

As usual, the committee's remarks are in red and my replies are in black.

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 high W bins (Fig. 9.14 in the new note) from both results to be compared. If the g6a yield extraction is the culprit, it should be demonstrated to be so.

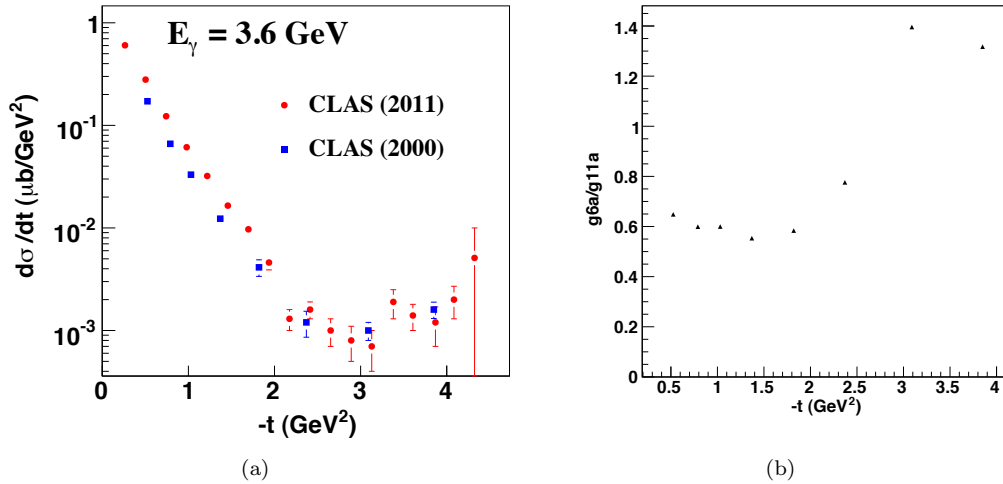


Figure 1: Ratio between $g6a$ and $g11a$ $d\sigma/dt$.

In Fig. 1 (this reply) I show the estimated ratio between Anciant and me. Since our kinematic points doesn't match exactly, for each Anciant point, I chose the two nearest neighbour $g11a$ point in t and calculated a weighted average of the two results to give an estimated $d\sigma/dt$ at each Anciant point and then took the ratio. Roughly speaking, $g6a$ is about 40% lower than us in the forward angle bins.

Please note that the $g11a$ - $g6a$ comparison plot in Ver. 2 (Fig. 9.14) of the note appears incorrectly. I mistakenly used my $E_\gamma = 3.3$ GeV bin (meant for Daresbury) instead of the $E_\gamma = 3.6$ GeV bin appropriate for $g6a$. The plot appears correctly in Ver. 1 though, and I will rectify this in the next version. Sorry for this.

When the committee asks for a comparison of the yields, are the numerical values of the $g6a$ yields listed somewhere? I didn't find it in Anciant's thesis, but my French is also not very good, so I might have missed it.

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 analyses 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),

I don't understand why the committee says this. First, we are set up to work in multiples of 10-MeV-wide W-bins only. Then, I have already had to rebin the neutral-mode several times while deciding upon a final binning, because this topology doesn't have enough statistics to work with 10-MeV-wide bins. It does take a lot of time, and I'd rather not do it all over again. I think the simplest solution is to make comparisons of $d\sigma/d\cos\theta$ in $\cos\theta$ bins, which has the least dependence on the particulars of energy binning.

so we would like to see this comparison to be done soon. We suggest that the CMU group provide the numbers, in the ODU binnings, for the data yields, acceptance, and photon flux(with and without the normalization factors), to compare with the ODU numbers.

I will correspond with Heghine to get the comparison plots soon. I was also wondering if ODU could provide plots showing (1) Data/MC comparison (2) quality of yield extraction fits, in *just* that ($E_\gamma \sim 2$ GeV, $\cos\theta = 0.9$) bin, which is where we seem to differ. Please note that these detailed plots appear in my Analysis-Note/extra-details-writeup, for my analysis.

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.

Sorry for this. Hopefully this will be addressed adequately in this round.

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. It is mentioned in the new note that this is due to constraints of new fitting parameters (BW width). Could a few comparison plots be presented, for both bins where changes are most and least significant?

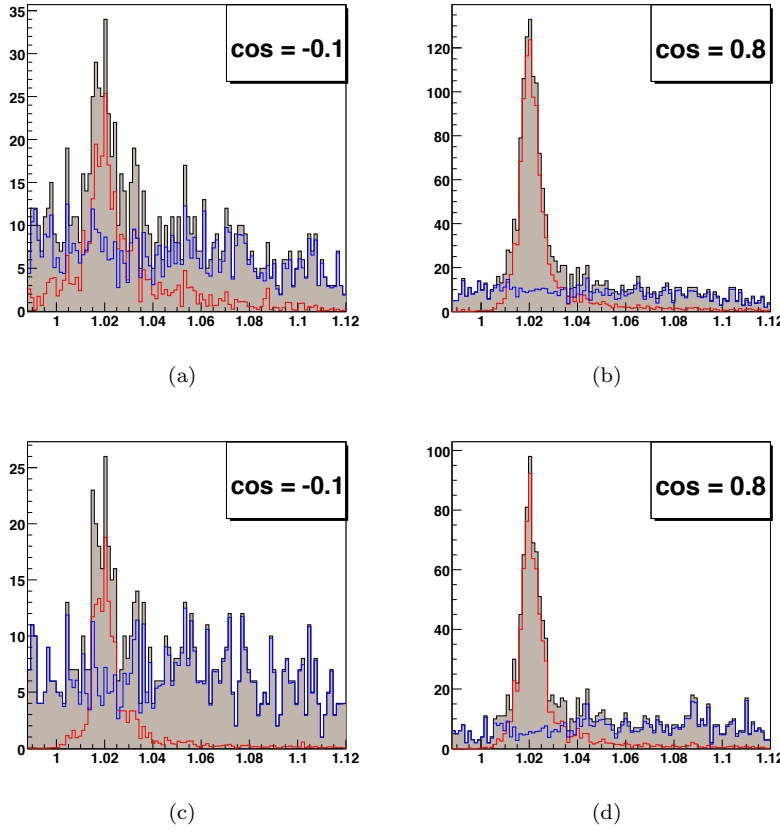


Figure 2: Plots of $MM(p)$ for $W = 2.105$ GeV bin (charged-mode): (a) and (b) shows the earlier Ver. 1 results while (c) and (d) shows after an upper limit of 15 MeV is placed on the Gaussian width going into the Voigtian. The difference is most stark in the mid-angle bins. As usual, the red histograms show the extracted signal and the blue ones are the background.

