Dear Editor(s) of Phys. Rev. C. and Reviewers of the manuscript:

We thank you for the review of our manuscript and the encouraging comments. We have accommodate most of the comments and we have prepared a brief reply as shown below:

(1.) I propose to use the nomenclature for baryon resonances suggested by PDG: e.g. P33(1232) -> Delta(1232)3/2+ (similar for all states).

*Answer:* done. We have replaced $P_{33}(1232)$ by $\Delta(1232)3/2^{+}$, $D_{13}(1520) \rightarrow N(1520)3/2^{-}$, and $F_{15}(1680) \rightarrow N(1680)5/2^{+}$.

(2.) Page 3, line 159- 163: The authors mention pion photoproduction data but refer only to upcoming CLAS data. Here the published work of other labs should be mentioned as well (e.g PRL 112 (2014) 012003, ...).

*Answer:* Thank you for pointing this out. We have added the reference PRL 112 (2014) 012003, and have also replaced the one of the JLab proposals (frozen-spin targe experiment) by two AIP conference proceedings that presented their preliminary results.

Upon careful review of all articles available, we think that PRL 112 (2014) 012003 is the only non-JLab data on the double spin asymmetry obtained from a longitudinally polarized target and a circularly polarized photon beam, in the high nucleon resonance mass region. Please point out any other existing data that we may have missed.

(3.) page 5: In Fig.2 the labeling inside the picture is missing. I see only the arrows.

*Answer:* We apologize for missing the labels. The problem seems to be related to the PDF processing software. It looks as if the font “Helvetica” can’t be processed correctly even though these fonts show up just fine in the original EPS file on local computers and on arxiv.org. Changing these fonts to “Times-Roman” seems to solve the problem.

(4.) page 5, line 313-324: The new Cerenkov counter is introduced but no details are given. I assume that its structure and performance (gas, efficiency,..) are similar to the existing Cerenkov detectors and Ref.35 is also relevant for the new device. If this is correct one could make such a statement or give some numbers and further reference should be given.

*Answer:* We have added two more sentences on the details of the new Cherenkov, such as its gas and the photoelectron yield.

(5.) page 7, line 482: "Beam charge asymmetry ...." replace by: "A helicity dependence of the integrated beam charge...."

*Answer:* done.

(6.) page 8, Fig.6: I do not understand the interpretation of the data points and extrapolation bands. What happened e.g. before run number 51500 (1.3 GeV data)? Two Moller measurements are significantly lower compared to other. It should be explained why the inter-(extra-)polation was done as indicated by the gray bands. I assume, that its due to changes in the configuration or breaks during data taking. Same question comes up when looking at the extrapolation of the last 2 data points.

*Answer:* At JLab, because the beam polarization in the experimental Hall is uniquely determined by the initial setup at the source injector, the precession of the electron spin in the accelerator and the recirculating arcs that connect the south and the north linacs, and the beam bending from the accelerator to the Hall, we typically expect the beam polarization to be stable during a specific period where none of the above conditions changes. In the case that one of these conditions changes, which happens if a change in the beam energy or polarization is required by the experiment or the experiment(s) that ran concurrently in the other two experimental halls. In this case, a Moller measurement is scheduled as soon as possible following the beam configuration change.

Therefore, for example, the beam polarization Pb obtained from the first Moller measurement performed during the “1.3 GeV NH3” period is applied to all runs of this period, until the next configuration change. Sometimes another Moller measurement is performed during the same period which may provide a different value on Pb. And in this case the Pb result from the newer Moller is applied to runs after that Moller measurement. Of course, multiple Moller measurements during the same beam configuration period should provide the same values, within the uncertainty of the measurement.

We provide a brief explanation of the above reasoning for Fig.6 in the updated manuscript as follows, and have updated Fig.6 caption slightly. We hope it provides a reasonable clarification. As for the low beam polarization around run 51500, it was a result of the beam configuration change that was specific to that run period.

“Typically, M\o ller measurements were performed as soon as a change to the beam configuration was made, and then intermittently throughout the run period. Therefore, the beam polarization from each M\o ller measurement was applied to runs that immediately follow such configuration changes, and to runs that follow the M\o ller measurement until the next valid measurement is available.”

(7.) page 8, Fig.7: axis labels missing

*Answer:* fixed, please also see our answer to comment (3) above.

(8.) page 8, line 549: "ep -> e'pi+(n)" replace by "ep -> e'pi+(X)" as correctly done in the caption of Fig.8

*Answer:* done

(9.) page 9, caption of Fig 8: .... (the proton mass) replace by .... (the neutron mass).

*Answer:* done

(10.) page 9, eq.(26) and eq.(27): There are more recent form-factor data at low Q2 which where not available in 1995 when the fit of Ref.(45) was performed. I assume that details in the form-factor parametrization at low Q2 are not very important for the method and additional errors from uncertainties in our knowledge on proton form factors are small. Probably even a simple dipole parametrization would work. If my assumptions are correct, a short statement about the sensitivity of the method to the form factor parametrization should be added.

*Answer:* We compared the form factor fits from Ref.(47) and the fit from the latest work of J.R. Arrington et al (unpublished), and we found the asymmetry of elastic scattering varies only by 1-2% (relative) for the Q2 region relevant to our analysis. We added a short sentence to explain this in the updated manuscript.

(11.) page 10, Fig.9: The variable "dil" is not defined. I would use the name "f_el^incl" as in the text.

