RESPONSE TO COMMITTEE COMMENTS USING ORIGINAL TEMPLATE
AUGUST 11, 2010

REVIEW OF THE CLAS-ANALYSIS 2009-104

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1. General comments

The analysis note 2007-117 dated on December 16, 2009 is significantly improved in comparison with the note dated on November 11, 2007.

(1) The Introduction was rewritten with the explanation of the basic physics, mass estimates, production cross sections and branching ratios. It gives the reader the relatively nice description of the main goals of the experiment.
(2) Data selection procedures and particle ID are described in more details. Especially I like how the authors made particle ID based on the selection of $\Lambda(1115)$ ground state.The improvement of the plot of β versus momentum of negative particles is very impressive (Fig. 5).
(3) The extensive eg3 trigger inefficiency study was one of the major effort of the group. As a result the upper limit was changed from 200 pb to 550 pb in comparison with the first reported upper limit at 2007.
    - Note that the main change in the upper limit was not due to the trigger acceptance, but rather the change in acceptance for the physics model, which the committee rightly noted had some inconsistencies (Table 6, rows 1 and 4). These were corrected and resulted in a factor of 2 change in acceptance. The trigger efficiency increased the upper limit by an additional 30%.
(4) The check of the absolute normalization of the cross section upper limit was done using the reaction $\gamma d \to \Delta^{++}\pi^- n$. The good agreement between eg3 cross section and g11-SAPHIR cross sections gives the confidence that there are no big uncertainties in the reported upper limit.
(5) The use of different methods for the upper limit determination makes sure that the upper limit was evaluated correctly.
(6) Extensive checks of the possible systematics uncertainties was performed. The final result is stable under the variations of the major experimental cuts. The different MC production models gave the possibility to evaluate the uncertainty in the determination of the upper limit. It is the largest systematic error in the

analysis and it is as large as 20%. It is very nice result taking into account the high luminosity of the experiment and the difficulty with the trigger inefficiency.

2. Specific comments

(1) **Abstract**: I did not find the $\Lambda\pi^-\pi^-$ mass spectrum in the note. All figures include the cut on the invariant mass $\Lambda\pi^-$ around $\Xi(1321)$ mass. Please correct me if I am wrong. However in the abstract it is explicitly written that the invariant masses of the $\Lambda\pi^-\pi^-$ and $\Xi\pi^-$ were used in the analysis to set limits on the strength of narrow resonant structure near the mass $M = 1.86$ GeV.
- Removed $\Lambda\pi^-\pi^-$ from the statement.

(2) **Abstract**: It is not clear what does it mean : to set limits on the strength of the narrow resonance?
- We meant cross section times branching ratio. Text has been modified.

(3) **Introduction**: All particles in the anti-decuplet have spin=1/2 (including Ξ^{--}) , not 3/2 as it is pointed out in the Introduction. It doesn't necessary mean of course that pentaquarks have J=1/2, but anti-decuplet particles have J=1/2.
- This is a typo, should be I=3/2, which has been corrected and clarified.

(4) **Identification of** $\Lambda(1116)$, page 9: How did you define the timing cut boundaries in Fig. 4 (black curves)?
- Timing cuts were defined by eye and variation about cuts checked for sensitivity. A missmatch in the tightness of these cuts in the data and the simulations would introduce an uncertainty. In order to test the sensitivity of the results to these cuts we added a 200 ps time shift in the time difference between the pion and proton from the Λ-decays in the simulations, effectively shifting the distributions with respect to the cut curves in Fig. 15 in the note. We compared the resulting acceptance and the upper limits. Fig. 1 in this document shows the acceptance without the 200 ps shift (blue markers) and with the 200 ps shift (red markers). Fig. 2 in this document shows the comparison between the upper limits calculated using the two acceptance histograms. These plots demonstrate that the results do not have high sensitivity to these timing cuts.

(5) **Identification of** $\Lambda(1116)$, page 9: To skim files you consider all the possible pairing of negative and positive tracks to select Λ events and then you repeat the cut afterwards, using energy loss correct momenta. Are events passing the second cut a subset of the skimmed events? In other words is it possible that events that would pass the cut after the eloss correction do not pass the the cut when producing the skim files?
- Skimmed files with $\Lambda(1116)$s include events passing EITHER of the two cuts (with or without energy loss corrections) to ensure efficiency.

