Small Angle GDH
December 18, 2003

Tentative Dates of the next Meeting: March 19-20, 2003

Kees de Jager gave the welcome and general remarks:

Both Septa are superconducting. The Right Septum retained it's memory and reached 350 Amps on the first try. The Left Septum has reached 215 Amps after a few training sessions. Energy spread measurements are on going to check compatibility with G0 beam. Without G0 beam, MCC was able to reach 3 x 10^{-5}.

For the future Pol. ^{3}He experiments, there is a possibility to run the Transversity experiment right after G_E^n. Kees was asked for possible space to setup and test the G_E^n magnet. Kees said that the requirements needed should be defined, and the request should be made to Bodo for the necessary space. On a related topic, the Laser Building is still waiting for final approval, which should be made in mid-January.

Alexandre Deur gave a status update of the analysis:

The analysis has so far focused on the 2^{nd} run period. Analysis is currently on going in the following areas: detectors, optics database, bleedthrough, Left arm, target polarimetry, background study, elastic tail study, run summaries and lists. Also presentations for the experiment have been given at the DNP and Hall A Collaboration Meetings. Alexandre has also written reports for the Hall A and JLab status reports.

The only concern is that the analysis is progressing slower than expected.

Jing Yuan discussed the PID detector calibration:

The hardware efficiencies of the Cerenkov and lead glass detectors are above 99.8% and 99% respectively for all energies. Next Jing will work on the cut and pion rejection efficiencies.

Vincent Sulkosky discussed the optics database for the 2^{nd} run period:

There were a few issues with the target reconstruction. For y_{tg}, the upstream and downstream foil peaks became tall and narrow compared to the central foil, and the angular reconstruction was not improving with further iterations. The angular reconstruction was improved by splitting the angular optimization for θ_{tg} and ϕ_{tg}, optimizing the detector offsets, and ignoring the x_{tg} calculation in ESPACE and replacing it with a calculation in the optimization code. The results looked better, however some of the holes from the central row were still off, and a comparison of the optimization at other energies showed that ϕ_{tg} was shifted. This is probably due to the Septum magnet saturating and can be corrected with an offset.

For y_{tg}, further iterations appear to cause the narrowing of the outer two foils. This is probably due to too many unknowns and not enough constraints. The solution is to eliminate

spurious matrix elements and avoid higher order y_{tg} elements greater than 3rd order. The plan is to finish the 6° optimization and move on to the 9° optimization.

Vincent Sulkosky gave an update on the Duality water analysis:

For Duality, there were 6 post-experiment water calibrations. The first three had a significant background problem due to the target lifting motor. For the last three, the lifting motor was unplugged, and for the last two, an A-ϕ box was used to help cancel the background signal. However the A-ϕ box caused significant distortion of the water signal wings.

For the analysis, Vince compared the signals by using a linear and quadratic background. The three calibrations without the lifting motor are understood, but more work is required to understand the errors and background contribution. The up sweeps from the first three calibrations are probably lost because the lifting motor had the most impact near the up sweep resonance peak. The plan is to finish the background analysis and see if the signals with the lifting motor can be used. Once the water analysis is completed, Vince will work on the Pol. ^{3}He signal analysis.

Alexandre Deur discussed his study of the background analysis:

During the experiment, the 2-step contamination was measured by taking empty reference cell data. A first order correction is available using these data. Alexandre improved his early simulation by using Be and Al form factors instead of Nitrogen and computing an absolute quantity. The simulation gives the right order of magnitude. The discrepancy seen at higher energy transfer could be due to using Al form factors instead of glass, multiple scattering was not taken into account, a crude solid angle calculation, and flat HRS acceptance. The simulation is probably good enough for a 2nd order correction.

Alexandre also discussed the effect of electrons punching through part of the collimators. These events could create additional radiative tails. Alexandre made an approximate simulation to see the size of the effects. From the simulation, the finite cell size and finite acceptance have a large effect. However the punch through effect is reduced by multiple scattering. Since the effects are large, they need to be taken into account. The simulation is good enough for the standard cells, but a more complete simulation is needed for the ice cone cells.

