Review Comment Round 2

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

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

We would like to thank the authors for the work accomplished during the last months. Some of the questions from the first review have been answered, some have been unfortunately ignored. We suggest that these questions must be clearly answered to get approval for publication.
We also appreciate that some details have been added to the analysis note, for instance the Monte Carlo studies and the inclusive cross section analysis. However, in some cases these new details raised new questions that we would ask you to reply.

We attach here the list of comments.

## REFEREE 1

Chapter 2:

2.3) Determination of good run list:
As I have written in my first set of questions, the ratio N/Q cannot change from 4000 to 5000 by only statistical fluctuations. There must be other significant factors explaining this variation over the run period such as different trigger configuration. Can you list them?
Depending on them, some subsets of runs may need a dedicated analysis.

Moreover, since we want to extract an asymmetry, how is behaving the ratio Q+/Q-? Or N+/Q+ and N-/Q-? Would it be possible to have a timeline of these quantities as a function of the run number, with highlighted range (or a color code) to know which runs have been used in this analysis.

2.6) Timing corrections:
Just for curiosity, I was wondering why protons are not used to determine tcorr? It would be a good way to cross check or maybe improve the results obtained with pions.

Chapter 3:

3.2) Electron identification:
Thank you for adding figure 3.4. Looking at figure 3.3, we clearly see a dead band in top right sector. It is definitely because of such a kind of details that one must perform a dedicated analysis of the sector, or apply some corrections with a Monte-Carlo simulation. How was handled this inefficiency?

Figure 3.5: Can we have this figure once all other electron ID cut have been performed? Because we clearly see a cloud of events with a decreasing sample fraction as a function of p which might not be electrons. Does this cloud of events vanish once we apply all other cuts?
Same request for Figure 3.4 please.

3.3) Inclusive cross sections
Looking at Figure 3.9, I cannot believe the 5%-difference between prediction and experimental results stated in the “results” section. Sector 1 and sector 5 are seriously off at low Q^2. (Not easy to spot despite the log scale).
I would recommend to plot all sectors on a same graph for the same Q^2 value, it will make the comparison easier for reviewers (there will be no need for a log scale since it will be same Q^2.) I would say that this inclusive cross section study raises more questions than provides answers.

3.4) Hadron identification
There is a typo: you fit with a second order polynomial the mean and standard deviations for your likelihood ratio.
To fit the kaons beta vs p spectrum, would it not have been better to fit the entire spectrum but fixing the protons and pions parameters?
Can you show the equivalent of figure 3.14 but with your anti-selection of protons and pions?

Concerning the 20%-contamination of pions, I would like to emphasize that an asymmetry is a ratio. If we assume that the asymmetry for pions is 0, you still have a 20%-pions at the denominator of your asymmetry which must be taken care of. Otherwise you are wrong by 20% on the asymmetry.
Even if you stated that it is not for precise measurements, it is not acceptable to leave this 20% pion contamination diluting your asymmetry.

Chapter 4: Event selection
I am very confused about the cut on the Missing mass squared to remove exclusive resonances:
1) Why do you want to remove them? In principle, for SIDIS, you just require the K+ and forget about the remaining final state.
2) Moreover, this cut removes the resonances at low mass but keep the strong lambda resonance at 1520 MeV (Thank you for the information). Why do you want to keep this one? For consistency, I would say that either you remove all resonances, or keep them all. In the current case with this cut on the missing mass, I am not sure you provide an unbiased asymmetry.

Looking at your reply, it seems you wanted to comment more on the missing mass dependence of your result.

4.3) Phi distributions.
Please correct Eq 4.2 to include Q+ and Q-.

I like the figures 4.8, 4.9 and 4.10.

Why haven’t you looked at the asymmetry per sector? I think it must be studied considering the dead band in ECAL and the variations from one sector to the other of the inclusive cross sections.
I wish to check them for the next round of replies. Thank you.

4.4) Extraction of modulation
Why do you have these singularities in Figure 4.13 and 4.14? Are you looking at fit replicas with eq 4.9? (are these the unstabilities mentioned when you keep a1 and a2? Why is the figure not referred inside the text?)

REFEREE 2

I disagree with General reply #1: this vertex cut does not remove events that actually originate from outside the target. As far as I can tell from Fig. 3.11, these events are reconstructed outside the target because of detector resolution.

In general, cutting within a detector resolution is not recommended, since it would biased some of the statistical distributions of correlated variables. Unless a good reason is given, ex. known contaminating material such as target windows, this cut should be removed.

It is good to study the effect of such cut for systematic study purpose (e.g. studying possible bias from contamination) but unless there is a compelling reason, the cut should not be used.

Regarding the charge asymmetry, it is stated that it was studied by W. Gohn for E1-F and found to be small (0.3%) compared to the size of your statistical uncertainties. Could you clarify:

- What are the smallest statistical uncertainties? For example, on Figs. 4.13-4.14, some of the statistical errors seem to be below 1%, maybe not far to 0.3% (it's hard to read) and thus implying that the charge asymmetry should be corrected for.
- 0.3% is larger than most of the other systematic uncertainties listed in Fig. 4.8-4.10. From these tables, it does not seem negligible. So it still seems to me that this correction should be applied, especially since it is simple to implement.

