Asymmetry Slopes for Deuterium and Hydrogen, Part II

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Below is a short report summarizing a comparison of asymmetry slopes for our 98 hydrogen and 99 deuterium data. This is a followup to the early April 2000 note. In order to make this document self-contained, I have repeated the notation and the table of results.

The executive summary is that, with the exception of the anomalously large sixth parameter slopes seen in the Hydrogen data, these residual correlations between the beam parameters and MIRSUM asymmetry are small and should result in a negligible uncertainty. I will attempt to show this with a few examples.

In attempting to address our large H_2 shutter closed systematic error, I also studied the yield slopes. In the 2nd half of this note I will make a radical proposal for decreasing the size of our systematic error, based on my examination of the yield slopes and on the observed $\cos 2\phi$ dependence of the H_2 data. This is meant to stimulate discussion rather be a final proposal.

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 2163 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. Alice's new analysis of the D_2 data gives consistent results for all the slopes, except that she has reasonable values for XPOS as well, so apparently whatever went wrong during the summer has been fixed.

Note again that all the numbers in the table have 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.

In principle, one could use these numbers to estimate a residual false asymmetry that we are not accounting for in our corrections procedure:

$$(\delta A)_{sys} = \left(\frac{\partial A_Y}{\partial A_{Pi}}\right)(\Delta P_i) = \langle \mathrm{corr} \rangle \left(\frac{\Sigma_{A_Y}}{\Sigma_{A_{Pi}}}\right) \Delta P_i \tag{3}$$

where ΔP_i is the helicity correlated monitor difference and $\langle \mathrm{corr} \rangle$ is the quantity in the table. Consider for example the position monitors: typical values of the monitor difference widths result in $\Sigma_{Pi} \sim 50 - 150\ \mu$m, and typical yield asymmetry widths are 0.38 (1.3)% for H_2 OPEN (CLOSED) and 0.3 (0.8)% for D_2 OPEN (CLOSED).

Using a typical parameter difference of 100 nm in 1 run, or 10 nm averaged over the entire data set, one can estimate the residual false asymmetry. For example, for the one run of the D_2 OPEN data and the YPOS monitor,

$$(\delta A)_{sys} = \frac{(0.05)(0.003)(10^6)}{(50 \times 10^3)}\ \mathrm{ppm/nm}\ \times\ (50\mathrm{nm}) = 0.3\,\mathrm{ppm}\,. \tag{4}$$

Averaged over the summer this would be about $\times$ 10 smaller. This example represents about the worst case for the D_2 data. Since we cannot use the LUMI data to estimate our systematic error on the corrections procedure as we did in the case of H_2, it seems to me that we would be safe in estimating the magnitude of the systematic error to be about the same in D_2, since there are no residual correlations that would warrant a larger error.

Mark rightly points out, however, that this is only valid in the limit that there is no seventh parameter for which we have no information. Recall the notes by Bob and Mark about temporal correlations between parameters.

However, for the H_2 data, the residual correlation between AMIRSUM and ATTG2 is potentially troublesome. In this case, for the OPEN data averaged over the experiment, we

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

had a 1 ppm toroid asymmetry, which results in

$$(\delta A)_{sys} = \frac{(-0.11)(0.038)(10^6)}{(4000)}\,\text{ppm/ppm} \times (1\text{ppm}) = -0.10\,\text{ppm}\,. \tag{5}$$

The result is about the same for the CLOSED data, is about a factor of 2 larger than our present systematic error due to the corrections procedure, and is comparable to our OPEN statistical error, so it probably warrants more close investigation. (Damon has double-checked that this quantity is being properly calculated in the code.)

III. Comments on the Yield Slopes

I also spent some time investigating the yield slopes for the CLOSED H_2 data. Below are some facts that I uncovered. Please see the attached figure for reference. The relative errors on the slopes are again very small.

1. For a given monitor, the slopes of all mirrors have the same sign. (For example, the YPOS slopes are all negative.) This is inconsistent with the slopes resulting only from

geometry, such as a change in solid angle.

