

The third round of comments to g12 note.

Most of the concerns from the second round were addressed in the response. However not all of them propagated to the g12 note.

- Normalization.
 - You take as systematic uncertainty estimate for normalized yield the overall shift between two beam intensities. You did not comment on the width of the distribution for given intensity. Is it consistent with the expected statistical width? If it is large than statistical you should add this to your systematics.
 - Target. You answered our question in the response but it did not propagate to the note. Please fix.
 - ω cross sections. Same as previous. There are requested plots in the response but they are missing on the note.
- Beam polarization. Provide a table with the information which Moller measurement should be used for given run range. Also indicate run numbers when half wave plate was changed.
- TOF knock out. In the response and in the discussions you state unstable and poor resolution counters were added to the knock out list and single particle efficiency were recalculated. However from the note is not clear what is the final knock out list. Please clarify this in the note.
- We suggest moving Fiducial cuts subsection 5.4 from section 5 to section 3.
- Momentum and photon beam energy correction. Chapter 3.3
 - In the latest response, you state that you will modify the note to include clarifications, but this has not been done. Also, a lot of our previous specific comments on that chapter have not been implemented.
 - Clarify if the photon-energy corrections derived and reported in the note were obtained after the application of the phi-dependent momentum corrections reported in 3.3.1. This correction should be derived after e-loss and phi-dependent momentum corrections are applied.
 - Some of us are concerned that the photon energy correction may absorb any remaining systematic biases in the pion momenta. In such a case, the method by which the photon energy correction is derived ensures that the missing mass peaks are placed right at the nominal mass of the missing particle, but the calculated kinematics at which you report your observables (W , θ_{cm} angles) will be incorrectly estimated. We would like to see control distributions that provide evidence that after e-loss and phi-dependent momentum corrections, there are no remaining systematic biases in the particles' momenta:

- Plot the $\pi^+\pi^-$ invariant mass (K_s) as a function of the p_{π^+} and p_{π^-} momenta. On each plot show with a solid line the mean of the integrated invariant-mass distribution.
 - Plot the $\pi^+\pi^-$ invariant mass (K_s) as a function of the p_{π^+} and p_{π^-} theta lab angles. On each plot show with a solid line the mean of the integrated invariant-mass distribution.
 - Plot the $\pi^+\pi^-$ invariant mass (K_s) as a function of the p_{π^+} and p_{π^-} common Z vertex. On the plot show with a solid line the mean of the integrated invariant-mass distribution.
 - Plot the $\pi^+\pi^-$ invariant mass (K_s) as a function of run number. On the plot show with a solid line the mean of the integrated invariant-mass distribution.
- Show plots that the exclusivity cuts described after Eq. (14) indeed remove the background in your sample (this is important for the identification of the position of the missing mass peak later).
- In Fig. 69 you show a fit consisting of a polynomial and a Gaussian - if the purpose of the polynomial is to describe any remaining background, provide arguments why you use a 3rd order polynomial shape (it seems to have a maximum right at the mass of your good events). In any case, if there is remaining background its shape has to be fitted properly. If, instead the purpose of the polynomial is to describe the tails of the peak, then a double Gaussian with a common mean is a better way to fit. We are not sure how much any of these would change your results for the corrected peak position - address.
- Fig. 70: show the the polynomial background curve and label the fit parameter giving the peak position in the fit report consistent with the label in Fig. 69, i.e. "Neutron Mass" instead of "Factor".
- Fig. 72 caption: Clarify the corrections applied in the order as listed.
- p.69, paragraph 1 in "The problem was first noticed by g12 participants at the analysis level in which missing particle masses were systematically low." - Explain if this observation was made after the g12 momentum corrections were applied.
- Some editorial suggestions:
 - p. 69, change "There was also features..." to "There were also features..."
 - p. 69, last paragraph: change "...for runs 56515 and 57130 revealed only a mass deviation of ≈ 1.4 MeV in which disclosed that the problem with the g12 data stream to be solely in the photon beam energy." to "...for runs 56515 and 57130 revealed only a mass deviation of approx. 1.4 MeV, which suggested that ...is most likely in the photon beam energy."

- Single particle efficiency. Comments from Yordanka. I looked at MK's thesis for the dynamic efficiency corrections. They were derived using kinematic fitting of $p\pi^+\pi^-$ events, for each particle separately, for the real and for the simulated data. I cannot see how we can approve these efficiency corrections without reviewing the full analysis that produced them. The only independent (of analysis) g12 argument at this time that can be acceptable in my view about the validity of the correction would be that after the corrections, the omega cross sections from three-track and two-track events are consistent with each other, but I did not see a comparison of these in MKs thesis.

Some major concerns:

- a) According to MK thesis, the kinematic fit was done after e-loss and beam-energy corrections. Phi-dependent momentum correction was not done. This is not consistent with the analysis chain in the g12 latest note.
- b) The parameters of the pull distributions of the simulated and the real data are not consistent with each other within their reported uncertainties. (The error matrices for the simu- and real-data fits seem to have been tuned independently. I wonder how the error matrices compare with each other). I suspect that one of the reasons for that discrepancy are residual differences between the resolutions of the simulation and the data. These would not be necessarily caught by comparing integrated distributions, such as invariant and missing masses, but would become prominent as one "zooms" in a particular bin of (p, θ, ϕ, z) .
- c) For a better estimate of the magnitude of the issue above, one would need to see differences, such as $p_{\text{calculated}} - p_{\text{measured}}$ for the narrow kinematic bin (θ, ϕ, z) as a function of momentum (etc), plotted together for simulated and real data (mean values, widths).
- d) The counting of detected particles was done by checking if a real particle was detected with kinematics falling within the kinematic bin of interest. On average, that is not a problem, if one derives an overall correction, since bin migration tends to average out. However, that can be very dangerous if the local resolutions in the simulation and the real detector do not match. Different bin migrations will cause a local bias in the efficiency.
- e) The uncertainty of the correction has not been estimated (this **may** take care of my concern in d) as long as the biases are randomly distributed over the (p, θ, ϕ, z) bins) as far as I could see. The quoted value of 3% for 3-prong events seems to account for photon multiplicity effects in the correction, but not for inherent effects, such as resolution mismatches simulation-data.
- f) In MK's thesis, the detection efficiencies for p , π^+ , and π^- are shown both for the simulation and for the data. While, the simulated results nicely show the location of the coils, the real data do not. A relatively high efficiency is for the regions of the coils for the data, where we expect zero. One explanation would be that in the determination of the θ_{acosphi} and θ_{asinphi} bins, the bins covering the coil areas contain areas both with zero efficiency and with high efficiency. I would like to see if the efficiency for that bin was reported for the actual average of the events' kinematics in that bin, or the for the middle of the bin (which is unacceptable). I also wonder if the procedure of correction evaluation did check that the actual mean values of the kinematic bins, for which the ratio of

efficiencies simu/real was formed, did indeed match, or one trusted the middle of the bins. Again, this may not be an issue for observables reported for wide bins, but brings into question potential errors of the correction (i.e. systematic biases).