Anyway, on to my comments. I thought that Dan's analysis of the systematic effects was both valuable and interesting, though I had already reached the conclusion by the end of the actual beam runs that the systematic effects were tiny at best. I think he could take things one step further by doing a few Chi-squared tests. For example what is the Chi-squared probability that the four data points at different values of the 0L02 phase are the same value (high, I suspect)? Similarly, one could simply make a model for the various beam positions on the target foil (by the way, I thought we used 1 (or 2?) mm position changes, instead of one "spot diameter", but this is easily checked) such that the asymmetry is the central value plus a linear (or more likely quadratic) term for left-right displacements and a different one for up-down displacements, and then fit the data. I wouldn't spend a lot of time on this, but I believe it would show that there is no meaningful dependence of the asymmetry on beam position on the target foil for any realistic beam position motion during the run. Recall that we ran for hours, and NEVER re-steered the beam from run to run because nothing had changed on the scale of a small fraction of the spot size, verified by (occasional) observation. Properly set up, CW SRF accelerators are well known for their high quality stability. I guess all this is to say that I think a very small amount of additional work would pretty cleanly establish that the systematic effects he looked at are tiny compared to our experimental error bars (which are small), and thus that we don't really need to spend much time on systematic studies in the October run (once we reproduce the January results with convincing precision, though not on all foils).

Is seven shifts all we can hope for in October (says he who may not be able to attend if its the latter part of the month)? Or is it just what you think will solve all remaining issues?? I personally would like to see a couple different energies run, for several reasons. Beam energy meaningfully influences the nuclear size effect on the analyzing power, and may also affect the size of the radiative corrections - our greatest uncertainty in my opinion. While one might not choose to run as detailed a set of measurements at two other energies (meaningfully higher and lower), it would be nice to be able to compare the measured asymmetry with what we would expect hopefully again demonstrating that the nuclear size effect and the radiative corrections (?) can't be vastly larger than what we presently believe. Perhaps we should ask Xavier what he things the energy dependence of the radiative corrections might be??

I also believe there is some merit in rotating the plane of polarization by 90 degrees, and demonstrating that the up/down and left/right asymmetries are basically statistically equal with pretty good precision (and by inference we have built a good instrument). Eliminating the dump dipole has great intellectual appeal to me, and a convincing demonstration would, I believe, add to our result (and to future use of the polarimeter for helping the halls).

I have done a fair bit of playing with the information that Dan has sent regarding his many fits, and have reached the conclusion that the only factor that influences our measured asymmetry is the energy cut. I think that studies to further illuminate or understand this are the most important item we could address in October. But, what exactly would I recommend I cannot exactly say right now. I do believe that, were funds available like in the old days, the one possible improvement to the polarimeter would be to go to a considerably better scintillator (i.e. considerably improved width of the elastic peak). I don't know if one might hope to scrounge such scintillators from long gone experimental equipment or not - there is certainly a lot of stuff out there. I was interested in Marcy's fit to the asymmetry versus thickness, where she gets a lambda of 0.316. All of Dan's many results are consistent with lambda being 0.324. I assume that Marcy's fit is different from Dan's but this might be worth a few minutes of someone's looking.

I liked you analysis along the lines of estimating the analyzing power of the second scattering. I had had some thoughts along these lines, hoping to do analytical estimates, but your presentation this morning was quite nice. To me, the difference between the Lebow and FESEM thicknesses and their effect on the second scattering analyzing power was very interesting - they can't both be right, and I thought the FESEM results looked more believable, though that's just a qualitative impression from this morning. Clearly more work is needed here, and this might be part of the October work.

Anyway, if we are to keep the same foil ladder, then the above encompasses pretty much what I think we should focus on in October. I personally feel that there is considerable merit in exploring the effect of changing Z, but if I am the only advocate................... I certainly DO NOT advocate changing the foil ladder before October. And, personally, I have zero interest in making 499 MHz measurements. I would personally much rather invest in making it "quick and easy" to switch to and from 62 or 31 MHz for measurements for the halls.