Photoproduction of Λ and Σ^0 hyperons using linearly polarized photons — Responses to Collaboration Review Comments

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RESPONSES TO COMMITTEE REVIEW - ROUND 2

Thank you to those who commented on the draft paper. These are our responses to comments from collaborators. When items are marked with "OK", we have followed the suggestion completely.

DAN CARMAN

General:

- Use units of c=1 throughout as you are not consistent in your notation.
  We have retained the units of c in the text, but have been through the text to ensure the correct dimensions for mass, momentum and energy are used.

- Even though you define θ_K as the CM angle of the kaon, I am not a fan of your notation. I much prefer notation like θ_K^* or θ_K^{CM} to ensure there is no confusion with lab angle and to be consistent with notation in all of our other CLAS papers.
  We have altered the notation to read $\theta_K^\star$ instead of θ_K throughout the text.

Page 1:

- Line 66. Use "... has been carried out ...".
  OK

- Line 76. Use "... and the angular range ...".
  OK

Page 2:

- - Line 60. Use "... of roughly 1 T in the ...".
  OK

- - Line 77. The Start Counter is not made from scintillator "tiles". I recommend "consisting of scintillation counters surrounding ...".
  OK

- - Line 84. Use "... flight, were used to deduce ...".
  OK

Page 3:

- - Line 42. Use "$K^+\Sigma^0$".
  OK

- - Line 45. Use "In order to "clean up" the ...".
  OK

Section B.

- - Did you not apply momentum corrections to these data?

- - Did you account for TOF paddle inefficiencies or study them for this analysis?

- - What about the standard Tagger corrections for sag, etc?

- - Did you study acceptance corrections or quantify your level of sensitivity to them?

- - What about bad Tagger elements that would affect your statistics vs. W?
  These are all valid questions, but have they have been dealt with in the analysis review. The main reason for not worrying about acceptance and inefficiencies is that the extracted results are all asymmetries. *The CLAS detector does not distinguish between the parallel and perpendicular settings of the photon E-vector, so anything related to acceptance cancels.*

Page 4:

- - Section E title. Use "Background Corrections"
  OK

- - Line 40. Use "... of a Voigtian ...".
  OK

- - Line 54. Use "Section".
  OK

- - Line 67. Use "cross section".
  OK

Page 5:

- - Line 28. Nuisance Parameters? I loved this terminology!
  Great isn't it! However, it is standard terminology in statistical literature.

- - Line 35. Use "Subsection".
  OK

- - Line 60. Use "Section".
  OK

- - Line 89. Use "Σ^0".
  OK

- - Line 92. Use "Σ^0".
  OK

- - Line 97. Use "... the statistical uncertainties.".
  OK

Page 6:

- - Fig. 3 and 4 captions. Use "... coverage in W vs. $\cos\theta_K$ ...".
  OK

- - Fig. 3 and 4. I do not understand what the solid points represent based on your explanation. Are these the bin averages of your bins in both dimensions? If they are bin average values, how were these determined?
  The points represent the weighted mean of the bins, where the weight is determined

from the distribution of measured events. We have added a sentence to the text to this effect.

- - Line 12. Use "$K^+\vec{\Lambda}$".
  OK

- - Line 13. Use "$K^+\vec{\Sigma}^0$".
  OK

- - Line 24. The wording is not clear for what the blue curves are. How about "... whilst the blue curves are the result of a re-fit solution of the Bonn-Gatchina partial wave analysis of data for all channels, ...".
  OK

- - Line 28 and line 30. Use "$\cos\theta_K$".
  OK

- - Line 48. Use "... over the whole ...".
  OK

Page 7:

- - For the B-G fits, what is the advertised range of validity in W for the 2014 solution?
  Since the solutions are fits to measured data, the region of validity will be that spanned by the data. Cross sections in these channels go up to about 2.8 GeV, but our results are limited to the range 1.7 ¡ W ¡ 2.2 GeV.

- - What is not clearly discussed is the parameter changes with the Bonn-Gatchina model fit to all of the data including these but not including any new states. What can you say?
  We think this is probably an issue for the Bonn-Gatchina group to consider. We have been told that even small changes in couplings can lead to different results, due to interferences, even when there is no change in resonance contribution. It all just points to the problem still being under-constrained for fits.

- - For all data figures the x-axis range is not appropriate. The plots should start at 1.7 GeV to eliminate all this unused white area from 1.6 to 1.7 GeV.
  Fair point. We have altered the range in the plots.

- - For all data figures, you do not mention what your horizontal error bars are supposed to represent?
  They represent the angular limits of the bins (as defined in figures 3 and 4). We have added text to this effect.

- - Fig. 5 caption. Add a period at the end of the last sentence.
  OK

- - Discussion question: It is not clear the strategy that was employed for adding other N*, D* states to the fits. Was this a "random" approach or were the added states based on expectations from quark model calculations or 1- and 2-star states in the PDG?
  As shown in the plenary presentation to the collaboration, the approach to adding new resonances was fairly systematic and comprehensive, to include any possible partial wave.

- - Data plots. I am not clear on how you handled the 2-body analysis vs. 3-body analysis. You stated (if I understood what you wrote in Section III) that you somehow included both in your results shown here. That does not make sense to me. There are double counting issues and issues with different systematics, etc. Please clarify what you actually did. Also you claim that the 2-body and 3-body final state analyses gave consistent results. Why did you not include a plot to show this?
  There is no double counting. The events used for the results presented were either 2- or 3-body topologies. In the analysis review we showed a comparison with the case when only the 3-body events were used (roughly an order of magnitude fewer). Such a comparison plot would not, in our opinion, enhance the present paper.