As I mentioned in my additional note, the difference seems to matter only in the mid-angle, $W \sim 2.2$ GeV region. A good example is $W = 2.105$ GeV bin, as shown in Fig. 2 (this reply). The top two panels show results from Ver. 1 of the Analysis Note, and the bottom plots, those from Ver. 2. (please note that some of the analysis-cuts were also changed between the two versions). Comparing Fig. 2a and Fig. 2c, especially below the $MM(p) = m_\phi$ mass peak, signal yields seem to be somewhat over-estimated in Fig. 2a compared to Fig. 2c. The effect is much reduced in the forward-angle regions, and of course, once we are out of the $W \sim 2.2$ GeV region. Figs 3a and 3b

(this reply) show the comparison in a bin beyond $W \sim 2.2$ GeV – the effect is almost gone.

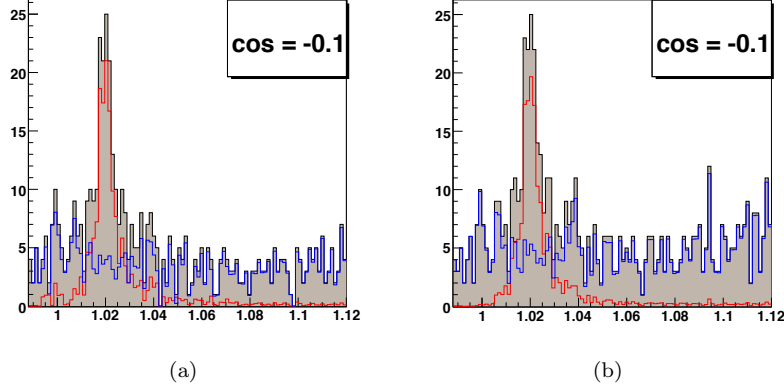


Figure 3: Comparison for $W = 2.305$ GeV bin: (a) Ver. 1 and (a) Ver. 2. The difference in the quality of the fits is almost gone once we are above $W \sim 2.2$ GeV.

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.

Michael Dugger:

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.

The 5 GeV runs in *g11a* is a sub-percent of the entire dataset. Especially for a channel like ϕ , there is simply not enough statistics for such a systematics comparison.

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.

It is not obvious to me, how. When I say that the $\Lambda(1520)$ doesn't quite go away from background-subtraction, this is before applying the hard 15-MeV cut around the $\Lambda(1520)$ mass. After this hard cut is placed, it's a completely different kinematics from the perspective of the $\Lambda(1520)$ channel and I don't see how the two situations can be compared. As Valery pointed out, once this cut is placed, kinematically, the $\Lambda(1520)$ can't leak in. The only caveat is that the Φ width is quite large, so the $M(pK)$ boundaries calculated assuming $m_\phi = 1.019$ GeV is not quite correct. However, I think the much better agreement between the charged- and neutral-modes from Ver. 1 to Ver. 2 sort of speaks against the $\phi - \Lambda(1520)$ interference case. Looking back, I think there are two points that come out – first how important the neutral-mode analysis is, and second, clearly, the $\phi p - K^+ \Lambda(1520)$ are strongly correlated in certain pockets of phase-space. In fact this could have a bearing on the $\Lambda(1520)$ analyses as well.

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.

I disagree that going from 1% to 1.5% signal loss represents a 50% increases in my *yields* uncertainty and that this should be a cause for consternation. At worst, I think it's a 0.5% uncertainty in my yields, but there is already a 3% systematic uncertainty from the kinematic fitter that we put in. This particular study was a very hand-waving argument that we did with the $K^+ \Sigma^0$ to show that we were in the correct ballpark with our kinematic fitter. I also remind you that the $K^+ \Sigma^0$ 2-track did not use the kinematic fitter at all, and it agreed with the $K^+ \Sigma^0$ 3-track which did use the fitter. Regarding what happens if the cut is at 10% instead of 1%, this was checked during the ω analysis by comparing against a more conventional total missing mass cut.

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 $t=t_{min}$. It is a good sign.

Indeed, pion/kaon separation, especially at high momenta, forward angles, has always been an issue. The problem is exacerbated if one is looking *just* at a single-track PID, which is what PART does. This's why we do not use PART. We look into the characteristics of the entire reaction of interest to do our PID. The kinematic fitter certainly does this, and this is also the motivation behind our 2-D timing cuts. Lastly, acceptance is also an issue, due to the forward-hole, and I've heard complaints from the LEPS people about this. However, if the MC (rather, the weighted MC, after our PWA fits) follows the data and shows the depletion as well, we should be fine. I show these Data/MC comparisons in detail in my thesis.

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.

If it's the naming issue, not a problem, I can change the name of the correction in the next version. If this issue has been better understood elsewhere, may be you can point me towards the documentation as well. For the live-time 3% uncertainty error, I quote from Williams' review document (<http://www-meg.phys.cmu.edu/williams/review/norm.html>):

"The stability of the normalized yield should incorporate any "relative" systematic uncertainties with this approach. As for a possible systematic offset which may exist, I'd refer you to Figure 4.13(c). The g11a production runs were taken using 55 nA $\leq I \leq$ 75 nA. In this range, FCUP and Clock*Clock agree to about 3%. Therefore, we will add an additional systematic uncertainty for the live-time correction of 3%",

where he is referring to Fig. 4.13(c) here:

<http://www-meg.phys.cmu.edu/williams/pdfs/thesis.pdf>

I believe you suggesting we remove this from the list of our systematic errors? If so, people might question why CLAS has published results which did include this error. In the context of the correction as applied in our CMU *g11a* analyses, I think it would be better to include it. May be we can call it "current-dependence normalization error" or something, instead of "live-time correction error"?

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.

Sounds like a rather harsh statement to me. It's a paper from more than a decade ago, from a run-period I'm not familiar with. The original authors are not available for consultation and my French is not good enough to pore thru Anciant's thesis. The only person I know who was connected to this is Laget, and I've asked him about this several times without obtaining any clear answers. I asked Reinhard about it and he said that the French *g6a* group had their own normalizations, distinct from the Genova *g6a* group for the corresponding ω paper. From my understanding of the available documentation, those *g6a* yield extraction fits look suspicious to me, and this is what I had pointed out to the committee. If there are specific recommendations, I would be happy to look into them.

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.

First, please note that we don't use a physics MC generator at all. We mock up a "model" that best describes the data, but we don't demand that this model represent any physics. We clearly demonstrate that our MC matches the data well enough – this is why I chose $W = 2.135$ GeV in Fig. 9.2a-c (Analysis Note, Ver. 2), because this bin lies right in that overlap region. Yes, by making the L1520 hard cut, a direct interference is kinematically not allowed, and the fact that the charged- and the neutral modes agree so well now also speak against the interference hypothesis. However, rescattering can still take place and this is independent of how the Phi or the L1520 decays subsequently. Btw, I should also point out that any L1520 analyses will also be affected by this.

