Questions/To-do

1 Questions

1.1 Question 1: How well do we need to tune the simulation (HAMC)

This has everything to do with the radiative correction. Let's first review how the radiative correction factor is calculated. While reading others' thesis, I got confused on which of the following two is the correct way to calculate the radiative correction:

$$f = \frac{A(< Q^2_{data} >, < x_{data} >)}{< A(Q^2_{vx}, x_{vx}) >} \tag{1}$$

$$f = \frac{A(< Q^2_{hamc} >, < x_{hamc} >)}{< A(Q^2_{vx}, x_{vx}) >} \tag{2}$$

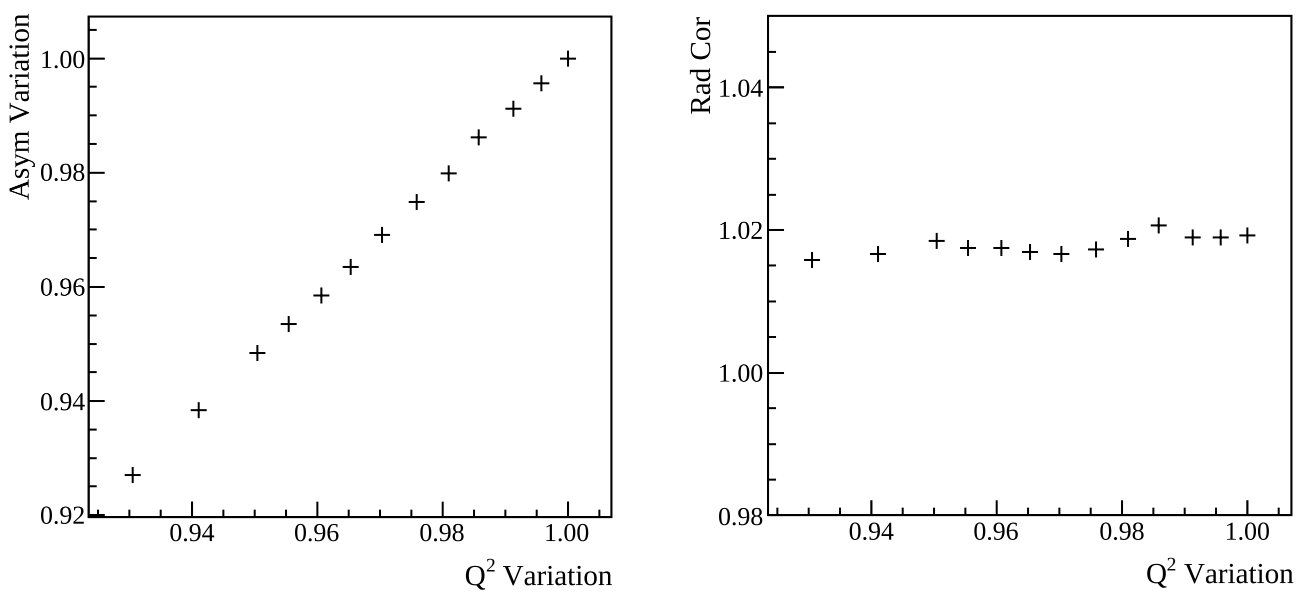


Figure 1: Radiative correction factor stays stable as the simulation changes slowly

I think Eqn. 2 is the correct one(it is what I've been using to calculate radiative correction, but I implied differently in the SOM writing). The difference between the two is what Q^2 and x mean values, from data or from HAMC, to use for the point calculation. Eqn. 1 is actually a measure on the combination of radiative correction and the disagreement between data and hamc. Since asymmetry is proportinoal to Q^2 (see Figure 1 left plot), 1% of disagreement is then directly translated into 1% of radiative correction by Eqn. 1. It should not be the case, because radiative correction is mainly defined by the acceptance, which is an intrinsic character of the spectrometer, and should remain relatively stable if we vary the kinematics slightly. Figure 1 right plot shows how the correction factor calculated using Eqn. 2 changes as we vary Q^2 by changing the incident beam energy, and one can see that the correction remains stable even as Q^2 is changed by 5%.

So to sum up, I think the radiative correction should be calculated as in Eqn. 2, and we only need to tune the simulation relatively well, i.e., even 5% of discrepancy is acceptable (in practice, we can always achieve 1%). Based on this argument, I don't think the absolute comparison on kinematics between HAMC and Data is relevant, or important for anything.

1.2 Question 2: How good is the pairwise pull plot?

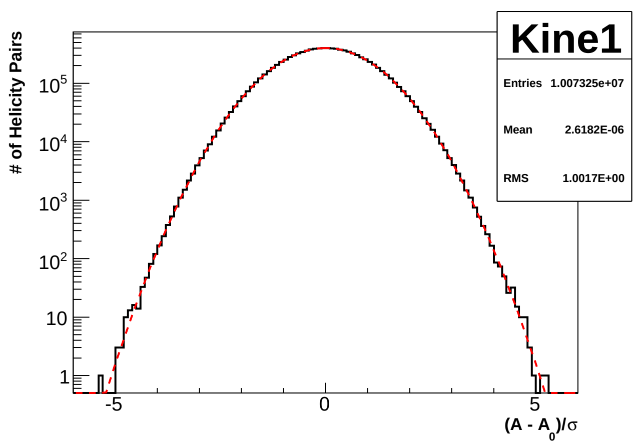
This concerns the last two figures in the NIM paper.