(6) **Identification of negative pions**, page 11: It is very smart to use the π^- vertex time to select the RF bunch due to the large number of hits in tagger. The fig. 5 is very impressive. How did you define the boundaries for π^- selection in Fig. 5.? How did you define the boundaries for π^- selection in Fig. 15 (page 26)?

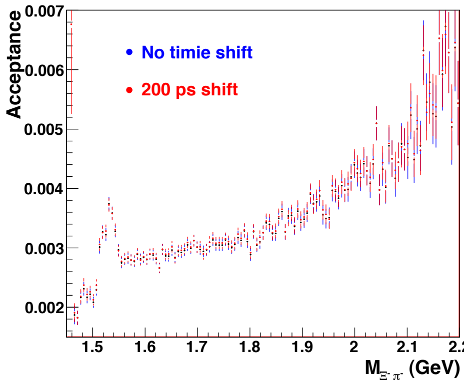

FIGURE 1. Comparison of the acceptances calculated with (red) and without (blue) 200 ps time offset in time difference between π^- and the proton.

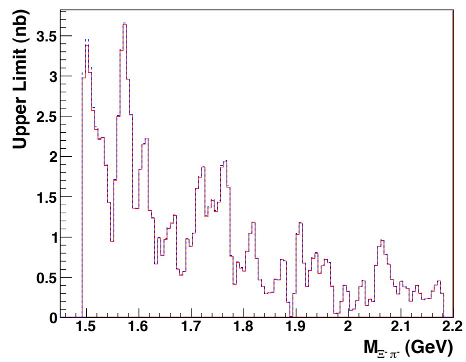

FIGURE 2. Comparison of the upper limits calculated with (red) and without (blue) 200 ps time offset in time difference between π^- and the proton.

- Selection cuts were determined by eye and insensitivity was later checked by modifying cuts. Using wider cuts did not gain visible number of events containing Ξ^-.

(7) **Identification of negative pions**: You used the pions to define the start time of the event and then you used the time defined by pions to define pions. It could be circular argument or at least a bias cut. It will be nice to see the β vs p for kaons and the vtime-stvtime (with and w/o the cut) to compare this cut with the standards time based cuts.
- The first pion is identified by matching it with the proton to obtain the Lambda. This pion is subsequently used for start time of other pions in the event, and it is not included in the π^- identification plot in Fig. 5 of the analysis note.

(8) **Energy Loss Correction** page 12: You say "..particles MAY lose part of the energy..". Particles always lose the energy due to the ionization. The Landau distribution has the minimum energy value and never equals zero.
- Text changed from "may" to "will".

(9) **Energy Loss Correction** Fig. 6: Do you have scatter plot of the $p\pi^-$ invariant mass as a function of proton momentum and of pion momentum?
- The two requested scattered plots are presented on Fig. 3 of these document. The left panel shows the $p\pi^-$ invariant mass versus proton momentum, the right panel shows the $p\pi^-$ invariant mass versus momentum of the π^-. The

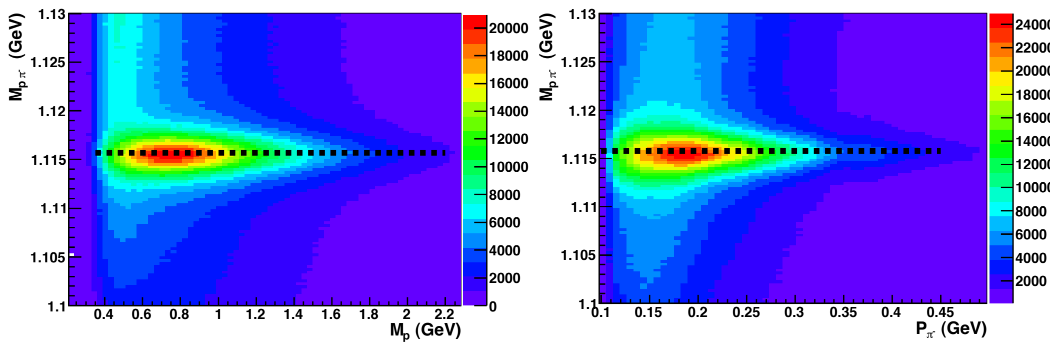

FIGURE 3. Invariant mass of the $p\pi^-$ system versus proton (left) and pion (right) momentum. The black dashed line shows the value of the $\Lambda(1116)$ mass.

