Response to Reviewer Comments for Precision Measurements of A_1^n in the Deep Inelastic Regime PLB-D-14-01723

January 30, 2015

First, we would like to thank the reviewers for their thoughtful reading of the paper and their detailed and helpful comments and suggestions. We discuss their specific concerns and our modifications below.

1 Reviewer 1

1. **Abstract: it's likely pure semantics whether the measurement was done on 3He and the neutron asymmetry was extracted from this measurement, but in any case I would bring up here that indeed a 3He target was used (indeed, maybe even mention that both longitudinal and transverse polarizations were employed making the measurement less sensitive to assumptions on the values of g2, even though the latter was not such a big issue in previous measurements).** We have added this information to the first sentence of the abstract.

2. **line 39: maybe also add here "employing longitudinally and transversely polarized 3He targets". (or similar)** With the target information added to the abstract, we fear that its additional inclusion here will disrupt the flow of the discussion relating the neutron A_1 to the distributions of quarks within the nucleon. We feel the ad-

ditional complication of a nuclear target is best addressed later in the paper.

3. **line 90: in consistency with the hyphenation convention adopted elsewhere in the manuscript, I suggest to add a hyphen here: virtual-photon** Fixed.

4. **lines 181-202: a) was $A_\perp$ indeed corrected using the same approach? What was, e.g., the input for the DIS region?** Yes, it was. As was very briefly stated on lines 181-183 of the original submission, our radiative correction procedure depended on expressing the asymmetries as polarized cross-section differences, which in turn can be exactly expressed in terms of the spin-structure functions. The radiative corrections were then performed on those expressions. The input needed is thus a parameterization of the spin-structure functions rather than of $A_\parallel$ or $A_\perp$ specifically. We have added a reference to the PhD thesis where the radiative correction procedure is explained in much more detail.

5. ■☞lines 181-202: b) the subtraction method of RC used in this analysis does not remove systematic correlations between the various x bins. In the manuscript there is only a statement about detector smearing and how much it contributes to the systematics (line 199), but what is the typical percentage of migration from one x to another x bin, what the maximum and the minimum? Such possibly large but now unknown systematic correlations can be turned into known statistical correlations by an unfolding approach.

6. **Eq. 4: shouldn't better the unpolarized cross sections in acceptance appear in this expression instead of the F2. It is difficult to imagine that bigBite has a flat acceptance over all the x bins covered here. Maybe it is only a small effect? (How small?)** Acceptance corrections would typically come in at an earlier stage of the analysis, in the computation of the asymmetries on ^{3}He. It is true that BigBite did not have uniform acceptance over our kinematic range, but this was somewhat mitigated by our data-quality cuts, which removed electrons passing through poorly understood portions of the magnet. The longitudinal extent of the target and the large acceptance of the spectrometer allowed us to study the variation of the

measured asymmetries in each x bin in different regions of the BigBite acceptance and determine that it was not significant. We have added a brief note to this effect to the end of the paragraph describing the BigBite detector stack.

7. ■☞line 214: COMPASS also published results on the proton, e.g., PLB 690 (2010) 466 - why was it not included?

8. **line 221: I would expect that the average Q2 differ for each x bin, so why quote only one average for all bins? Will the neutron results be also available separately for the two beam energies? They give two independent points in Q2 for each x (especially as later on you advocate measuring the Q2 evolution within the JLab12 program), thus in principle useful for global analyses.** The choice to combine the data sets for the two beam energies here is due to space limitations in the letter format; we intend the quoted average Q^2 value to give a rough sense of the statistical distribution in the kinematic range. (The Q^2 values for individual x bins are given in Table I.) Our collaboration is presently working on a long archival paper about the experiment. In that paper, we plan to publish tabulated DIS neutron results separated for the individual x bins at each beam energy. As the reviewer notes, these more fine-grained results will be useful for future analysts.

9. ■☞Fig. 1: a) I assume all the other experimental points are also not at the same Q2 as the one from this measurement, why it was thus chosen not to include the JAM parametrization seemed a bit arbitrary (also in view of the statements in line 214 where in the extraction it was even assumed that A1 was Q2 independent.

