Reviewer #1: Report on PLB-D-14-01723 "Precision Measurements of A_1^n in the Deep Inelastic Regime" submitted to Physics Letters B.

The manuscript by the Jefferson Lab Hall A Collaboration submitted for publication in Physics Letters B reports on the measurement of double-spin asymmetries in inclusive longitudinally polarized lepton scattering from either longitudinally or transversely polarized 3He with the goal of extracting the neutron A1 in a kinematic region where sparse data are available. It is also a region where different approaches of describing the spin structure of the nucleon start to diverge. As this kinematic region still contributes significantly to the spin sum rule it is important to better constrain A1 beyond the presently available world data. The results reported upon in the manuscript go definitely into that direction and as thus are important and deserve publication in Physics Letters B. I have only a few minor comments which can probably be clarified quickly.

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).

line 39: maybe also add here "employing longitudinally and transversely polarized 3He targets". (or similar)

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

lines 181-202: a) was A_\perp indeed corrected using the same approach? What was, e.g., the input for the DIS region?

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.

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?)

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

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.

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.

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.

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.)

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.

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.

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).

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

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.

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.)