DOCA and the timing cuts described in the analysis note are already applied to the Λ-candidates shown here.

(10) **Momentum corrections** page 13: This question is from our previous list of questions. Could you please provide the supporting pictures and formulas for the momentum corrections? Especially interesting how did you study the drift chambers misalignment? I believe that you did everything correctly, however it will be nice to have it in the analysis note.
- The procedure is described in Paul Mattione's master's thesis. Ref [30] Kinematic fitting of detached vertices. JLAB-PHY-07-643, May 2007.

(11) **Selection of $\Xi(1321)$ events** Fig. 7. Does $c\tau$ value for Λ consistent with the table value?
- We included a plot of $c\tau$ in this document with two fits to a function which is a convolution of Gaussian and exponential (see Fig. 4 of this document). In the left panel the fit is performed in the full range of $c\tau$, while on the right panel the fit is in the tail, where the non-Λ background is less prominent. In the left plot the value of $c\tau$ is 7.6 cm, while in the right plot $c\tau$ is 8.2 cm. The PDG value for $c\tau$ for $\Lambda(1116)$ is 7.89 cm. Note that such a fit assumes a constant resolution for $c\tau$, which is strictly is not correct. Even if the vertex resolution was constant, the $c\tau$ resolution would change because of different momentum of Λs in different events.

(12) **Selection of $\Xi(1321)$ events** Fig. 8. Does $c\tau$ value for Ξ consistent with the table value?
- The signal/background is < 1, so there is substantial contamination from accidental background and no reason to assume that the measured number should correspond to the $\Xi(1321)$ lifetime. We included a figure of $c\tau$ for $\Xi(1320)$ in this document with two fits to a function which is a convolution of Gaussian and exponential (see Fig. 5 of this document). In the left panel the

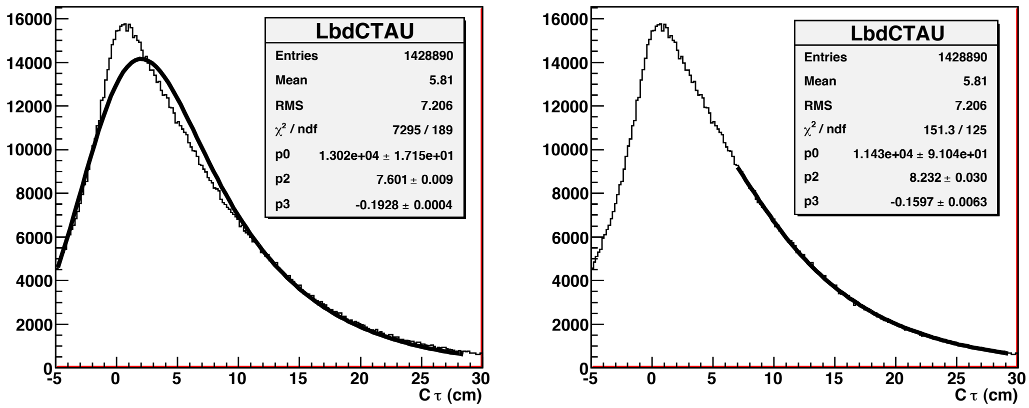

FIGURE 4. Fits to $c\tau$ distributions for $\Lambda(1116)$ in the full range (right) and in the tail (left). The fit parameter for mean $c\tau$ in cm is $p2$.