*Answer:* done

(12.) page 9, line 578-580: A reference for the formula A^el_th = ... should be given. I think that in the second term of the denominator G_M^p should be replaced by (G_M^p)^2.

*Answer:* That's correct. We have however re-arranged this formula so that the fraction can be shown properly.

(13.) page 8/9: In the description of the reconstruction of elastic events the authors refer to an invariant mass (e.g. page 9 line 584). To be consistent with the other chapters, this quantity should be called "missing mass".

*Answer*: Actually, this is the invariant mass of the (virtual photon + nucleon target) system and it is different from the missing mass of the semi-inclusive channel. Therefore we did not make the change requested.

(14.) page 10, line 675: The authors claim that their run-by-run values of Pb*Pt determined via elastic scattering are consistent with all configuration changes. I propose to include a figure similar to Fig.6 where Pb*Pt is shown as function of the run number (or blocks of runs). Such a figure would be helpful for the reader to get an impression about the data taking and the consistency.

*Answer:* We added a plot of PbPt as suggested. To reduce the statistical uncertainty of PbPt obtained from each individual runs, we combined adjacent runs that had the same configuration of the beam insertable half-wave plate condition. Note that we did not provide an explanation on any significant change in PbPt that could be observed from this figure, as those are usually due to changes in the target material or beam polarization change that are too detailed for this level of publications.

(15.) page 11, section E: It should be made clear that the discussion about the asymmetry extraction is only valid in each single kinematic bin (Q^2,W,theta,Phi). Only later in the text it becomes clear that this was correctly done. I propose to introduce the 4-dimensional grid of the kinematic variables used to extract the asymmetries already at this point.

*Answer:* done

(16.) page 13, Fig. 10: I assume that the missing mass distributions are integrated over all other kinematic variables. I suggest to show also the missing mass spectra for e.g. 2 typical kinematic bins, which have been used in the real analysis.

Replace the label "dil" in the figure by "f_dil^pi" as given in the text.

*Answer:* There exists a JLab analysis note associated with the work presented in this manuscript. The analysis note contains several missing mass plots for different Q2 and cos(theta*) bins. However, these

plots all look similar to the “integrated” plot of Fig.10 and thus adds little information. In addition, these figures are best printed in color and we would like to minimize the color-printed figures. Therefore we prefer not to add addition missing mass plots.

Also, the CLAS analysis notes are typically not used as references because they are not formal publications that are available to the public.

We did replace the label in Fig.10 as suggested.

*Answer:* done

(17.) Presentation of results: The way the results are presented and compared to model calculations is reasonable for this experimental paper. I have two suggestions:

a) The authors stress several times that the new data are consistent with previous data at higher Q2. But no comparison is shown. I propose to include a plot which shows the consistency to previous data or to add previous data in existing plots.

*Answer:* The published data from CLAS eg1a, in Ref.[17], included the A_LL asymmetry for a higher Q2 region that covers only a small overlap with the current analysis. They also did not divide the data into multiple cos(theta*) or (phi*) bins due to limited statistics. There is a graph on A_LL vs. W, integrated over all cos(theta*), (phi*), and the EG1a Q2 range in the Ph.D. thesis of the first author of Ref.[17], that look remarkably similar to the A_LL figures presented in the highest Q2 bin in Fig.14 (last figure of the manuscript). On the other hand, the different binning means that in order to compare to the EG1a results we need to add a plot of A_LL vs. W integrated over all cos(theta*), phi*, and a comparable Q2 range. THis can be done but we think it will disrupt the flow of the manuscript and add very little information.

There are other asymmetry results on both A_UL and A_LL from later CLAS experiments such as eg1b. These were presented in various Ph.D. thesis (Refs. 20-21) and are recently being drafted into a manuscript (Ref. 22, arxiv:1604.04350 [nucl-ex]) which was also submitted also to Phys. Rev. C. Reference [22] itself does not contain comparison plots with the present manuscript, however in the CLAS analysis note associated with Ref.[22], such comparison is given for both A_UL and A_LL and all results from Ref.[22] and this manuscript are in very good agreement. In fact, should this manuscript and Ref.[22] be published soon, readers can place the two publications side-by-side and compare the results plots, and will easily draw the same conclusion.

In short, CLAS/EG1a results can’t be added directly to our existing figure and EG1b results have not been published. Because of these reasons, we did not add a separate figure or add previous data to the existing figure to illustrate the agreement.

b) Results for the beam asymmetry A_LU were obtained but are not discussed in the paper.

The authors say that A_LU was just used as a cross-check and that the results are available for download. This is reasonable as this observable can be better obtained with a hydrogen target. Nevertheless a short comments about these data should be made (E.g. are the data consistent with existing data? What is the contribution of quasi-free events?). Maybe a reference (thesis?) could be added.

*Answer:* because we are not publishing the A_LU results in this paper, we did not look into the consistency between our A_LU results and earlier data, nor are these results corrected for the contribution from nuclear material. The fractional contribution from the free proton to the A_LU results should be the same as the exclusive dilution factor presented in this paper, and average at the 42% level as shown in Fig.10. The A_LU results were uploaded to the CLAS database *in case* someone would like to take a look at them.

There was no graduate student involved in the exclusive-channel analysis work and thus there is no Ph.D. thesis available. As mentioned earlier, there exists a JLab analysis note associated with this analysis, but such notes are typically not used as references.