2. Typically, the magnitude of the measured asymmetry is not accounted for by the product of the (even uncorrected) slope and the helicity correlated beam motion. For example, the measured raw CLOSED asymmetry of MIR2 is -10 ± 4 ppm. The (normalized) measured slope (averaged over the run) in YPOS is $\frac{1}{2S}(\partial S/\partial P)_{YPOS} =-.01$ ppm/nm. It would therefore require an average helicity correlated beam motion of 1000 nm to make a -10 ppm asymmetry. Although this is only one of 6 measured parameters, all six would have to conspire to add up to -10 ppm, which is unlikely.

3. The magnitude of the position slopes are consistent with the magnitude of the angle slopes, that is, from geometry one expects that a 1 ppm/nm sensitivity to position corresponds to a 10 ppm/nrad sensitivity to angle. So position and angle are likely strongly correlated.

4. Another interesting thing to consider is the sensitivity to similar types of motion between mirrors 2 and 4. For example, one might expect that MIR4 should have the same sensitivity to vertical motion as MIR2 does to horizontal motion, and vice versa. This appears to be true for motion that is perpendicular to the mirror, but not motion that is in the same plane as the scattering angle of the mirror. For example (all are in ppm/nm):

$$\frac{1}{2S_4}\left(\frac{\partial S_4}{\partial P}\right)_{YPOS} = -0.004\,,\ \frac{1}{2S_2}\left(\frac{\partial S_2}{\partial P}\right)_{XPOS} = -0.002 \tag{6}$$

whereas

$$\frac{1}{2S_4}\left(\frac{\partial S_4}{\partial P}\right)_{XPOS} = -0.0014\,,\ \frac{1}{2S_2}\left(\frac{\partial S_2}{\partial P}\right)_{YPOS} = -0.01\,. \tag{7}$$

All mirrors are more sensitive to motion in Y than in X, but the "vertical" mirrors seem to have significantly more sensitivity to vertical beam motion.

This last fact could be caused by a small amount of scraping on lower portion of the inner tube of the target. All detectors would likely see a drop in yield, if the beam were to move up, but it is conceivable that the "vertical" detectors would see it more dramatically. It might also come from a change in vertical beam size (which could change the mean location of the beam).

If there were a helicity correlated quadrupole-type breathing mode to the beam, (X gets smaller as Y gets larger), one might be able to construct the $\cos 2\phi$ dependence we see in the CLOSED asymmetry because of the magnified sensitivity of the vertical mirrors in one direction vs. the other that the horizontal mirrors do not have.

This leads me to my radical proposal. I believe, although I cannot account for it quantitatively, that unmeasured changes in the beam shape could give us the mirror-by-mirror asymmetry we see in the CLOSED data. (The ϕ-dependence plots are attached.) By fitting the asymmetry to the form $A(\phi) = A_0 \cos 2\phi + A_1$ we can separate the ϕ-dependent part from the overall constant offset, and the reduced χ^2 is much better. For the CLOSED data, A_1 is very consistent with 0. For the OPEN data, A_1 is very consistent with the result we get when we add a mirror-by-mirror systematic error, so performing this fit would not change the H_2 result. In addition, χ^2/dof does not improve much by going to a ϕ-dependent fit so it's not so clear whether one use it. I therefore propose that we take as our systematic error the difference between -5.21, when the data are fitted to a constant, and -5.08, when the OPEN data are fitted with a ϕ dependence, of ± 0.2 ppm. This procedure would also be consistent with no systematic in the D_2 case where the two fits give identical results.