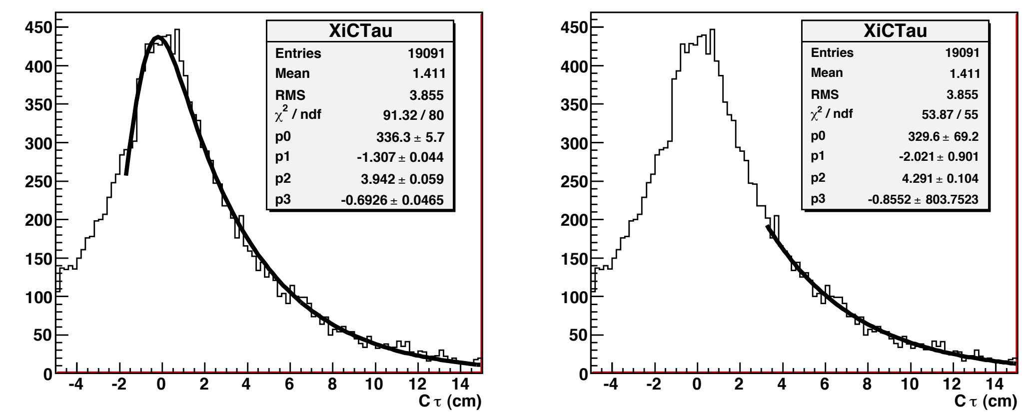

FIGURE 5. Fits to $c\tau$ distributions for $\Xi(3120)$ in the full range (right) and in the tail (left). The fit parameter for mean $c\tau$ in cm is $p2$.

fit is performed in the full range of $c\tau$, while on the right panel the fit is in the tail, where the non-Λ background is less prominent. In the left plot the value of $c\tau$ is 3.9 cm, while in the right plot $c\tau$ is 4.3 cm. The PDG value for $c\tau$ for $\Lambda(1116)$ is 4.91 cm. Note that such a fit assumes a constant resolution for $c\tau$, which is strictly is not correct. Even if the vertex resolution was constant, the $c\tau$ resolution would change because of different momentum of Λs in different events.

(13) **Selection of** $\Xi(1321)$ **events** Did you take into account that Λ and Ξ decays may be out of the start counters coverage? You need to check that the events satisfy your trigger conditions in your MC analysis.
- Follwing this comment we performed a study of the possibility of the decays of Λ and Ξ decays affecting the triggering efficiency for this reaction. We used Monte-Carlo events and GSIM to study the difference between acceptances when requiring a hit in the Start Counter TDC matched by the sector number with a track and a reconstructed TOF hit. We found that there is an overall inefficiency on the order of $\sim$ 15% due to the decays of Λ and Ξ outside of the Start Counter. This additional correction was used in the extraction of the upper limit of the cross sections. We also assigned an additional uncertainty to the acceptance calculations of 5%, which was also used in the upper limit calculations for the cross sections. The text of the analysis note was updated.

(14) **Selection of** $\Xi(1321)$ **events** Fig.9. Could you show the same distributions without $c\tau$ cut?
- See Fig. 10 of the new version of the analysis note (Fig 12 in previous version), which shows the distribution for four different $c\tau$ selections. Note that no $c\tau$ selections were used in the final analysis to extract limits. The $c\tau > -25$ cm is almost equivalent to not having any $c\tau$ cut.

(15) **Selection of** $\Xi(1321)$ **events** page 17: Table 2 is not referred anywhere in the text - please refer to it when mentioning the cuts (e.g when you mention you improve the Λ selection with a cut on $c\tau$ at page 15)
- Added a reference to Table 2 on page 16.

(16) **Conservative selection of events** page 17: how do you know the background under the $\Xi^-(1321)$ is distorted when selecting the best $\Lambda\pi^-$ pair? Do you know it from simulations?
- We can see this in the mass distribution of the $\Lambda\pi^-\pi^-$ system when we look at the sidebands(see Fig. 6 in this document). We think it is due to the uncorrelated $\Lambda\pi\pi$ events combined with the cut on the cascade mass. There is very strong distortion near $M_{\Lambda\pi\pi} \sim 1.53$ GeV, which is difficult to take into account if the sideband subtraction method is to be applied. If all remaining π^-s are considered for reconstructing the Ξ^-, then we do not see such a behavior in the sideband spectrum (see Fig. 7) of this document. Although this happens far away from the mass range of the main interest, we decided that it would be better not to use “best pion“ selection.

(17) **Conservative selection of events** page 17: Figure 12 is out of order.
- Moved Fig. 12 back to Fig. 10 in the analysis note.