The pairwise pull plot plots the following:

$$pair\ pull \equiv \frac{(A_i - A_{mean}) \times 10^{-6}}{\sigma_i} \tag{3}$$

The 10^{-6} appears because the asymmetries have units of ppm. The distribution of such plot resembles a normalized gaussian distribution (i.e. mean 0, rms 1), which is what we see. The problem is this: if we arbitrarily change A_{mean} by, say, 50ppm (for DIS1), which is a very big change meaning that our measurement is far off, we still get a ok-looking normalized gaussian distribution, except that the mean value becomes farther off zero. So in the sentence "One can see that the asymmetry spectrum agrees to five orders of magnitude with the Gaussian distribution" in NIM paper, "five orders of magnitude" may be an over-statement?

OK... now I think this question is a little over critical so you can ignore it as you like.



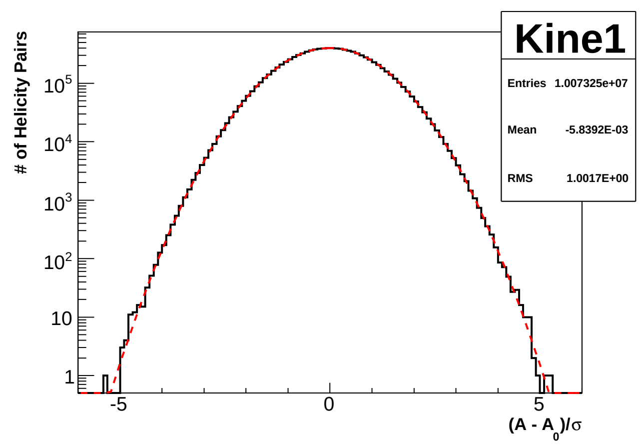


Figure 2: Pairwise pull plot for left kine 1(left plot), and the same plot with mean value shifted by 50ppm(right plot)

2 To-do

I'm summarizing all the things in the to-do list, and answering some of the questions. I'll be updating results here too.

Questions answered/solved are noted by $\surd$.

2.1 $\surd$ Need all run-by-run deadtime correction for DIS (run-by-run values missing, uncertainties can be derived from the 100uA values)

The run-by-run values are already provided in my initial som write-up, page 26.

2.2 Need all run-by-run deadtime corrections for RES (values are in elog, but missing uncertainties)

OK, let me think about it..

2.3 Overal electron rates for all DIS and RES - as suggested by Bob: should list overall rates for all DIS and RES to show that the deadtime is proportional to rates in the first order.

OK.

2.4 $\surd$ Need Q2 and W of kine 7b to send to theorist for APV calculation (2 res theorists, J. Erler for C1,2)

Already provided.

2.5 $\surd$ Need asymmetry results of Kine 7b

See resonance.pdf

2.6 $\surd$ Dithering correction table for BPM individual values is for narrow or wide (Table 3)? Should we list also for resonances?

See asymmetries.pdf and resonance.pdf for complete summary, including both narrow and wide, for all kinematics.

The dithering analysis has been revised so that:

- Total dithering correction is a simple sum of individual corrections from each BPM. $A_{cor} = \sum_{i=1}^{5} \delta A_i$.
- The relation $A_{cor} = A_{raw} - A_{dit}$ is now clear.
- The uncertainty of dithering correction can be estimated by the difference between A_{dit} and A_{reg}.

2.7 $\surd$ Dithering correction table with the asymmetry results (Table 4), what are the correction errors for the wide path? What about resonances?

See above.

2.8 √ need separate values for Compton and Moller for DISs (Table 5);confirm Compton, Moller, and COmbined values for RESs (Table 6).

As I mentioned earlier, we don't apply Compton and Moller seperately. Instead we combine them first and then apply run by run. Maybe we can just use one table summarize the final correction.

2.9 need comparison table (HAMC vs. data) of Q2 and x for DIS (in Table 8).

OK, but as I mentioned in Question 1, I don't think such comparison is important, as long as they agree "relatively well".

2.10 √ Need beam depolarization effect for all RES kinematics (line 807-808, below Eq.(45)).

The Depolarization corrections are summarized in Table 1. The numbers for DIS are slightly different from in the Elog entry because of the high trial numbers running HAMC (1M v.s. 200K previously).

	DIS 1	DIS 2	RES 3	RES 4	RES 5	RES 7	RES 7b
Corrections	0.096%	0.209%	$4.9e^{-5}$	0.028%	0.093%	0.061%	0.081%

Table 1: Depolarization Corrections.

2.11 √ What is the form factor used in the e-p and e-n elastic (part of the quasi-elastic) asymmetry evaluation (line 847-851)?

Form factors are directly borrowed from HAPPEx code, which are used for both asymmetry and (weighting) cross-section calculation.

2.12 √ "Currently the average of $e-p$ and $e-n$ asymmetries is taken as the quasi-elastic asymmetry. This will be corrected when we are ready for another round of HAMC simulations." – has this been corrected?(the formula of A_d says yes, but please confirm)

It has been corrected.

Comment: the $e-n$ cross section is significantly smaller than $e-p$ (due to electric form factor?), but their asymmetries are almost the same, so the effect of weighted average is small.

2.13 √ For the toy model of the resonances, how was $sigma_dis$ calculated? Was it from PDF fits, or NMC fits (line 856)?

It is from NMC fits.

2.14 Need radiative corrections for resonances -¿ Table 16.

Preliminary results can be found in resonance.pdf. Further discussion necessary.

2.15 $\surd$ Need RES7b results -¿ Table 15.

See resonance.pdf. Enough for Table 15, but still missing deadtime.