Raffaella de Vita:

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.

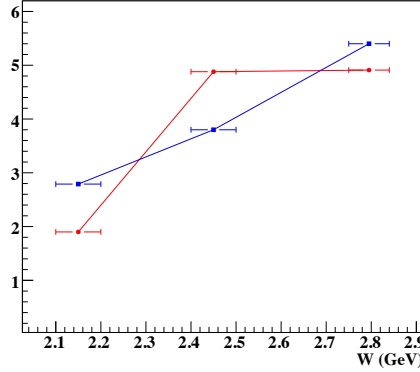


Figure 4: Estimated percent signal loss due to timing cuts for the charged-mode. Red is data and blue is acc. MC.

Well, this is expected, because at higher W , the momentum resolution worsens and there is more “movement” across the cut-boundaries. You can even see this in the Gaussian widths for the ϕ or Σ^0 signal peak – they widen as the energy increases. In Fig. 4 (this reply), I show how the estimated signal loss due to the timing cut changes with W , for both the data (red) and the accepted monte carlo (blue). There are a couple of caveats that I should point out here. First, it is tricky to extract out the phi signal out of Data “rejected” events, because it’s a very small signal on top of a large background. Plus, the phi has a complicated lineshape as well. Therefore, caution should be used while interpreting Fig. 4 (this reply) – I would probably add $\pm 0.5\%$ uncertainty in the numbers quoted. Secondly, a certain amount of signal-loss does not mean our systematic uncertainty is also that much. In our analysis, because our timing cuts are so loose, we quote the signal loss as an *upper bound* on the estimated uncertainty. In my opinion, the number (W -independent) we have quoted (4.8%) is conservative enough. As shown by Fig. 4 (this reply), the MC is matching the data pretty well in the trend (keeping the aforementioned issues in mind). Uptil now, I’ve done this study in three 100-MeV bins, but this can be extended as well.

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.

Yes, but if it is really a cause of worry, shouldn’t this cause a mismatch between the $pK^+\pi^-$ and $pK^+(\pi^-)$ topology results for the $K^+\Lambda$ channel? The fact that they agree so well shows that our cut limit for forward going tracks is conservative enough for -ve tracks as well.

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.

This study doesn't simply plot the momentum versus angle of a particle, but an empirically defined "acceptance" for that particle. To do it for the Phi, say for the proton track, I would need to do kinematic fits to $\gamma p \rightarrow (p)K^+K^-$, check whether the missing proton is present in the appropriate sector, both for the Data and the MC. It can be done, but it's not exactly a snap. To me, the three physics conclusions from the plot seems pretty robust. If you still think this's essential, I'll get it done in the next round.

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).

Shown in Fig. 5. Please note that the L1520 leakage problem is substantially reduced in Ver. 2.

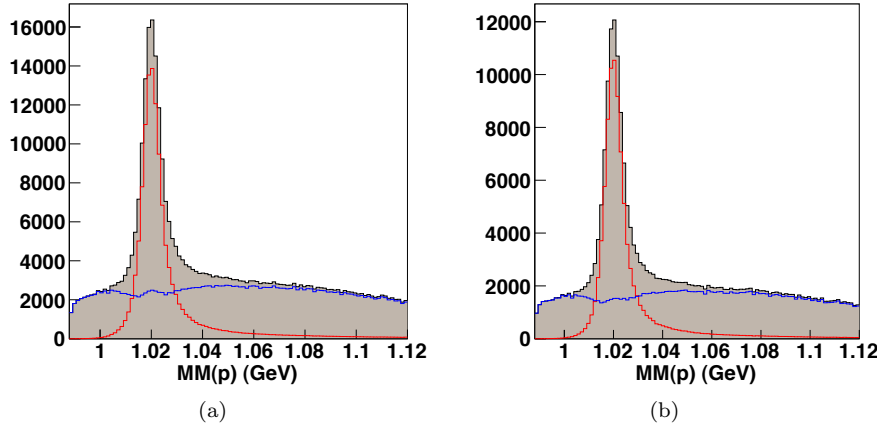


Figure 5: Cumulative yield with W from 2 to 2.2 GeV: (a) is without any cut on L1520 and (b) is with the 15 MeV cut around L1520.

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.

All I said was that at higher energies, the kinematics become more forward-peaked. So it is plausible that the greater uncertainty at higher W reflects the fact that that most of the statistics is now in the forward-angle regions, and our understanding of the detector tends to get poorer here (forward-angle hole, may be?). Looking at Fig. 4.8a (Analysis Note Ver. 2), I don't find the width of the "band" showing any clear-cut angular dependence, except for the edge bins, which have large statistical errors. This study was done for the $K^+\Sigma^0$ channel, but my point is, even if there is a small angular dependence *within* a bin, it would be very difficult to separate it out from the statistical uncertainties. To do this kind of a study, we need very fine W-binning, only possible for pion channels. For the $K^+\Sigma^0$ and $K^+\Lambda$ channels, we already had to merge bins to gather enough statistics during this study and for the ϕ , we would be killed by statistics.

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 N_c 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.

In Figs. 6 and 7 (this reply), I show the percentage spread $\frac{x_1 - x_2}{x_1 + x_2}$ where x_i is $d\sigma/dt$, between the choices $N_c = 100$ ($i = 1$) and $N_c = 200$ ($i = 2$). To keep keep things in perspective, I also show in red, the boundaries from the percentage statistical error $\sigma = \sqrt{(\sigma_1^2 + \sigma_2^2)/2}$, where σ_i is the percentage stat. err. In Figs. 8 and 9 (this reply) I show the cases for quadratic ($i = 1$) and quartic background ($i = 2$). Fig. 10 and 11 shows the cases for a quartic (for $i = 1$) and phase-space $f(x) = a\sqrt{x^2 - m_K^2} + b(x^2 - m_K^2)$ (for $i = 2$) background forms. The shifts are very small (percent-level mostly) and I remind you that the errors from the Q -value extraction fits do go into our final errors as well.

