Review Comment Round 1

First observation of Beam Spin Asymmetry for K+ (by D. M. Riser)

Review committee: M. Defurne, A. Deur, M. Mirazita (Chair)

The note presents the measurements of the Beam Spin Asymmetry (BSA) for K+ using the e1f CLAS data. Being the first time that the Kaon Semi-Inclusive production is studied in CLAS, the analysis is quite interesting and should be published. At the same time, the committee judges that the analysis note is not yet at a stage where it should have been sent for review. The writing is not polished and thus hard to read. This distracts from judging the quality of the analysis. In addition, there are several important points that need to be better discussed or are even totally missing. We report here these main general comments first, then the list of other specific comments from each committee member.

GENERAL COMMENTS

1) Analysis note

The writing of the analysis note must be definitely improved in several aspects.

- None of the figures and most of the equations are never explicitly referred to in the text. This is not acceptable.
- Please make sure that all variables are defined (even if they seem trivial, such as M in Eq. (1.2). Readers should not have to make assumptions, which we had to do frequently with this document). Also, variables should be used consistently (e.g. x_B is used in Eq. 1.1 and x in Eq. (1.2)).
- Most figure captions would benefit from more detailed information.
- While some information on the details of analysis are lacking, trivial information is given in the note, such as explaining what is a systematic error, or what is a chi^2.

2) Analysis procedure

Several other CLAS analyses have been already published from the e1f data set. The authors should clearly state if the cuts, selection criteria, corrections, etc. used here are the same used also in the previous publications. If not, the differences should be clearly described and justified.

3) Monte Carlo simulations

Monte Carlo simulations are totally missing. The usual assumption that, when computing ratio of cross sections, the acceptance corrections cancel out is true only for totally unintegrated asymmetries. On the contrary, in 1D projections the acceptance corrections might be important, especially when part of the phase space is not covered by the detector. This is the case for example of the kaon momentum (see below the specific comments).

4) Systematic errors

The paragraph on the systematic uncertainties is quite confused. Most of the contributions reported in the tables and figures are not discussed at all. Are they computed bin-by-bin (as suggested by the BSA plots) or integrated (as reported in the tables)? The various contributions should be listed one by one, how it has been treated should be clearly described and the corresponding estimate reported.

SPECIFIC COMMENTS

REFEREE 1

Chapter2:

2.2) E1-F

What is the Torus field polarity (negative charged particle inbending or outbending)? What is the corresponding maximal value for the magnetic field of the Torus (corresponding to 2250A)?

2.3) Determination of Good Run List

I was told this data has already been analyzed. I am first surprised that no good-run list has been produced and made available from the previous analyses. The selection of good runs does not appear to be rigorous. A cut at 3 standard deviations from the mean (of ratio N/Q? so mean of histogram displayed in Figure 2.1?) is applied.

a) Such a cut should be displayed on Figure 2.1.

b) I have assumed that you keep runs between 4000 and 5000 since the cut is not displayed. However it is a 20%-variation in your electron rate. Assuming it is the same trigger, statistical fluctuations cannot explain this 20%-difference.

Now, if triggers have changed from time to time, each trigger-period should be analyzed separately. A statistical error should be assigned to N/Q run-by-run. From this, you can then evaluate the quality of the run.

2.6 Timing corrections

-Where are the paddles excluded from the analysis due to low statistics for tcorr? If there are only paddles on the edge, it could be considered fine. But if you have to remove paddles which cover a significant geometrical acceptance, the consequences of removing it should be studied with a Monte-Carlo simulation.

2.7 Kinematic corrections

It is stated that DC misalignments implies a same correction for theta angle for positive and negative particles. It would be nice to have a plot illustrating this statement. I am not sure that, with the magnetic field, this “symmetry” exists.

Figure 2.7: Is there any reason why the resolution of the bottom right sector (sigma=0.043) is significantly worse than the 5 others (about 0.03)?

Back to timing correction: How does Figure 2.4 change with both timing and momentum corrections?

Chapter3:

3.2 Electron Identification

- Concerning the minimum energy deposit in EC, 60 MeV corresponds to 180 MeV (after sampling fraction correction). Isn’t it a bit low? What is the mean energy deposit in EC of a 2 GeV pion? A plot displaying Edep as function of momentum for pion and electron would be nice.

3.3 Hadron Identification

Figure 3.9: Can you redo Figure 3.9 with a gaussian fit and the parameters of the fit? I guess we can make the cut a bit tighter than +/- 5cm (+/-2 cm)?

- If I understood correctly the pid of the hadron is assigned with the highest value of

Rh(Beta)=Lh/(Lp+LK+Lpi),

with an additional cut on the alpha value. But when p> 2 GeV, I can easily imagine cases where Rpi(Beta_obs) ~ RK(Beta_obs), for instance say 0.47~0.485. It does not seem enough then to select kaons. To complete the set of criteria for hadron identification, I would have expected a cut on the momentum based on a study of misidentifications/mixing fractions of K/p, K/pi+.

