to neutral particles (3pi0 or pi+pi-pi0)

with  $\pi^0 \rightarrow 2\gamma$ ? Can you provide evidence that your simulation is reproducing this?

The cut is removed.

6. page 2, sec 3.1, "one photon with time close to the event start time": How often do you have more than one photon within this cut? Do you apply any corresponding correction factor? Is there an associated systematic error addressed?

It is about 3% of the events. The correction for this loss is included in the new version.

7. Where did you get the fiducial cuts from? Is it from the g11 group? Table 1 also seem to have a very small group of dead paddles. This should be double-checked.

The fiducial cuts were applied as a combination of 40% efficiency cut both for the acceptance calculation and for the yield extraction, and bad scintillator paddles cut. The full list of bad scintillator paddles is included in the new version.

8. Sec 3.5, Vertex cut. Is this cut necessary? Can your simulation reproduce your data with expected signal loss? Is there any s/b ratio improvement when you do this cut?

The cut is removed.

9. sec 3.6 second Para. When the  $K_S$  KL cuts are varied, do you see any signal-loss? Can you overlap the spectra before and after cuts?

Plot included.

10. sec 3.8. Did you use gpp to smear the detector? If not, how can you trust your acceptance?

I am using efficiencies calculated from the detector data directly. Based on the obtained efficiencies I am cutting out the acceptance regions, which have low efficiencies. I can trust my acceptance because I am using the efficiencies calculated directly from the data, not from GPP.

11. sec 3.8 Fig. 33: I'm confused by this sketch. In the text, it was stated you iterated this procedure "to obtain good agreement between the generator and the data", but the data was never entered in that flow chart. Please substantiate the "good agreement" with the comparison between data and simulation for various kinematic variables (momentum, angle, etc) as well as other observables such as invariant mass.

Sorry for the misleading scheme. You are right, it should also include DATA . We have used a different MC in the new version of the analysis. The comparison plots will be included.

12. Sec 3.8.2, page 50, first bullet, "Energy loss momentum corrections", what is this? Did you apply momentum correction to the simulated data? That can't be right.

In the new version there is eloss package applied.

13. Sec 4.3 Systematic uncertainties: This section is way too simple. For example, I cannot believe there is only 1.3% error on the yield extraction. None of the three discussed systematic errors were explained in enough details. For example, for the vertex cut, the note states that different cuts were used, but did not explain how the error was derived/calculated? Please rectify this and include relevant plots.

The systematic errors will be reviewed.

14. Please include a table of all kinematic cuts for you data selection.

Done.

15. Please include a table of all correction factors.

Done.

16. Can you overlap the theory over your data for the SDME's?