(18) **Fiducial and energy cuts** page 20: Timing window 0.8 ns could be too narrow for the event's selection. This cut has to be investigated in details to make sure that there is no hidden loss. I remember from the g11 analysis that you need to apply about 20% correction factor if you want to use such s strong cut. In your reply to our questions you wrote that the software timing window was changed to ± 3.2 ns. However in the analysis note you state that it is 0.8 ns.

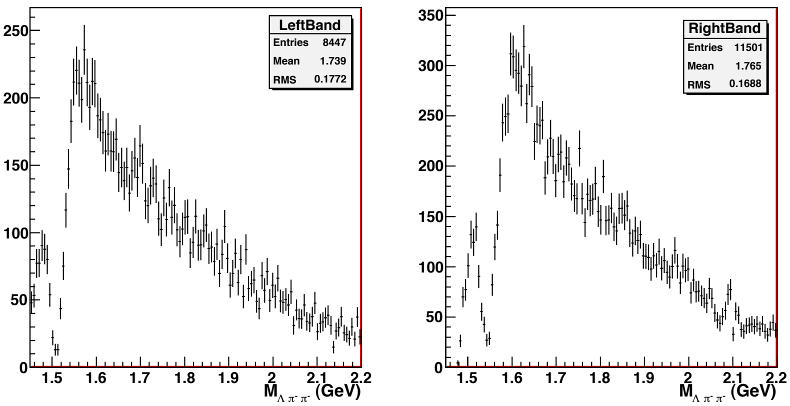

FIGURE 6. Mass of the $\Lambda\pi^-\pi^-$ system when the "best pion" selection is used showing distortions near the mass $M_{\Lambda\pi\pi} \sim 1.53$ GeV. The left panel shows the spectrum in the left sideband of Ξ^-, and the right panel shows the spectrum in the right sideband.

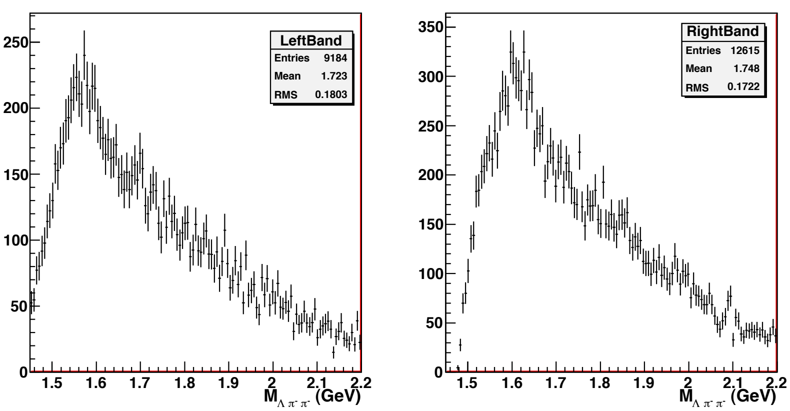

FIGURE 7. Mass of the $\Lambda\pi^-\pi^-$ system when all remaining pions are used showing no distortions near the mass $M_{\Lambda\pi\pi} \sim 1.53$ GeV. The left panel shows the spectrum in the left sideband of Ξ^-, and the right panel shows the spectrum in the right sideband.

- The cut is ± 3.2 ns. The text has been corrected.

(19) **Simulations** Fig. 13. The comparison between simulated and real data are very useful from my point of view. However it may be instructive to present the one-dimensional projections as well to stress the difference between phase space generator and real data. As I see the MC momenta are harder than in the data. it is not clear as well what π^- is it? We have 3 π^-s in the reaction.
   - The distortions are primarily due to the low-energy proton. (See Fig. 4 in the analysis note). Note that the differences between GSIM and data are largely due to the combinatorial background present in the data, but are not simulated in the GSIM. No good model for the background is available, so only qualitative comparisons can be made with the GSIM.

(20) **Simulations** Fig. 15: the simulation seems very different from the data in Figure 4. You mention you shift and change the cuts to adjust for small differences, but in this case the profile is completely different and the cut on the simulation does not select the events as nicely as in the case of the data.
   - Repeating our response to the previous question: The distortions in the data are likely due the relatively large combinatorial backgrounds in the data, and not present in the GSIM. Therefore, quantitative comparisons are difficult. However, we have tried to mach the distributions empirically as best we could. The width of the distributions are similar, therefore applying similar cut windows should provide us with a reasonable estimate of the acceptance correction. The uncertainty of the final result due to reasonably different cuts was negligible compared with the main sources of systematic errors.

