Preliminary Study of Asymmetry Slopes for Deuterium and Hydrogen

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April 4, 2000

Below is a short report summarizing a comparison of asymmetry slopes for our 98 hydrogen and 99 deuterium data. The motivation is twofold. Firstly, recall that last year we spent a significant amount of time trying to understand why there were residual correlations between the yield asymmetry and the monitor asymmetries (particularly for the LUMIs), which eventually led us to our 6th parameter. Such an analysis has not been performed on the deuterium data. In the interim, Damon modified the analysis code to calculate asymmetry slopes (dA_Y/dA_p) which are now written to a summary file. So it is now rather straightforward to look at these slopes as a function of run number and look at mean values for the entire run.

Secondly, I decided it might be worth plotting these to look for clues as to why there is a ϕ dependence to our H_2 shutter closed data that is absent from the D_2 data. So far I haven't found anything particularly useful.

Rather than draw too many conclusions yet, I thought I would just get the information out for discussion. Below I just remind us how the asymmetry slopes are defined, present a table of results, and a few observations. Plots are also available at the same location as this file.

I. Procedure and definitions

Expressions for the correlation between the yield asymmetry and the parameter asymmetries were outlined by Mark and Bob in several short notes about a year ago. To make this document self-contained, I will write them out again here. (See Mark's Feb 13, 1999 and March 26, 1999 notes for more detail.) As with the yield slopes, the asymmetry slopes are found by inverting the following matrix equation:

$$\langle \delta A_Y \delta A_{Pj} \rangle = \sum_{i=1}^{6} \langle \delta A_{Pj} \delta A_{Pi} \rangle \frac{\partial A_Y}{\partial A_{Pi}} \,. \tag{1}$$

The above expression is not quite what is in the code: the slopes are put into a dimensionless form by dividing the correlation terms on both sides by asymmetry widths. This was suggested by Mark to allow for a more meaningful comparison between parameters. It also is consistent with estimating the errors in both the parameters and the yields from the measured widths.

$$\frac{\langle \delta A_Y \delta A_{Pj} \rangle}{\Sigma_{A_Y} \Sigma_{A_{Pj}}} = \sum_{i=1}^{6} \frac{\langle \delta A_{Pj} \delta A_{Pi} \rangle}{\Sigma_{A_{Pi}} \Sigma_{A_{Pj}}} \left(\frac{\partial A_Y}{\partial A_{Pi}} \right) \left(\frac{\Sigma_{A_{Pi}}}{\Sigma_{A_Y}} \right) \,. \tag{2}$$

So what actually gets written out in the summary file as the asymmetry slope is $\left(\frac{\partial A_Y}{\partial A_{Pi}} \right) \left(\frac{\Sigma_{A_{Pi}}}{\Sigma_{A_Y}} \right)$. The error in this quantity is the error in the slope scaled by the same ratio of widths and divided by $\sqrt{N_A}$. Note that in the analysis to date, there is a bug in the code that made the sixth parameter asymmetry slopes incorrect: they can be made correct by multiplying them by $\frac{\Sigma_{A_{LUMR}}}{\Sigma_{A_Y}}$.

II. Results

In the table below are the asymmetry slopes, averaged over the entire data sets for hydrogen and deuterium. In each section of the table the first two rows correspond to the slopes for the uncorrected MIRSUM signal, whereas the latter two columns are for the 6-parameter corrected data. (There are no output results for uncorrected 6th parameter slopes.)

The hydrogen data are from runs 2162 to 3819. There are a few odd runs with somewhat larger slopes. We might consider ntupling those runs to look for clues in our shutter closed data. The deuterium data are from runs 1551 to 3009, but many runs were thrown out because the slopes were overwritten with garbage when ntuples were generated for those runs. Note also that there are no numbers in the XPOS column for D_2 because something went wrong in the summer 99 analysis that caused these values to be infinitely large all the time. This didn't happen the H_2 analysis, and Damon has so far been unable to find a bug in the code that would cause this.