Tim Holmstrom discussed the beamline:

Tim discussed the analysis of the bleedthrough of Hall C beam into Hall A. The primary influence was the slit position, and the Hall A and C currents. Tim saw a 0.5% reduction in the bleedthrough after the August maintenance period. An absolute measure of the bleedthrough is needed at the 0.9% level. There was some concern that the pre-maintenance data does not go to zero as the Hall C current goes to zero. Tim will look closer, however it should be noted that there is only one bleedthrough measurement during this period with low Hall C current.

Tim discussed the current calibration. The calibration is linear above 5μA, but OLO2 is very non linear below 5μA with an error up to 10% at 1μA. Tim is looking at the Left arm data to get another estimate of the current. Initial scaler analysis shows the BCMs are linear with the Left arm trigger rate, even at 1μA. Tim will look at the Compton systematic errors, energy measurement, beam position, and finish the Left arm study.

g_2^n and Cross Section Analysis
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Kevin Kramer discussed the g_2^n analysis issues and results:

During the last meeting, Kevin showed results from Wolfgang Korsch's value for g_2^p that caused a significant shift in g_2^n. After discussion, Wolfgang's calculation gave the best values, and the error bars are reasonable. However the shift in the g_2^n results was more dramatic because Kevin left out the unpolarized QE radiative corrections.

Kevin also completely reanalyzed the elastic data for the unpolarized cross section. He found that small adjustments in spectrometer momentum and central angle helped, and now he finds good agreement with simulation in both arms. The collaboration was concerned that he shifted the angle, and they suggested he should check the effect if the shift is not done. It was also pointed out that the invariant mass spectrum does not agree well with the simulation. It was suggested that if Kevin has time, he should analyze the ^{12}C data and normalize the data with the world data. Since it was clear that Kevin does not have time to do this analysis. Todd Averett suggested that he should look at a wider range in target variables, and show a comparison of the cross sections with and without the shifts in ϕ. Kevin said that any geometrical cut causes the Left arm results to be pulled down significantly. This might be a software problem. PID cuts have no effect. Kevin will send ntuples to Nilanga from the 0.79 GeV^2 data.

Xiaochao Zheng discussed the A_1^n Cross Section Analysis:

For the elastic analysis on the Left arm, Xiaochao found that the acceptance variables were in good agreement with data, but she needed to increase the glass wall thickness to 1.7 mm. For the Right arm, she found that the material thickness was okay, but there was an acceptance mismatch at large $\phi_{\rm tg}$. A comparison of the cross sections for both spectrometers agreed with the simulation within error bars. For the DIS cross section analysis, the data was about 8-9% higher than the simulation. The conclusion was there was no obvious problem with data.

Alexandre Deur gave a summary of his Elastic analyses:

Alexandre gave a summary of his GDH elastic analysis and his on-line analysis for the past four polarized ^{3}He experiments. For GDH, the asymmetries had good agreement with both arms. For the cross section, he used Xiaodong Jiang's acceptance, but he found agreement with the Right arm was worse. There was also a shift in $y_{\rm tg}$ that was never explained. There was one instance where the glass thickness had to increase by 10%, but this was later confirmed by measurements.

Alexandre then gave a review of his on-line elastic analysis for E99117, E97103, E01012, and, E97110. In general the asymmetries agreed well, and the cross sections to about the 10-15% level for A_1^n and g_2^n.

Target Lab Status and Upcoming Experiments Discussion

December 18, 2003

Ameya Kolarkar gave an update of the Target Lab status:

Water NMR have been done with the longitudinal correction coils on and off and the lifting motor on and off. Ameya discussed the laser optics setup and alignment. Currently three lines have been setup. The oven is also ready, except for the RTDs. The RF amplifier from Nilanga has been tested and the results look promising. The plan for the upcoming few weeks is to get the polarized system ready to perform a spin-up and spin-down hopefully before the Holiday season.

Kathy McCormick discussed the preparations for G_E^n:

G_E^n is tentatively scheduled for Summer 2005. There is much work to be done for the target design and machining. A work area is needed for G_E^n target testing, since the new Laser building will not be finished until late 2004, and there is no room in the Target lab unless the current chamber is removed. A lot of effort will also go into making and testing hybrid and different geometry cells. There has been significant progress in frequency swept NMR and the polarization direction measurement. Work has also been done to reduce the beamline material.

