General comments for the response to the committee's first round comments:

We thank the author for the detailed response. Overall, the committee is impressed by the amount of additional work the author has put into answering our questions. However, the committee remain unsatisfied by a couple of key issues.

The most important issue is the discrepancy between the g11a and g6a results. The committee do not believe "it seemed that he (Laget) was not too perturbed by the scale factor difference" addresses our concern. To be specific, we would like to see the difference (scale factor) to be determined and presented quantitatively, and suggest that the yield extraction in relevant hight W bins (Fig. 9.14) from both results to be compared. If the g6a yield extraction is the culprit, it should be demonstrated to be so.

The second issue is the remaining discrepancy at forward angles between the ODU and CMU results. If both ODU and CMU agree, then we are more likely to believe that the g11a results are more robust than what g6a had. Our understanding of the committee charge is to review both analysis towards a joint publication, therefore, it is important that the two groups communicate and work together to resolve the issue. To make our request to be more specific, we would like to see the comparison of yields, acceptance in particular bins (not many, just a few) quantitatively. It seems this would be easier done in the CMU framework (to rebin), so we would like to see this comparison to be done soon.

The third issue is that the committee find the answers to our questions at times to be not direct, and at times seem to be dancing around our concerns. For example, the 1d projection and pK- and pKs/pKL invariant mass spectra were requested, but only M(p, K-) vs M(K-, K+) scatter plots were presented, and the concern was not addressed.

The fourth issue, which is more specific, but is worrying and thus quite a general concern. The committee noticed that , in the CMU_extra details_r1_0.pdf, the bump in the differential cross section at 2.1 GeV is now completely gone (fig. 3-4) except for the very forward angles. The agreement between the charged and neutral modes was much better, as a result of the charged mode had changed so much. The new results and the better agreement between the two topologies are more reassuring but at the same time is quite worrying to see how the fit procedure used to extract the yield can lead to very different results if not properly constrained.

We hope the remaining concerns/questions are addressed and answered in a timely manner, the answers to our second round comments more direct, and on-target, and the committee shall work with the authors to insure the publication will happen soon.

Comments from committee members:

Michael Dugger

Specific comments:

My comment from round 1:
"The different run conditions could be helpful in determining the systematics."
was responded to in the form of a question.

My comment from round 1:

"Would like to see an estimate for how much Lambda(1520) is
still leaking into the signal."
was not answered. You do state that "The fact that the
Lambda(1520) does not completely go away is disturbing." It seems to me that the Lambda(1520)
persistence could be useful in estimating how much is leaking into the signal.

My question from round 1:
"...yield a signal loss of about 1.5%, in tune with
what we had found for the data. Question: How do you know this?"

Your response:
"For a 1% confidence level cut, you roughly expect to loose about 1% of good events as well. While
testing the kinematic fitter, I also showed specifically that this is what was happening. For the data, the
relevant plot is Fig. 1.4 where you can see that we are roughly loosing 1% good events, and here I,m
saying that the same thing is happening for the MC."

My new comment:
For a 1% confidence level cut, you would lose 1% within statistics. You did not give an uncertainty on
the 1.5% number. Having a loss of 1.5% is a fifty percent increase over the expected amount. This
probably is not a big deal. To make sure that this is not indicating something seriously wrong, could
you make a 10% confidence level cut and see how much signal is missing from the MC. If a 10%
confidence level cut results in 15% signal loss, I would be very concerned.

Valery Kubarovsky

1. My main concern was the PID for forward going kaons. In general the total loss of the events due to
PID may be small (say 5%), however in the region of low t the kaon loss may be essential. We
struggled with this in the L1520 photoproduction doing special PID for forward going particles.
Nevertheless I am satisfied with the reply to my PID questions. The t-distribution has no drop-off near
t=tmin. It is a good sign.

2. I did not get the reasonable answer to the question concerning so called Live Time Correction. The
reference to Mike Williams paper is not satisfactory. g11 group pointed out that this correction is
connected with the beam current. When the live time is high enough (>80%) the dead time just
proportional to the beam current (read trigger rate). Accidentally the correction is almost the same as
the dead time. I believe that we have to insist to change the name of this correction. The result will be
the same. The only difference is in the systematic error. CLAS has no systematic error in the dead time.
It is very small.