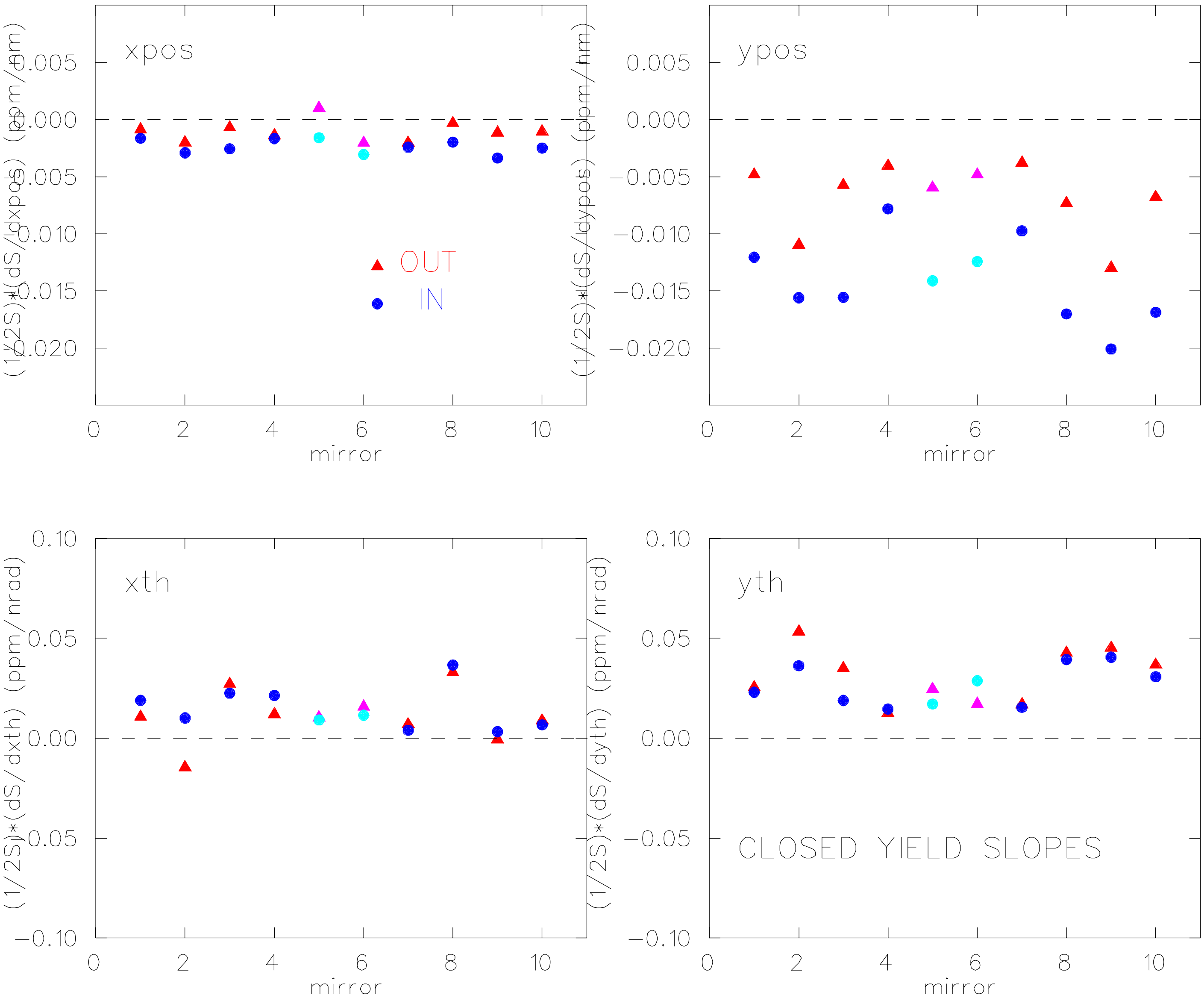
The case for this proposal would be stronger if the yield slopes are better behaved in both the H_2 OPEN and all of the D_2 data. The D_2 data have yet to be investigated, but I briefly looked at the H_2 OPEN data. The slopes in the Y- and X- directions are more consistent with each other, and in particular the Y-slopes change sign between the top and bottom detectors. So it may be more likely that these slopes are driven by geometry or physics.

For example, for the OPEN data

$$\frac{1}{2S_4}\left(\frac{\partial S_4}{\partial P}\right)_{YPOS} = -0.002\,, \; \frac{1}{2S_2}\left(\frac{\partial S_2}{\partial P}\right)_{XPOS} = -0.002 \tag{8}$$

and

$$\frac{1}{2S_4}\left(\frac{\partial S_4}{\partial P}\right)_{XPOS} = -0.001\,, \; \frac{1}{2S_2}\left(\frac{\partial S_2}{\partial P}\right)_{YPOS} = -0.002\,. \tag{9}$$