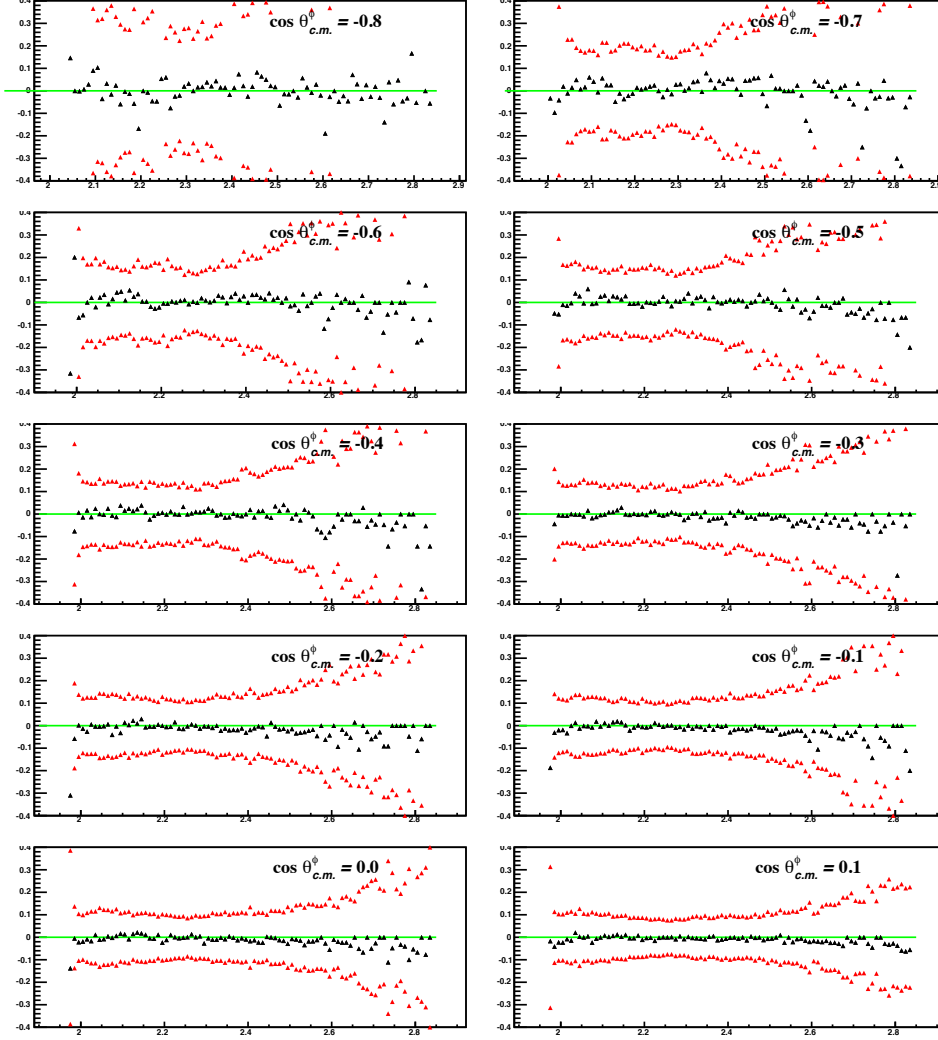


Figure 6: Relative % error between the choices $N_c = 100$ and $N_c = 200$, charged-mode.

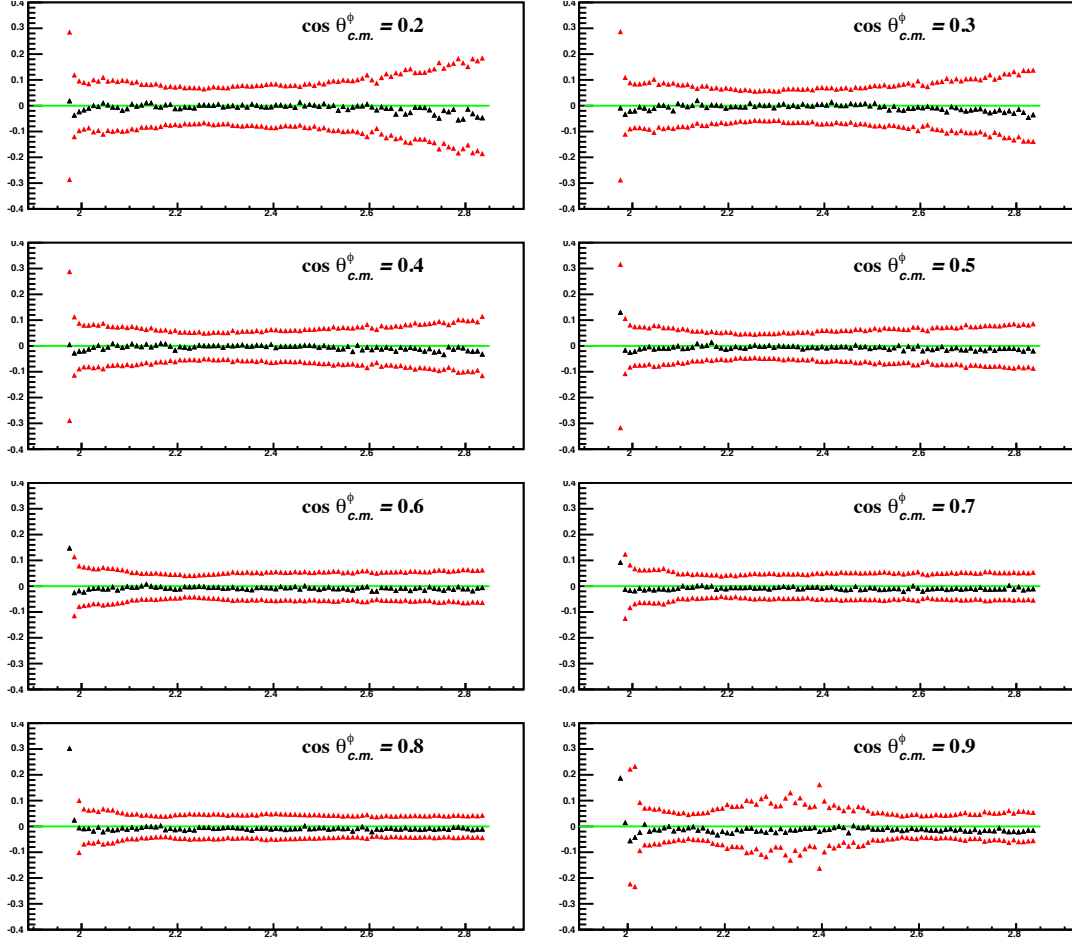


Figure 7: Relative % error between the choices $N_c = 100$ and $N_c = 200$, charged-mode.