10. ■☞Fig. 1: b) the neutron A1 does not have to be extracted from 3He data (together with the proton A1). One can also use deuteron data and combine it with the proton A1 to obtain the neutron A1. I don't see a big difference and why those should be better or worse compared to the selection of results plotted in Fig. 1. I would very much prefer including the results using deuteron A1 as well, e.g., from E143, E155, HERMES, and SMC (I believe COMPASS has not attempted to extract the neutron A1 from their deuteron data). The very least would be to

point out that only experiments using 3He as a neutron source and then give the reason for this restriction.

11. **Table 2: an observation: the systematics for the first x bin reduce a lot going from A1 to g1/F1. Is that possibly a misprint, e.g., 0.012 instead of 0.021? (Can well be that it is correct, it just sticked out.)** This is well observed, but is not a misprint; this is indeed what we found. Unfortunately we do not have an intuitive explanation for this oddity.

12. ■☞line 235: it was not entirely clear why the authors decided to only include those data for which explicitly g1/F1 was available. There are more data out there on only g1 which could be combined with the favorite choice for F1 to obtain g1/F1.

13. ■☞line 239: likewise it is no paramount effort to get from the many (Delta u + Delta ubar) [likewise for d] results (experimental and pQCD analyses, incl. the neural-network approach) the ratios plotted in Fig. 2. The artificial restriction to publications that included those quark-combination polarizations (which may be considered less interesting and is mainly due to the limited data available in this measurement) may be considered misleading as more information on quark polarizations is out there.

14. **Table 3: The caption might read like the systematics are *just* from the neglect of the strangeness contribution. Maybe slightly rephrase, e.g., mention propagated uncertainties from ... Actually, what was done? The extraction of these values included fits to world data. As there is still space in the manuscript, maybe better specify how the uncertainties were obtained (and do so already in the text around line 238).** We have clarified the caption to list some other sources of systematic error, and added a line to the text at the suggested location describing how systematic uncertainties were obtained: "Other systematic uncertainty contributions were determined from the change in the result from varying each input within its uncertainty."

15. ■☞Fig. 2: (s. above comments to lines 235 and 239)

16. ■☞line 249: I would even go so far that the data very much disfavors the original LSS (BBS): above x of 0.4 the new data is many sigmas away from the curve.

17. ■☞line 262: it was somewhat surprising to read now here that for the future JLab12 program Q2 evolution of A1n is an important point, especially as it was completely ignored in the analysis (see comment to line 221). (Shouldn't one then have uncertainties applied due to the assumption of no Q2 evolution here?) Actually, is it necessary to end this nice measurement here with advertisement for certain future experiments. (I admit, it's likely a question of taste.)

2 Reviewer 2

1. **4-6: it does not look correct to quote a phenomenological paper [3] only; a reference to RHIC experimental results is also needed.** We have replaced this citation with citations to the publicly released papers with the STAR and PHENIX results that provided the bulk of the evidence for the claim.

2. ■☞138: please add an example of a numerical value of the dilution factor f, in the region of measured x.

3. ■☞138-141: a figure with definition of all angles would help.

4. **190: The DSSV model [37] was used.... A model of what?** We now more correctly describe this as "The DSSV global NLO analysis".

5. ■☞Figure 2: HERMES points quoted here come from 1999; surely there are more recent results from their SIDIS analysis. Also COMPASS has published results at similar Q2. Please update that plot.

6. **[1] The EMC results on the proton spin were first published in Phys. Lett. B206 (1988) 364; you may add this to the long Nucl.Phys. paper you mention.** We have added this reference.

7. **[53] I guess it has already been published in Phys.Lett. B740 (2015) 168-171.** Yes, this is right. We have updated the citation.

8. ■☞The authors mention that two dedicated measurements of An1 will be performed at JLab in the future, extending measurements to x 0.8. A comment is needed whether systematic uncertainties at that high values of x, are expected to be substantially de- creased/eliminated in those experiments. Otherwise, if a trend visible in Fig 2 continues, the new measurements may not bring any new information, especially for d-quark distributions.