(21) **Model dependence** Fig. 17: What is the bump in the region of 1.55 GeV?
   - The structure is due to combinatorial $\Lambda\pi^-\pi^-$ background. There is a substantial chance of assigning the incorrect π^- to the Ξ^-. Because there is a cut on Ξ^- mass there will always be a chance that the π^- will be reconstructed with a different enough momentum to miss the cut. But the presence of another π^- along with the Ξ^- may artificially increase the chance of an event passing through the Ξ^- mass cut by an accident. That can happen if, for example, the momentum of the second π^- is close to the momentum of the true π^- from Ξ^-. In that case the invariant mass of the $\Lambda\pi^-\pi^-$ system will be ~ 1.52 GeV, which is approximately where the bump is located. Note that when the Ξ^- mass cut is removed, the bump disappears.

(22) **Model dependence** Fig. 17: This figure is dramatically differed from the Fig. 11 of the old analysis note dated November 11, 2007, but the figure's caption is the same. What's happening?
   - The new plot has an event selection cut on the $\Xi^-\pi^-$ mass, selecting events close to those simulated within ± 10 MeV to match the mass cut when selecting the ± 10 MeV mass window when estimating the upper limit for the cross section. The absence of this cut in this model in the first round lead to the inconsistencies in the acceptances for various models in Table 6, lines 1 and 4. This was pointed out by the committee and was corrected in the new

version, leading to lower acceptance for this model and much smaller rise of the acceptance with the $\Xi^-\pi^-$ invariant mass.

(23) **Model dependence** What model did you use for your final upper limit?
- Table 6, row 1. We refer to this model as our "nominal" model. The acceptance value from this model is scaled up by 20% to match the average acceptance value from Table 6 at the mass of $\Phi(1862)$.

(24) **Model dependence** page 29: you assign a systematic error of 20% but you have differences of 47% between #1 and #3. Aren't you underestimating the error?
- We thought that using the two extremes would be overestimating the error, which typically is interpreted as the equivalent of one σ deviation from the expected mean value.

(25) **Normalization** Could you please provide the plots to check the photon flux normalization:
- Yield of your Λ skim as a function of the tagger EID?
- Photon flux as a function of the tagger EID?
- Yield of the Λ skim normalized to the photon flux as a function of the tagger EID?
- Normalized Λ yields and the corresponding photon flux are given in the Normalization analysis note 2009-106 in Fig. 12, Fig. 13.

(26) **Trigger inefficiencies** Fig.20. It is impossible to identify the run number on x-axis. Please mark run number by arrow when the trigger was changed.
- We added green vertical lines in Fig. 20 to identify a few key periods.

(27) **Trigger inefficiencies** Fig 21: Could you please zoom Fig. 21? It is difficult to understand who is who at this picture.
- Enlarged figure for clarity.

(28) **Trigger inefficiencies** What were the trigger rates for trigger bit 5 and trigger bit 6 with and without prescale?
- At 5 nA, R5=970 Hz (no prescale), R6=275 Hz(no prescale), Rtot=950 Hz. Typical running at 30 nA and no ST in asynch: R6=1.7Khz (no prescale), R5 1KHz (prescale = 5+1). Total Rtot 2.4 KHz (R6 no prescale and R5 prescale=8+1). The MOR (T1-19) 13 MHz.

(29) **Trigger inefficiencies** The statement that a trigger inefficiency is associated with the start counter presence in the trigger is not correct. The start counters were always in the trigger. The coincidence of MOR and OR of start counters was added only in the asynchronous input of the trigger supervisor.

The start counters HV was decreased in comparison with the g11 HV setting. It may create the trigger inefficiency if the SC amplitudes became closer to the discriminator threshold. However in this case the trigger 5 will be affected as well.

There is interesting operator log entry 18483:

Running trk_mon on some cooked file, we noticed that sector 1 has a lot more tracks than it's fare share. After examining the trigger file Sergey found out that only the combinations with sector1 dot 3-sector trigger were included for trig bit 6.