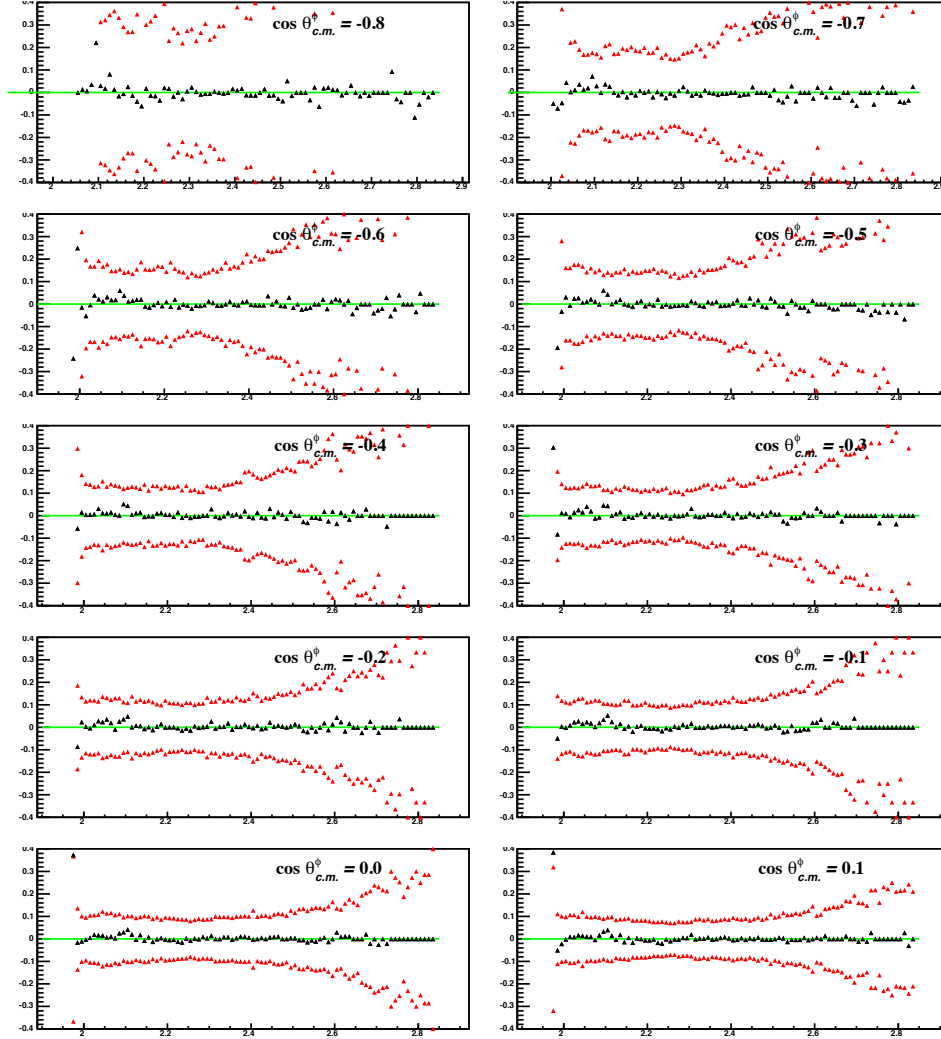


Figure 8: Relative % error between the choices quadratic and quartic background function, charged-mode.

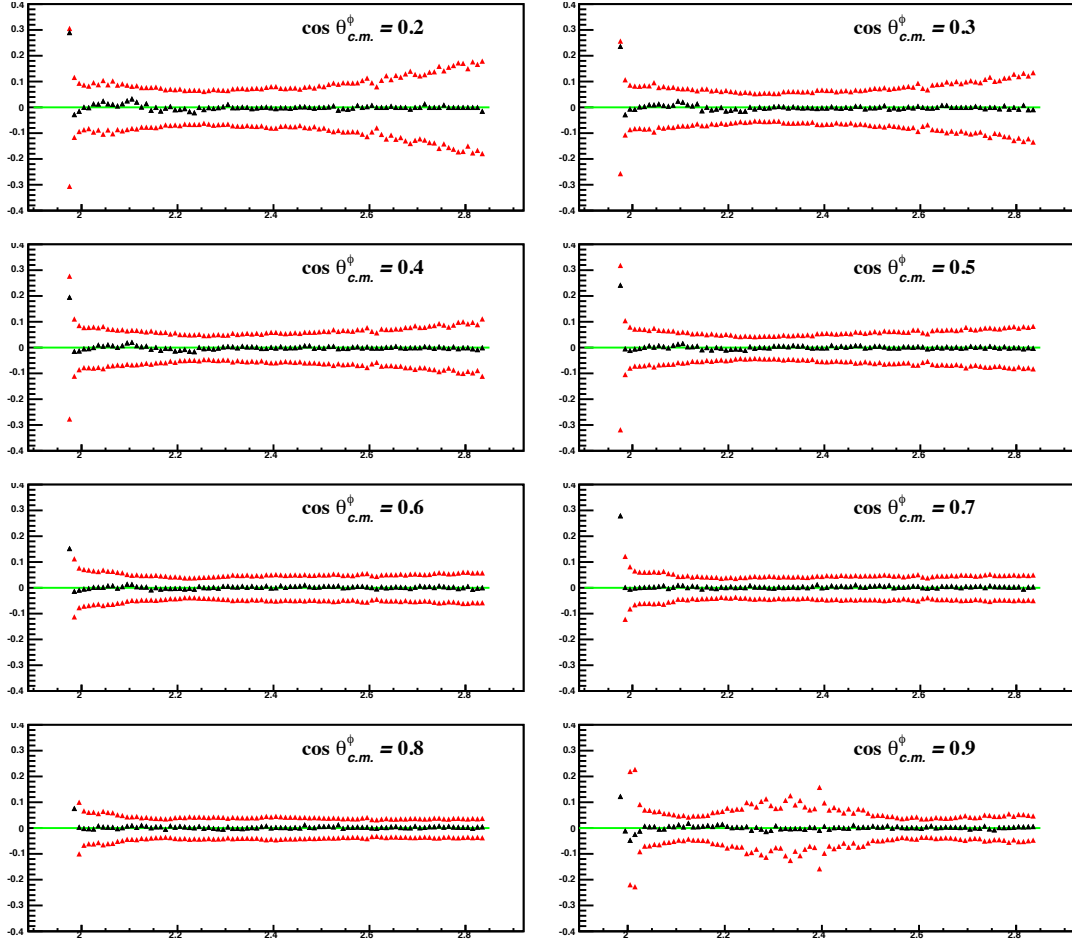


Figure 9: Relative % error between the choices quadratic and quartic background function, charged-mode.