Xiaodong Jiang talked about the Transversity experiment:

Kees de Jager has said that Transversity could run after G_E^n. A beam time request has been submitted. A different target will be used compared to the G_E^n target. The existing target system will be used, but new vertical coils need to be added. The experiment could also use students to begin working along side the G_E^n students. The target polarization will also need to be flipped every few minutes. The experiment might be followed up with π^+ on the neutron at the same kinematics to do a flavor decomposition.

Spin Duality Meeting

December 18, 2003

Patricia Solvignon discussed the Duality analysis:

Patricia gave an overview of the analysis done so far. She improved the cut on the Cerenkov by taking into account δ-electrons. The efficiencies are high for electron cut and pion rejection. The charge asymmetry is typical < 100 ppm with a few high runs that are mostly short runs. Patricia also looked at the livetime by comparing scalers from the Ring

Buffer and scaler-history file. The results are mostly the same. There are a few runs with problems, but she removed them for now.

For the physics asymmetries, resonances are seen for the 3 GeV and 4 GeV data. The asymmetries for 5 GeV are still small. Patricia looked at the pion asymmetries by using an anti-PID cut. The asymmetries are about 10% at 3 GeV. Patricia's plan is to work on the elastic and target analysis, regenerate the asymmetries, and extract the cross sections.

GDH and Papers Discussion
December 19, 2003

Karl Slifer gave an update of the GDH ^{3}He analysis:

Karl showed his results for g_1 and g_2 interpolated at constant Q^2. He also showed Γ_1 and Γ_2 on ^{3}He. For Γ_2, the error does not include the error from the DIS or elastic contribution. Γ_2 agrees well with zero. DIS makes the biggest contribution to the results, but one question is how well does $g_2^{WW} = g_2$.

Karl also showed results for the GDH integrand for σ^{TT} normalized to $1/nu$ at constant energy and Q^2. At low beam energy, the delta does not contribute significantly to the sum. Finally the GDH integral on ^{3}He was shown. The DIS has only a small contribution. There is a hint of a possible turn over, however the lowest point has a huge systematic uncertainty. The choice of Q^2 for ^{3}He could be adjusted, especially at the lowest Q^2 point. Another question is the confidence on the uncertainty. There is a large error due to the elastic tail subtraction. Karl's plan is to start writing the paper on the ^{3}He GDH integral, Γ_1, and Γ_2.

Zein-Eddine Meziani discussed the work on the 3rd PRL:

The goal Zein-Eddine presented for the 3rd paper was to go beyond the moments paper and discuss the underlying physics linked to higher twists. Only three Q^2 points are used and none below 0.5 GeV^2 for the extraction. There is large uncertainty associated with the value of α_s used to perform the evolution down to low Q^2. Zein-Eddine showed a plot of the higher-twist contribution for $\Delta\Sigma = 0.35$, which is obtained by fitting neutron data. The data was fit using a 2-parameter fit. It should be noted that the statistical uncertainty and the uncertainty in α_s are highly correlated.

It was suggested that Figure 1 should be changed to a log scale. Comments should be sent to either Zein-Eddine or Wally Melnitchouk. There was also discussion on whether the paper should be submitted to PRL or PRB. The final size of the paper will be taken into account to determine to which publication it will sent.

JP discussed the 4th PRL on Spin Polarizabilities:

It was suggested that the line type on the figures should be adjusted, since it is hard to distinguish the difference on black and white paper. Also Figure 3 could be cut at either 1.5 or 2 GeV^2, and Figure 1 and 2 could be combined into one figure with two boxes. The collaboration discussed the strategy in presenting the paper. The publication will only have E94010 results, since there is no other neutron data published at this time. More χPT calculations should be included. Zein-Eddine pointed out that the results show that both χPT calculation approaches have weaknesses and more theoretical work is needed. It's important to note that it is expected that χPT is good, but the results do not agree with the predictions. It was also pointed out that the collaboration may need to address the issue of serial publication of PRLs, if the issue arises.

Xiaochao Zheng discussed the status of A_1^n publications:

The A_1^n PRL has been accepted, and a summary has appeared in Physics News Update. Xiaochao needs more feedback on the PRC. Once she gets comments, she will distribute a new version for discussion at the next meeting.

Upcoming Conferences were discussed by the Collaboration:

Let JP know if anyone is interested in attending any of the upcoming conferences. He recommended that collaborators should register for GDH2004.