xpos
ypos
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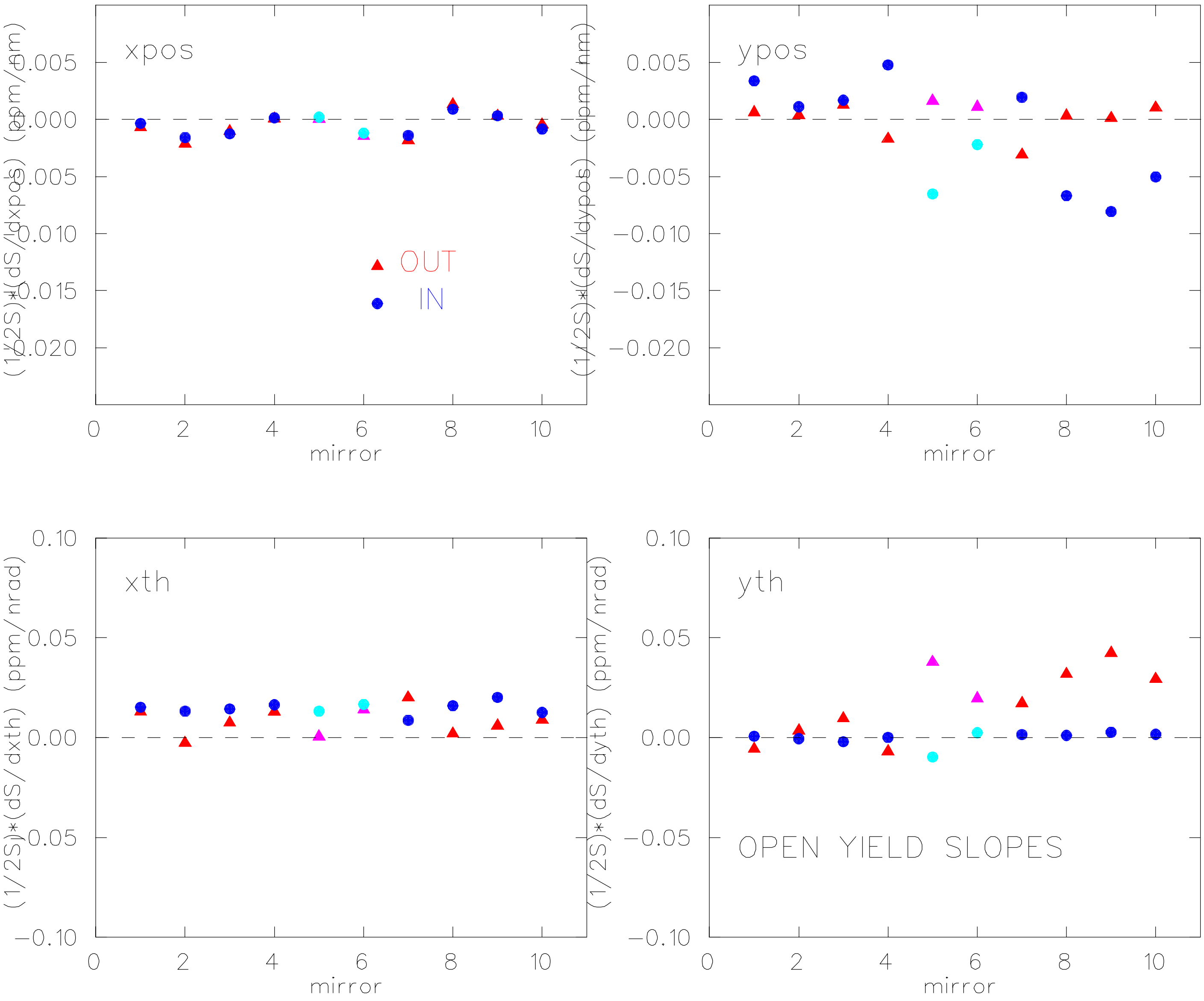
yth
OUT
IN
CLOSED YIELD SLOPES
(1/2S)*(dS/dxpos) (ppm/nm)
(1/2S)*(dS/dypos) (ppm/nm)
(1/2S)*(dS/dxth) (ppm/nrad)
(1/2S)*(dS/dyth) (ppm/nrad)
mirror
0.005
0.000
−0.005
−0.010
−0.015
−0.020
0.10
0.05
0.00
−0.05
−0.10
0
2
4
6
8
10