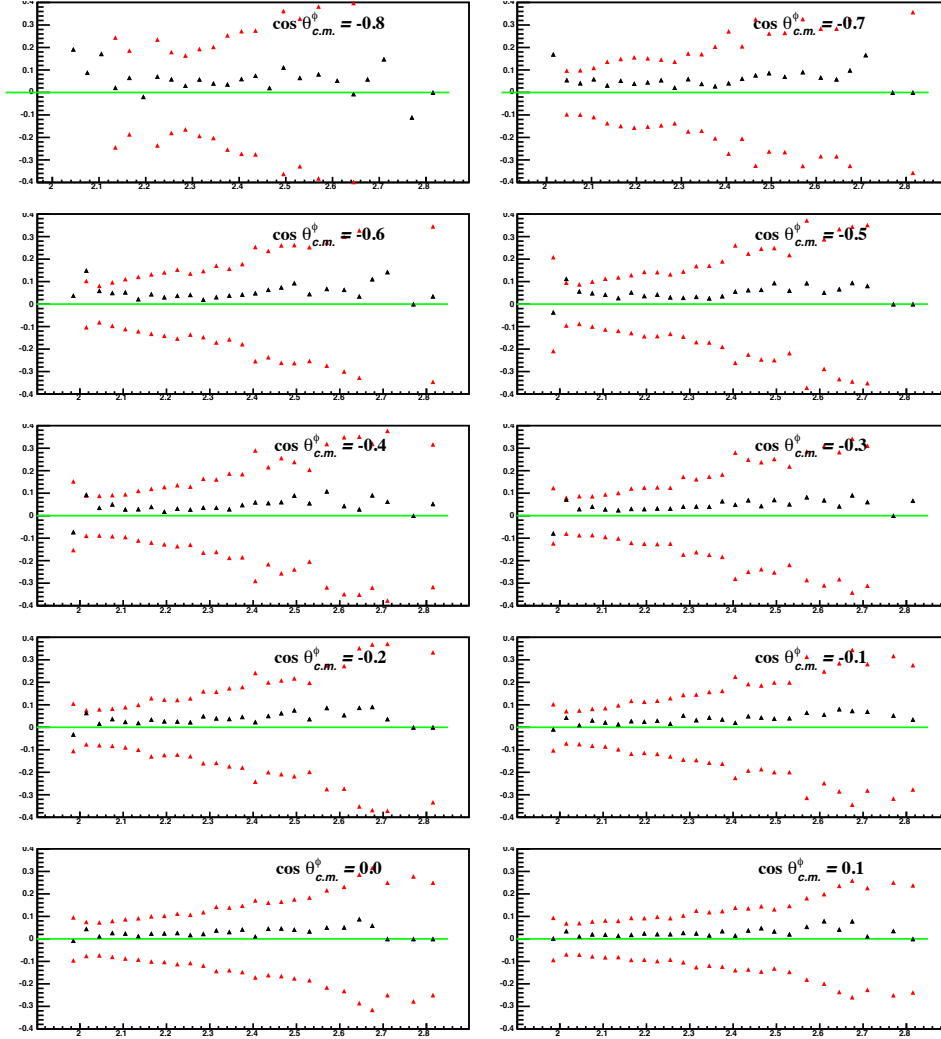


Figure 10: Relative % error between the choices quartic and phase-space background function, neutral-mode.

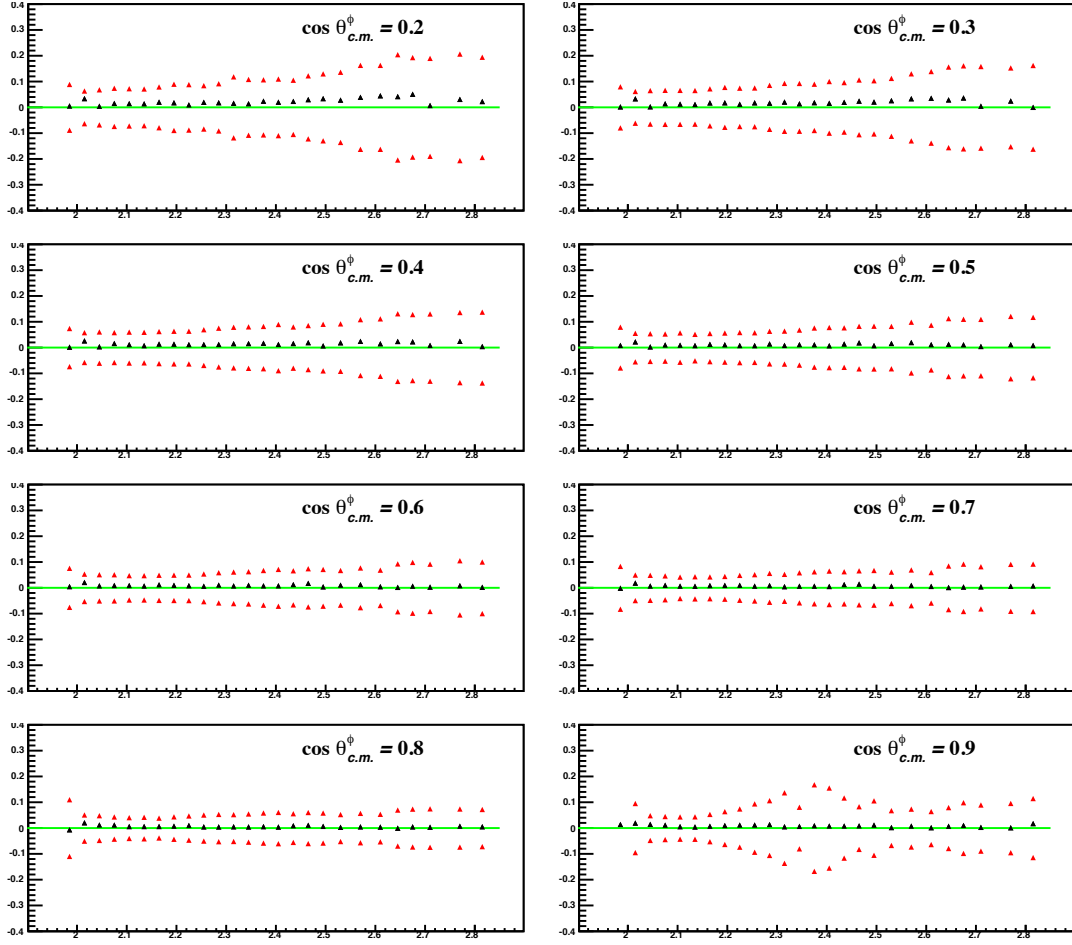


Figure 11: Relative % error between the choices quartic and phase-space background function, neutral-mode.

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.

Please note again that the error bars on the data have nothing to do with the PWA fit results. They are simply the statistical errors coming from the yields and have been included to show that the MC is following the Data within the limits of available statistics. Okay, in Fig. 12 (this reply), I show the rebinned plots corresponding to Fig. 9.1 in Ver. 2 of the Analysis Note.