Page 11:

- - Line 3. What do you mean by "baseline fit"?
  We have altered this phrase to read "fit with no new resonances"

- - Line 5. What do you mean by "diluted"?
  The improvement in overall χ^2 is due to very small improvements in χ^2 over many channels; in other words, the channels discussed here are not the "killer channels" that nail down an interpretation.

- - Line 29. What is "S.br"?
  This is the label of the formula in the cited original paper. We have included the word "labelled" to emphasise this.

Page 12:

- - Appendix A. I am not sure that I followed this sketchy discussion of how the data were analyzed. Given all of your symbols, it was pretty much all Greek to me.
  This is indeed fairly terse, but it should contain the salient points for developing similar procedures.

- - Line 45. Use "cross section".
  OK

- - Line 46. Use "center of mass".
  OK

Page 13:

- - Line 23. End the equation with a period.
  OK

- - Line 61. Use "Section".
  OK

Page 14:

- - I would like to see the different collaborations (CLAS, LEPS, GRAAL) actually listed with the papers. e.g. J.W.C. McNabb *et al. (CLAS Collaboration)*, Phys. ...
  OK

IGOR STRAKOVSKY

Hi Craig and all Glasgowers,

Thanks, that is really a good piece of work. I have two minor problems with your Figs 3 & 4. It seems to me that these plots shown all data included unpol and pol measurements. I was thinking that is good to show it up and additionally spell it out in the text or figure caption. Then let me suggest to use theta frame instead of \cos \theta one. The \cos \theta squeezes forward and backward directions which are sensitive to the highest multipoles.

Cheers, Igor

The comparison of kinematic reach refers only to linearly polarized photon results. We have altered the caption accordingly. Most of the plots shown in previous CLAS papers on this reaction, and other similar ones, use $\cos\theta$*, so we will stick with this convention.*

NATALIE WALFORD

Hi,

So I have actually never commented on a paper before, so I am not really sure how it works. However, I do have some comments. The first is that the English is written somewhat strangely, which is odd because the writers are native English speakers. I am not sure if anyone else feels that way, but I thought some parts really did not flow very nicely. But that is a side note.

Response: We would need specific suggestions for improvements in order to make any changes.

As someone else is also doing the observable T for K+Lambda and Sigma, I have seen and discussed these results with Glasgow people often. First, I do not understand why the observables are not compared to either GRAAL or LEPS published data, when you have gone into detail about the previous results in the paper and also in talks (and to me), discussed

how the CLAS data is essentially the same as GRAAL and LEPS. I cannot understand then why your data is not compared to published data. One comparison in a histogram is when you discuss the overlap in kinematic coverage (Figs 3-4). The only comparison to any actual previous published data that I can see is in Fig. 13 to previous CLAS data of the Fierz identity. Also, I don't know if this is that big of a deal, but in the GRAAL paper, they also compare their results to the RPR model and here the RPR model is not shown, but I am not sure if that is so crucial.

Response: The comparisons are shown in the analysis note, and also in the presentation given to the collaboration. Adding these plots to the paper would not, in our opinion, improve it. Adding another calculation to the plots will not improve the paper either, since the main points are derived from the comparison between the predictions and the refitted calculations from the Bonn-Gatchina group.

It just seems like often I was told that these results are in line with GRAAL and LEPS (while my data from g9b differ in some kinematic regions) and that made the data strong, yet it is not in the paper. I don't understand that.

See above.

Thanks, Natalie

VOLKER BURKERT

Craig et al,

This is a well written paper and it carries a large amount of important information on several polarization observables in k-hyperon production.

I just want to add a couple of general comments:

1) All the data are presented in a number of figures only. I think you should include the actual data values in tables to make this a complete publication.

2) The asymmetry data should be included in the CLAS Physics Data Base and the text should make reference to the data base.

Response to 1) and 2): The data have now been included in the CLAS Physics Data Base. We now include a specific reference to this in the text of the paper. Since this data is publicly available, we believe that this is sufficient for others to use it in further fits, since they will most likely want an electronic copy.

3) Ref. [22] refers to a CLAS Note as (unpublished). However, CLAS notes are accessible by the public. II suggest to add the full link to the CLAS note directory.

OK

Otherwise, full speed ahead with the publication.

Volker

FRANZ KLEIN

Dave, Bryan, and Ken:

I finally got time to read through the long(!) paper. I am glad that this is finalized now!! and I am surprised how much the BoGa result changes due to the g8 data. Though it might not be kosher to mention a list of modified resonance parameters in such a publication, but which additional resonances or significant modifications did they find?

Another thing: the GRAAL data for Ox and T look less negative than the g8 results. I am not sure because it's difficult to see in the plots. Do you have some figures with the final results of g8, GRAAL (,LEPS)?

Congratulations to have this finished!

Franz

We state in the paper that the combination of additional resonances that gives the smallest overall χ^2 is the addition of both $N^\star(\frac{3}{2}^+)$ and $N^\star(\frac{5}{2}^+)$ states. With the plots in the presentation to the collaboration that show new and previous data, we believe that we have answered the point about comparisons.