Chapter4:

4.2) Event selection:

- I am not familiar with CLAS analyses but Q^2>1 seems fairly low for DIS. I would recommend 1.5 or 2 GeV^2.
-Figure 4.2: What is the peak at 1.5 GeV^2 in the missing mass spectrum ep->EK+X? It really seems that you have a few exclusivity peak in this spectrum.

No empty target run was taken? Assuming that there is no helicity dependence in the K+ from the target wall, it still contributes to the denominator of your asymmetry.

4.3) Phi_h Distributions:

- Table 4.4 is not really clear, especially that it is integrated over all bins. I wish we could have the same for a0, a1 and a2. Doing so, you might see stange behaviour of your results in specific bins.
For instance, I am very surprised concerning the magnitude of EC-U cuts. Any particular reason? Is it uniform for all bins? Or is a bin more sensitive to EC-U than others?

- I am not convinced by the estimate of the systematic uncertainty with the cut on pK. Again a more careful study must be performed on the hadron identification. (See hadron identification)

- How the pion contamination affects the measured Kaon asymmetry? I would use the pi+ result in a Monte-Carlo simulation, generate a known K+ asymmetry and check all the analysis step on this simulated sample.

- Have you looked at the asymmetry per sector?

- Could you check your code by extracting the pi+ asymmetries which has been published?

- In fact, the same dataset has been used for pi+ SIDIS asymmetry. How much of the cuts and methods have you re-used? Most of the cuts (except of course the pion selection) must be the same.

REFEREE 2

Beam helicity reporting is not discussed. Was it delayed reporting? If so, was the algorithm to get the beam helicity verified?

Page 2 2nd line. EMC did not measure g_2. It measured g_1. g_2 has no direct information on the nucleon spin decomposition.

Page 2 5th line: The statement that the spin crisis is largely unresolved is wrong. It is in fact largely resolved since we have a precise experimental determination of the quark spin contribution, measurements of the gluon contribution, and some indication of the importance of the OAMs. All those give a consistent picture that is backed-up by lattice computations. These are now giving accurate numbers for the nucleon spin components. While these numbers are not constrained by the nucleon spin sum rule, they do fulfill it. So we have come a long way since EMC and the situation is much clearer. Please see e.g. arXiv:1807.05250 for a recent review of this question.

Page 10, last line: Is the beam position offset varying? Is 0.15, -0.25cm the average over the whole run period? Beam position is prone to variation and I don't know if a single number averaged over the run period is relevant, vs an event by event correction.

Page 17, Fig. 2.7: Why is the correction not completely successful for sectors 1 and 2 (assuming the top plots correspond to these sector), c.f. Fig. 2.6 too? Why is sector 6 (assuming it's given by the bottom right plot) so much worst?

page 22: what is the reason for the event enhancement in between the sectors?

Page 25, first line: What is the meaning of index i?

Page 25, lines below Eq. 3.4. You say that n_sigma=2.5 is the nominal value. In what cases are you not using this value? If you are always using 2.5, remove "nominal". If not, please give the detailed information of what value is used and when. In Fig, 3.4, you wrote that N_sigma=4, not 2.5. (I assume N_sigma and n_sigma are the same quantiy, although one is capitalized and not the other).

Page 27: why are the cuts asymmetric? One cuts the target window at z \approx 22.5 and the other keeps the other window. This seems an inconsistent treatment.

Page 32: the cut seems unwarranted since it seems to only remove the natural tail expected from such the distribution. In general, cutting within the resolution of a system is not a good idea unless we cannot avoid it. It may bias the statistical distributions and worsen the statistics of the result for no good reason.

Page 33: Is the denomination "confidence level" properly used? Usually, confidence level should be high, not 0.05 or 0.01.

Page 42: Shouldn't it be 1.2*10^4 bins not 120, or is the sentence misleading?

Page 42: Was a correction for beam charge asymmetry applied? Eq. 4.2 should be A_i=\frac{1}{P_e}\frac{n_+^i/Q_+ - n_-^i/Q_-}{n_+^i/Q_+ + n_-^i/Q_-} with Q_+ and Q_- the charges in the respective beam helicity bunch. Q_+ and Q_- may differ at the 10^-3 level, which is not negligible compared to the 10^-2 level of the asymmetry considered here.

Page 43, on the top left panel: the fluctuations seem random and much larger than the stat. error bars (and systematic estimate too). This suggests that the error bars are underestimated or a systematic effect has been neglected.

Less important comments written directly on the note and posted at:
https://userweb.jlab.org/~deurpam/comments_K_Riser_1.pdf

REFEREE 3

Chapter 1

- the eq. 1.1 is valid only for unpolarized targets, otherwise there are much more phi-modulations

Chapter 2

- section 2.3

Is it the standard good run list used by other e1f publications?