Several variables are not defined: page 6:
$\alpha_e$, $l$, $l'$, $P$, $P_h$ and $M$ in Eq. 1.1
$M_h$, $\hat{h}$, $k_T$ and $p_T$ in Eq. 1.6
$e_A$, $P_{h\perp}$, $\omega$, $f^a$ and $D^a$ in Eq. 1.7
Also, for consistency with the rest of the document, $x_B$ in the rho of Eq. 1.1 should be $x$

Page 7: First line, second paragraph: fix reference link

Page 43: define $z_h$

Page 47: Please add the 2.4% (relative? absolute?) uncertainty on the beam polarization of 74.9%.

REFEREE 3

General comments

1) There are still a lot of figures never cited in the text. Also, the caption of the figures quite often doesn't describe what is shown, which has to be just guessed from the labels of the plots.
2) The text shows many unclear references (e.g. last row of page 19, EC min. energy cut, what is 3.2.1?)
3) Thank you for adding some monte carlo studies, this is really important, however now that you have these data they must be used in full glory. For example:
    a. how the MC data compare with the experimental ones? this is crucial for the reliability of the kaon efficiency and contamination estimates

b. you should study acceptance effects on the asymmetry extraction by putting in the generated MC data some asymmetry and verifying that at the end of your analysis you get beck what you put in. This can be particularly relevant to study the effect of the kaon momentum cut

4) As far as I know, the replica method in normally used with monte carlo data, as it is also shown in the reference paper 14. In the real data, you always have the acceptance folded in in all the replicas you are generating. How this will affect your results? Again, use the MC to study this effect.
5) Inclusive cross section. The Fig. 3.9 raises a lot of questions, because the comparison between points and curves is not good, especially at low W. But my main question is: is it relevant for your analysis? The way it is showed, I would say no because the kinematics is totally different (W<2 instead of W>2). And in first approximation, the asymmetry should not depend much on the electron acceptance. However, Fig. 3.9 shows large differences among the sectors, therefore I would ask you to check this in your data. Can you produce asymmetry plots when the kaon is detected in each of the sector separately?

Other comments

CHAPTER 1
page 7, line 6: missing reference

page 7, lines 9-10: I’m pretty sure the Hall A measurements is on the He3, while Hermes should be on proton. Please clarify.

CHAPTER 2
section 2.7, pages 12 to 16: ok, this shows how you correct the elastic electrons, but the kinematics of your electrons is totally different. Can you show how much the correction improves your electrons? For example, how the missing mass resonances of Fig. 4.2 are before and after the correction?

CHAPTER 3
page 19, EC min. energy cut: the pions don’t shower

page 19, sampling fraction: The correct definition of the sampling fraction is the total deposited energy divided by the particle’s energy. For electrons it can also be written Edep/p, but not for low energy pions

page 24, z-vertex cut: it doesn’t make sense to have a cut with 4 decimal digits, please round it to 1 digit (or to whatever is the sensitivity of the vertex measurement).

page 27, Cherenkov cuts: you describe how you do matching on phi_rel, but what about theta_cc matching?

page 30, binning: you describe the bin migration effect, but what you do with that then? (Not to be pedant, but if you consider this relevant, monte carlo can help)

page 32, hadron-electron vertex cut: a cut corresponding to the target length doesn’t seem reasonable. Suppose the electron is produced on one longitudinal edge of the target, then your cut can accept hadrons with production vertex way off from the target volume.

page 36, first line: please remove the sentence “Then a cut … neutron mass”. In fact, to select e p pi0 events you should cut on the pion mass.

page 40, validation of kaon id. Thank you for adding this monte carlo study, but I have few questions here.
eq. 3.18, why did you cut only on deltaP and not also on the angles? How the 0.05 cut has been chosen? Does it depend on the momentum?
eq. 20, I can understand left and right side, but what is f_n?

page 42
The 20% pion contamination deserves some more discussion. How did you include it in the systematics? How your asymmetry is changing by reducing this contamination to 10% or even less? Perhaps you would need larger kinematics binning to do so.

CHAPTER 4
page 43, event selection, line 6: factorization -> fragmentation

page 45, missing mass: I pose the question again, can you show the asymmetry as a function of the missing mass, in particular in the bins where the resonances are?

page 48 to 55, systematics uncertainties
You make a detailed introduction on how the 1 sigma systematics uncertainty should be computed, but then:
eq 4.6 is it for +/-1 sigma or for +/- 0.5 sigma?
the variations in the cuts in tab. 4.1 look more some “random” variation than the 1 sigma variation considered in the introduction. How did you fix them?

page 56, extraction of modulations, line 2: you are extracting structure function ratios (or moments), not the structure functions.