There are a few comments to make about the numbers in the table. Note that they have all been multiplied by 100 to make them easier to look at. So they are all small compared to unity. I did not include the errors in these quantities, but, as can be seen on the fraserplots, they are typically a few percent of the value.

- There doesn't seem to be much difference between IN and OUT in all cases. Not even a change in sign.

- For the position and angle differences, the corrections procedure seems to do a good job of correcting away any correlations between MIRSUM and the horizontal helicity correlated beam motion. The corrections in the vertical direction are not quite as good. One speculation I had is that the motion in the Y-direction is generally smaller, so that our ability to determine the YPOS slopes may not be quite as good. It could be a concern for deuterium because the YPOS slopes seem to be somewhat larger.

- There seems to be very little correlation with energy in all cases.

- The residual correlation between AMIRSUM OPEN and the 6th parameter (NTTG2) could be a concern. It appears to be much smaller in the D_2 data.

The next question is what to do with this information. It would be interesting to make a similar table for the LUMI data. As suggested by Bob, it may also be interesting to estimate how the UNCORRECTED correlations compare to what we might expect from physics effect (solid angle vs. (x, θ, y, ϕ), e.g.). For example, the shutter closed data yield is primarily from relatively forward angle scattering (particles going directly toward the phototubes), whereas the shutter open yield is mostly backward angle scattering, so the change in solid angle with change in beam position could be different. This is work in progress.

Y	xpos	xtheta	ypos	ytheta	ecsx	sixth
H2 (multiplied by 100)						
uncorrected CLOSED IN	-1.05	0.42	-2.66	-0.60	-0.25	
uncorrected CLOSED OUT	-1.00	0.43	-2.16	-0.44	-0.17	
6P corrected CLOSED IN	-0.07	0.05	-1.68	-0.40	-0.016	-3.56
6P corrected CLOSED OUT	-0.05	-0.16	-0.66	-0.18	-0.005	-4.46
uncorrected OPEN IN	-1.45	1.04	-0.37	-0.09	-0.004	
uncorrected OPEN OUT	-1.39	0.90	0.36	0.02	0.002	
6P corrected OPEN IN	0.17	-0.24	1.12	0.23	0.007	-11.8
6P corrected OPEN OUT	0.16	-0.27	1.01	0.17	0.008	-11.2
D2 (multiplied by 100)						
uncorrected CLOSED IN		1.11	4.43	1.35	0.022	
uncorrected CLOSED OUT		1.36	4.42	1.40	0.026	
6P corrected CLOSED IN		-0.09	1.93	0.60	0.009	-0.51
6P corrected CLOSED OUT		0.23	2.50	0.79	0.014	-0.41
uncorrected OPEN IN		1.71	8.29	2.51	0.042	
uncorrected OPEN OUT		2.14	9.44	2.99	0.056	
6P corrected OPEN IN		-0.63	4.73	1.45	0.02	-1.49
6P corrected OPEN OUT		1.26	5.49	1.75	0.03	-0.74

The graphs

It is also interesting to look at these quantities on a run-by-run basis. The graphs are not attached but will be made available for downloading along with this note.

In the hydrogen data, one can see that the vertical slopes are considerably noisier in the data before the piezo was working. In all cases, the corrections procedure seems to add noise, although it does reduce the mean value.

In the deuterium data, long term correlations between A_Y and both the vertical parameters and the energy are clearly evident. The energy corrections seem to get rid of them, but the vertical motion is not as well corrected away. Again this may be due to the fact that our measurement of the vertical slopes may be inferior because there is typically less beam motion in the vertical direction. Someone could probably confirm this with a 2D plot of A_Y vs. A_{ypos}, and yield vs. YPOS, from an ntuple (I don't have any D_2 ntuples).

Hopefully a more quantitative analysis will follow.