The current dependence of the track reconstruction was understood in dvcs experiment. It was shown
that background hits decrease the track reconstruction by 15% due to the high occupancy in the drift
chambers. It is just the reply to the author's comment that no one really knows why there is a current
dependence. The g11 experiment is very high luminosity experiment that has compatible DC
occupancy to the dvcs data.

3. We did not get the answer to the question about the discrepancy between g11 result and previously
published CLAS data from g6a. There are a lot of words but no attempts were done to resolve this

important issue.

4. I don't think that we have to chose what analysis is better: ODU or CMU. It is for these two groups to come up with combined physics results that we are going to publish. All discrepancies must be resolved between two authors.

5. We can't describe the bump in the cross section by interference term if you cut out the L1520 region. Am I wrong? It is something different. It may be MC generator for example. This question has to be investigated in more detail. The author wrote that he doesn't understand this issue. But it is not the right answer I believe.

Raffaella de Vita

The comment numbers refer to those in the first round documents.

- my comment #6 on the 5% event loss: in his answer Biplab confirms that the loss depends on the energy. I would like to see a plot or at least to have more quantitative information on how the event loss varies within the kinematics. I think it is important that the loss does not exceed reasonable values close to the edges of the kinematics. I also would like to see the same for MC.
- my comment #12 about the minimum angle cut for positive and negative particles: I think it is not correct to apply the same angle cut for pos and neg. particles. This has nothing to do with Pid but simply with tracking.
- my comment #13 about fig. 1.21: I don't understand why it is not possible to redo a p vs. theta plot from this analysis. Are the fiducial cuts built in the skim? If not it should be possible to do a momentum versus theta plot before and after the momentum and angle cuts.
- my comment #18 on fig. 1.27: sorry for the misprint. I meant S between 2. and 2.2 (in the region of the bump).
- my comment #21: I don't understand the answer: basically he is saying that the W dependence of the systematics is a reflection of the t dependence. Then why not evaluating the dependence of the systematic from t instead of W? Also the quote sub percent variation in a 10 MeV bin doesn't make sense to me: I don't think one can relate W and t so easily.
- my comment #23 about the systematic on the yield: I'm not satisfied by the answer. The fact that the errors are summed coherently when integrating is not enough to decide that it is not necessary to evaluate proper systematic errors. If this then are found to be negligible than it's another story. As shown in the pdf document with the "extra details" comparison of the yields for different values of Nc and different background function have been performed. From these one can easily evaluate a systematic error. This should be done bin by bin, as the fact that the differences are in general small does not imply that they cannot be significant in some specific kinematics. I think this is a crucial point also for the comparison with the ODU analysis and with the world data.
- my comment #24 about fig. 5.1: ok for the explanation about the errors on the MC not being shown. However I still would like to see a data-mc comparison with more reasonable binning.

Lei Guo

I am satisfied by the answer to my specific questions except the following (The comment number refers to those in Rd1 comments)

Comment #3: The effect of hyperons in general. The 2d plots of M(pK-) and M(K+K-) in Fig.5 of the answers to Rd1 comments are difficult to judge from, whether there is anything around 1.8 GeV. Why not provide the 1d projection as well? And pK0 and pKL should also be checked for excited Sigma's.

Comment #5: The part of "why doesn't phi sho up in the rejected events,..." is answered, thanks. However, the other part of m y original question was whether the 10% of signal loss was taken into account as a scaling factor in the cross section calculation. Or is this totally reproducable by MC? If not, how is it treated and what is the systematic error? Is there any kinematics dependence? In particular, how does the confidence level distribution look like for Egamma=3.6 GeV, where g6a and g11 overlap yet differ, for both data and MC?

Comment #10:
Whether the bump in Xsection at 2.1 GeV is a result of L(1520) or not still is not addressed in clear manner. The author stated to have hoped to gain insight from the BARYONs'10 talk as a result of discussions with the RCNP folks, and I would like to be updated on that. Thanks.

Comments #9:
Author acknowledges that no one knows why there is a current dependence, therefore I don't know why it is insisted upon using "live-time-correction". As far as I understand, it's a pure accident that the live-time^2 formula works. It could well be live-time^2.2. It is simply a current-dependent normalization factor, and should be referred to as such. It certainly should not be called live-time correction in the eventual paper.