- Fig. 2.1

Are all the runs in the figure good runs? If not, what runs have been discarded and why?

- Fig. 2.2

Is the lower plot already corrected for the half wave plate?
In the upper plot, it may help to have a line indicating when the half wave plate has been taken in/out. It could justify why the few runs with negative asymmetry have been accepted as having the correct beam helicity.

- section 2.5

Is this correction run dependent? I can expect that it may depend from how well the beam is centered on the target.

- section 2.6

Why the paddle-to-paddle time correction should be different between pions and electrons?
Also, if that is true, I would expect an even bigger difference between pions (used to calculate the correction) and kaons (for which the correction is applied).

- fig. 2.4

In the left plot, there are several bands below the one that I assume is from protons.
In the right plot, these bands are not there, or at least not so clearly evident. Why?

- Section 2.7

Not very clear. Besides the methods used to calculate the correction, that should be standard for e1f, what momenta did you correct? The electron? The kaon? Both?

- Fig. 2.5, 2.6, 2.7

What is the purpose of these figures? They (should) demonstrate the effectiveness of the correction in general, that we can assume true being it the standard for e1f data.
Why don't you show instead for example the missing lambda or sigma peaks before and after the correction?
By the way, from fig. 2.7 the sector 6 seems to have 40% bigger resolution than the other. Do you know why?

Chapter 3

- section 3.2.1

Why no fiducial cuts on DC region 2? Are they the same for both positive and negative charges?

-page 21, sentence after eq. 3.2

I guess the cut should be y_left < y < Y_right

- Fig. 3.1

The DC fiducial cuts discussed in the previous page have a triangular shape, while from the figure it seems to be trapezoidal, like in fig. 3.2.
Is there any additional cut on the theta angle?

- page 21, EC minimum energy

How the E>60 Mev cut is justified? Do you have a plot?
Do you have any time cuts?

- vertex cuts

How the cuts have been set? For all the sectors, the cuts cut much more on the right side than on the left side.

-cherenkov cuts
The piece of code in page 28 is not relevant.
How the thetaC selection boundaries in Fig. 3.7 have been determined? Is this cut applied?
Do you have any time cuts? Fiducial cuts?

- Section 3.3.1

Why this time no fiducial cuts in region 2 and 3?
Here and in the next sections, you refer to negative hadrons. Unless there are motivations that are not explained in the text, they are not relevant, since you are studying the BSA for K+ only. Therefor better drop it.

- vertex difference, fig. 3.9

How the cuts have been determined? From the plots, they seem to be not justified and they remove very little events.

- page 34, probability density

I understand the method, you want to extract the pion and proton beta resolution from the data using exclusive events. It is not clear instead how you extracted the kaon beta resolution shown in Fig. 3.11.
Where do you cut in the confidence level plots of fig. 3.10?
Can you estimate pion and proton contamination vs momentum in your k+ sample?
Again you mention negative pions and kaons, that are not relevant.

- Fig. 3.13

I guess p^+ means proton

Chapter 4

- Section 4.2

Do you have any lower cut on z? Any cut on y? How other kinematic distributions look like? For example Q2 vs x or W?
In the text you say you have a cut M_X>1.25, but in fig. 4.2 the hatched area labeled as good events has no such cut.

- page 42, binning
You say you choose equal statistics bins, but it seems not so from the statistical errors on the figures.

- figure 4.3 and 4.4
Please add the curves of the fits.
Also, please add an appendix with the same plots for all the other kinematic dependencies of the asymmetry.

- page 44 to 48, systematic uncertainties
Not clear.
Can you explain one by one the various contributions you considered?
Did you calculate the systematics bin by bin (as it seems by looking at fig. 4.3 and 4.4) or integrated (as it seems from table 4.1? And what are the numbers reported in this table?
How these systematics errors are propagated to the final asymmetries of fig. 4.10?
What is the purpose of fig. 4.6 and 4.7?

- page 48
The study of the high kaon momentum cut you mention has nothing to do with systematics, at least not in the way it is described here.
The maximum momentum should be set based on the likelihood analysis (page 34) and choosing a maximum pi+ and proton contamination you can accept. Then, the contribution of this background to the measured BSA should be studied, using both data and monte carlo.
I'm pretty sure that already above something like 2 GeV, you are measuring pi+, not k+ (see fig. 3.11).

- page 50, first paragraph
What minimization method did you use? The chi^2? Other?

- BSA extraction
What is the size of the a1 and a2? And how the value of a0 changes if you simply fit the data with a1=a2=0?

- Fig. 4.9
What is this figure for?

- Fig. 4.10
The missing mass spectrum of fig. 4.2 shows several lambda and sigma resonances, thus it would be interesting to study the BSA also as a function of the MM.
In addition to that, it would also be interesting to isolate the various exclusive channels (lambda, Sigma, Lambda(1530)).