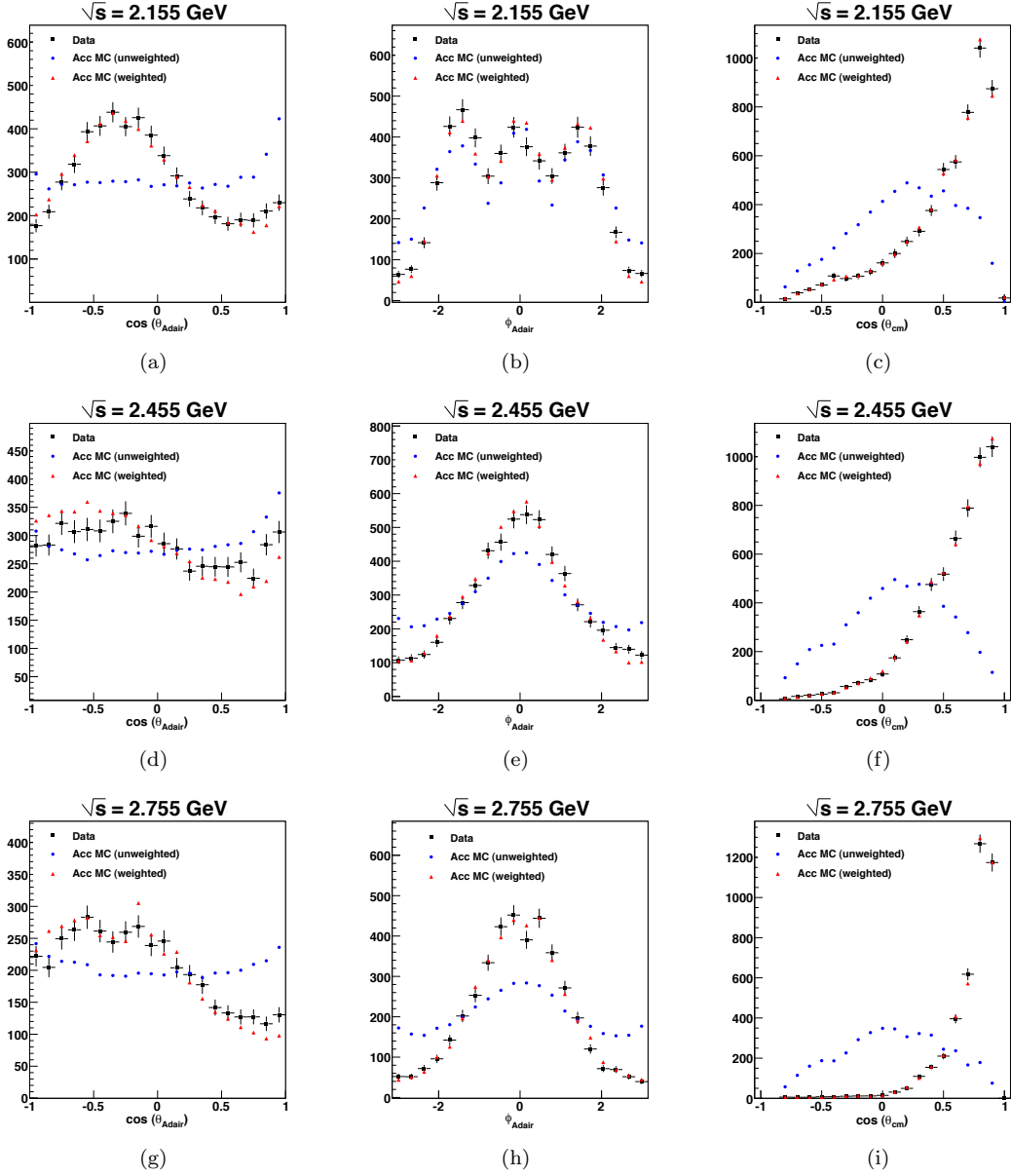


Figure 12: Charged-mode fit-quality check plots.

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.

Fig. 13 shows the plots for the charged-mode. Fig. 14 shows the plots for the neutral-mode in three different W-bins.

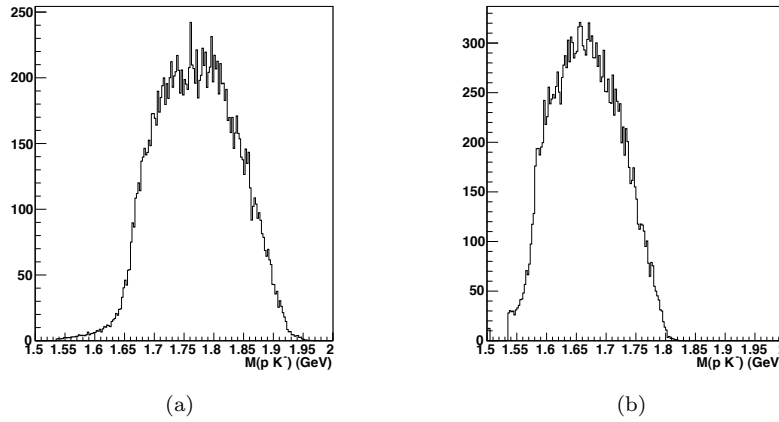
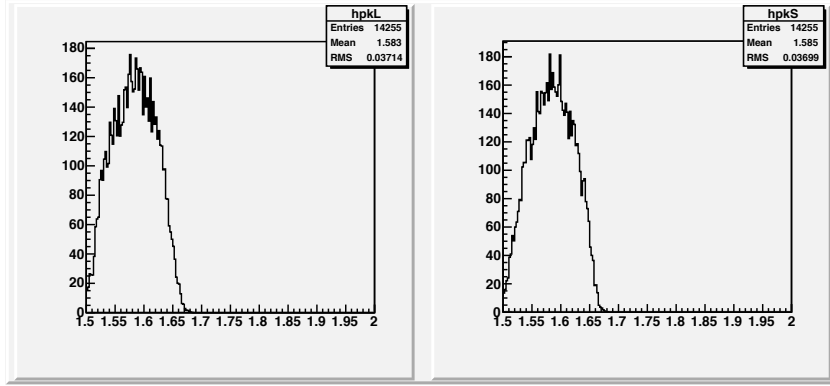
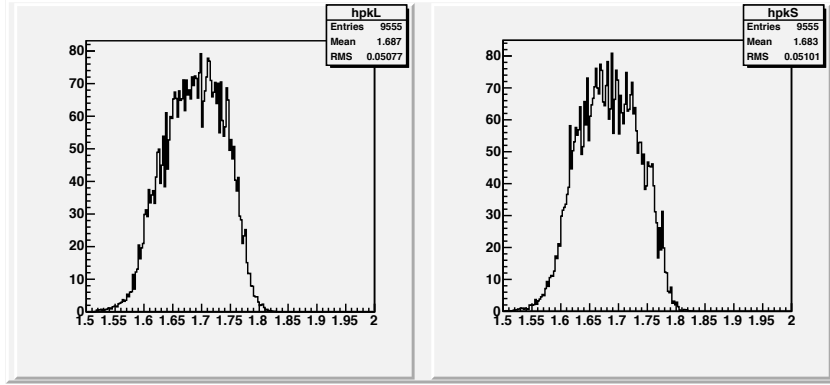