That makes (5x4)/(1x2) combinations out of (6x5x4)/(1x2x3) combinations, which is factor of 2 less. After loading the new fixed trigger file from Serguey the bit 6 increasing from by $\tilde{1}.8$.

The log entry has run number 45512. Where is this point in the figure 20?

Pulse Width	Trigger Rate STxMOR (trigger bit 6)	MOR only (trigger bit 5)
20 ns	3.53 kHz	4.10 kHz
15 ns	3.07 kHz	3.73 kHz
10 ns	2.08 kHz	3.27 kHz

TABLE 1. LogBook entry 18499. Run Number 45543. I=30 nA

This is the extraction from the operator logbook 18499.
The run number in this log is 45543.

Six different configurations were tested with current I=30 nA.

The delay curves MOR-Start counters are in Log book 10 ns looks good. We decided to take test run with 10 ns, and stay with 15 ns. with Start counters in ASYNC.

The conclusion from the logbook:

However the ratio With_Start_counter_in_ASYNC/MOR only (2.08/3.27 for 10 ns) was not understood. Start counters enter into L1 trigger any way. There is a possibility that we have dead time in OR of all start counters. From this point of view it is more save to have MOR only and work with width=10 ns. The rate will be 3.27 kHz what is acceptable for DAQ.

My guess is that when we returned back to the configuration with ASYNC the pulse width of the MOR and Start Counter discriminators was set to 10 ns. It is clear from the table that this is not correct. I did not find in the log book when this trigger configuration was implemented (probably in January). It is the main reason for the trigger inefficiency from my point of view.

- We do not have a model that can explain the origin of the trigger inefficiencies. However, we do know that the inefficiency depends on a) beam current, and becomes more pronounced for higher beam currents, and b) depend on particular start counters, noting that the effect is most pronounced when the center start counters in each sector fire. The center counters reach down to lower angles and have the highest rate. The suggestion that the ASYNC pulse width was not set correctly could lead to inefficiencies, but the inefficiencies should not increase with higher beam current, but rather decrease due to pileup effects. Phenomenologically, the inefficiency could be caused by a failure of the OR of

ST counters input to the MOR to fire at high rates (even though individual ST inputs to the L1 fire correctly). But we know of no common hardware failures of this type. But whatever the reason, we were not able to pinpoint the exact reason for the inefficiency. Instead our approach is to find a phenomenological parametrization of these inefficiencies to take them into account, as discussed in the analysis note.

(30) **Invariant mass spectrum** Fig 23: figures 23a and 23b are swapped.
  - Figure caption has been corrected.

(31) **Invariant mass spectrum** Fig 24: could you please indicate the bin size: What is your mass resolution?
  - The bin size in this plot is 6 MeV. Text clarified.

(32) **Invariant mass spectrum** Fig 26: could you please indicate the bin size?
  - The bin size for red points is 25 MeV, for the black points is 4.5 MeV. Text clarified

(33) **Upper limits** page 44, line 3 from the bottom of the page: What is ξ^2-distribution?
  - It is a χ^2 distribution. Typo corrected.

(34) **Conclusion** page 49: you present theoretical estimations of the Ξ^{--} cross section in the Introduction. The theoretical value for the photoproduction off the neutron is in the range 0.4–1.5 nb. The conclusion is the right place to compare the experimental upper limit and theoretical expectation for the cross section and probably discuss this comparison.
  - Text modified

3. Grammar/typos comments

(1) Page 16: "The cascade ground *state* $\Xi(1321)$ ..."
(2) Page 18: next to last line: addition*al*
(3) Page 27: remove the second "and are in given in Table 5" in line 4
(4) Page 29, end of the first paragraph: configuration*s* (plural)
(5) Page 29, beginning second paragraph: the third column *of table 6*
(6) Page 29 second paragraph 5th line: raw → row
(7) Page 31: wrong units for the density (g/cm^3)
(8) Page 33 last line Christmas (one "s")
(9) Page 34 first line Christmas (one "s")
(10) Page 34 third line from bottom "below" (one "l")
(11) Page 37 third line "statistical" (one "l") and "*of* the order
(12) Page 43: missing reference after Feldman and Cousin

- All typos fixed. Thanks