xpos
ypos
xth
yth
OUT
IN
OPEN YIELD SLOPES
(1/2S)*(dS/dxpos) (ppm/nm)
(1/2S)*(dS/dypos) (ppm/nm)
(1/2S)*(dS/dxth) (ppm/nrad)
(1/2S)*(dS/dyth) (ppm/nrad)
mirror
0.005
0.000
−0.005
−0.010
−0.015
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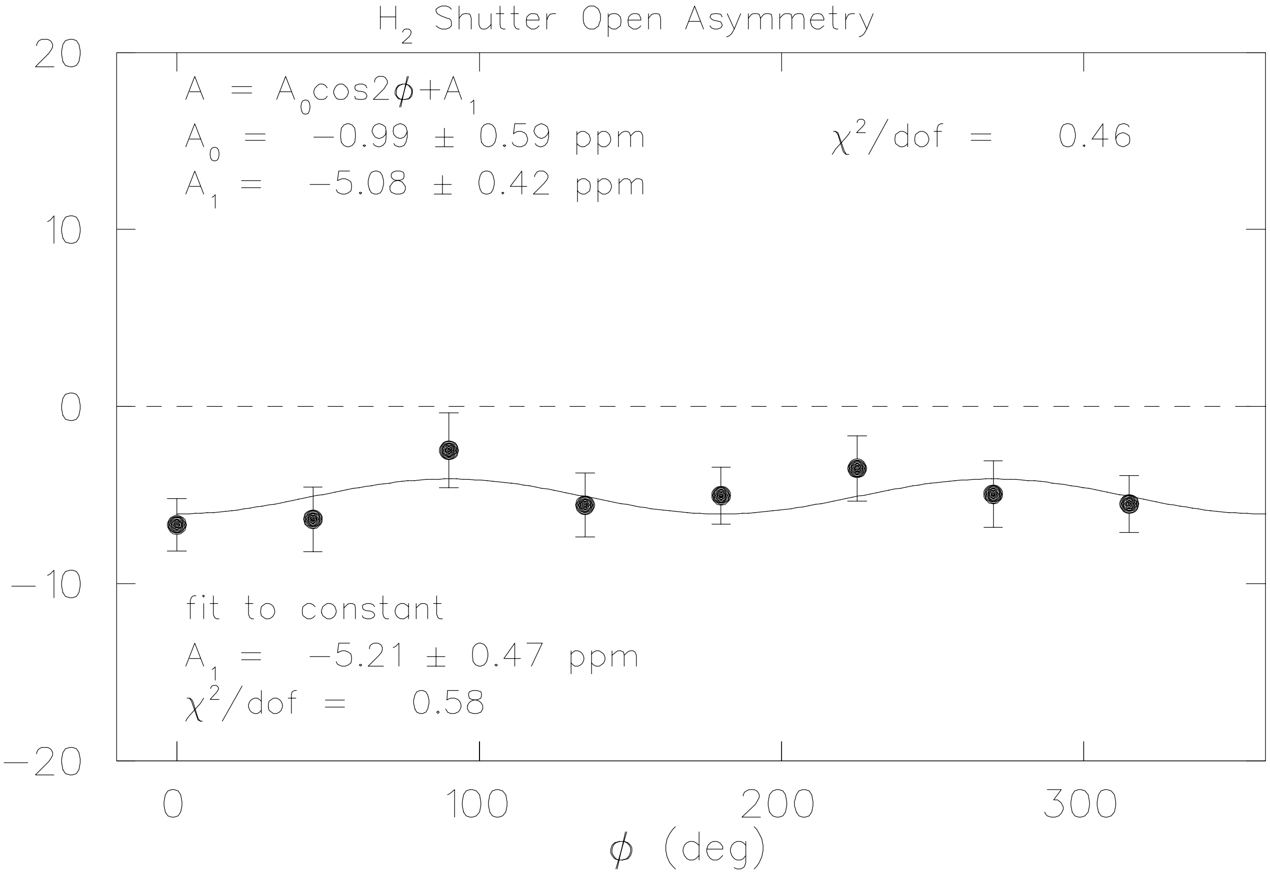
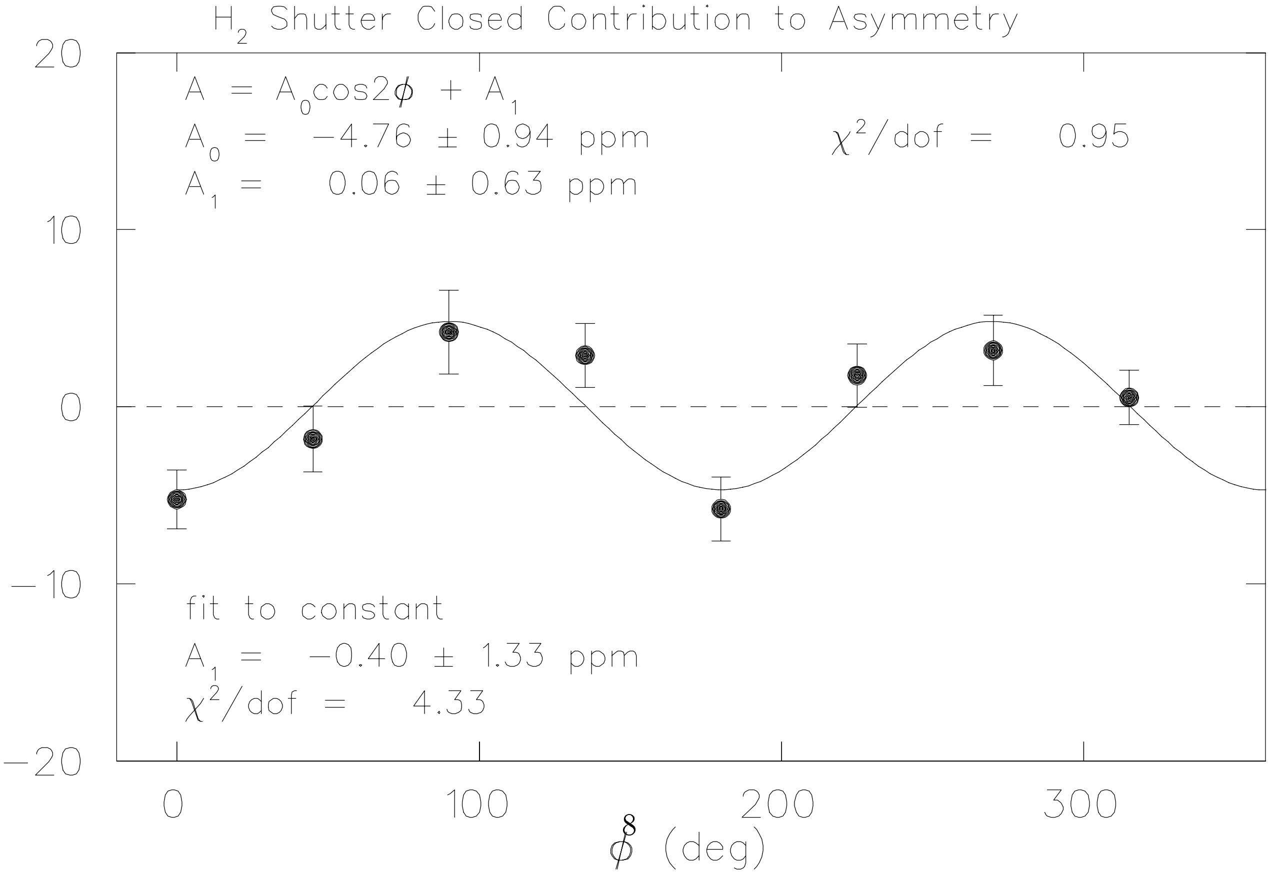


H2 Shutter Open Asymmetry
A = A0cos2ϕ+A1
A0 = −0.99 ± 0.59 ppm
A1 = −5.08 ± 0.42 ppm
χ2/dof = 0.46
fit to constant
A1 = −5.21 ± 0.47 ppm
χ2/dof = 0.58
20
10
0
−10
−20
0
100
200
300
ϕ (deg)



H2 Shutter Closed Contribution to Asymmetry
A = A0cos2ϕ + A1
A0 = −4.76 ± 0.94 ppm
A1 = 0.06 ± 0.63 ppm
χ2/dof = 0.95
fit to constant
A1 = −0.40 ± 1.33 ppm
χ2/dof = 4.33
20
10
0
−10
−20
0
100
200
300
ϕ (deg)