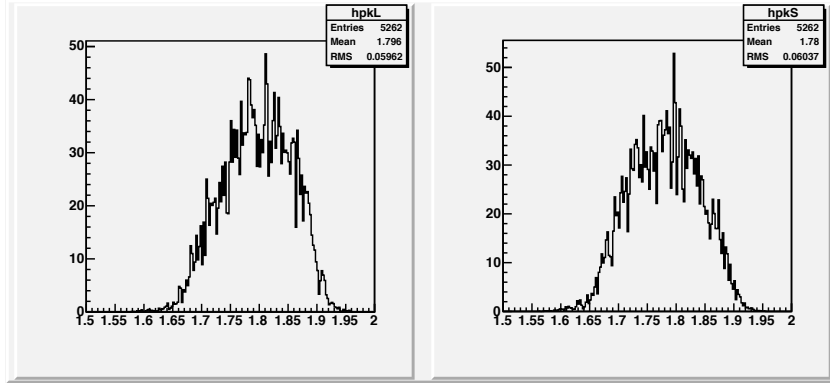
Figure 13: Charged-mode pK^- plots: (a) $W = 2.3-2.33$ GeV (b) $W = 2.45-2.48$ GeV.



(a)



(b)



(c)

Figure 14: Neutral-mode pK^0 plots: (a) $W = 2.15\text{-}2.18$ GeV (b) $W = 2.3\text{-}2.33$ GeV (c) $W = 2.45\text{-}2.48$ GeV. The left-side plots show $M(pK_L^0)$, and the right-side plots show $M(pK_S^0)$.

Comment #5: The part of why doesn't phi show up in the rejected events,... is answered, thanks. However, the other part of my original question was whether the 10loss was taken into account as a scaling factor in the cross section calculation. Or is this totally reproducible 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 $E_{\gamma}=3.6$ GeV, where g6a and g11 overlap yet differ, for both data and MC?

All our cuts are applied to the Data and the MC in the exact same fashion, so we don't "correct" for any event loss due to these cuts. The efficiency of the Kinematic Fitter in producing the same pull distributions and flat confidence levels between the Data and the MC has been studied in depth through all the CMU g11a analyses. Please note that our timing cuts are also very loose.

In Fig. 15, I show the confidence levels in the region $|E_{\gamma} - 3.6| < 0.1$ GeV, for the charged-mode. These plots already include the timing cuts. Fig. 15c is a zoomed-in version of Fig. 15b and only a fraction of the g11a dataset has been included (the phi has very little statistics, so it takes a long time to collect enough data). The CL looks pretty flat in both MC and Data.

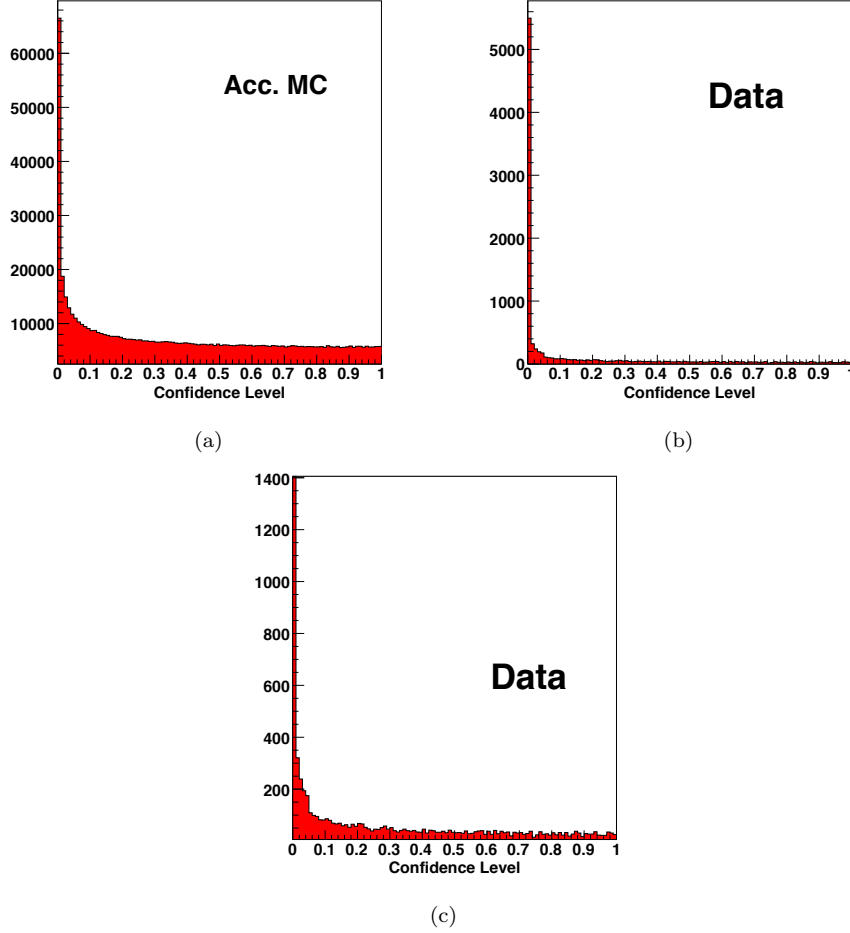


Figure 15: Confidence-levels

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.

There was a talk by Shin Nan Yang right after mine about the 2.2 GeV structure being ascribed to a D13 resonance. Alexander Titov pointed out that the degree of OZI breaking from that resonance would indicate an abnormally high strangeness content in the proton (50% or something) and there were some arguments about this. Atsushi Hosaka pointed out that according to their model, the 2.2 GeV structure is not a bump, but a dip, right *after* 2.2 GeV. However, our $\cos\theta=0.9$ bin results shows that it's a clear bump. The fact that the difference between the charged- and neutral modes is almost gone now signifies that we have been able to get rid of most of the interference (if that is the hypothesis). But then the question remains, what is the forward-angle "bump" in both the charged- *and* the neutral-mode, and why does it appear only in the forward angle bins? Overall, I think it would be pre-mature to claim an understanding of this issue at this stage.

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-time² 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.

Sure, I will change this, both in next version of the Note and the paper as well.