HIN-19-014 preapproval

General Comments

Nice and interesting result!

Paper Comments (v0)

- abstract: significantly broadened
  - Done
- Results stringent constraints -> verb missing
  - Added “depoist”
- l81: explain why these HF thresholds
  - Added one sentence “which are determined from empty bunch crossing events” after thresholds values
- l82: "nothing else in the central tracker region" -> be explicit. No add'l track with |eta|<2.5
  - Changed to “no additional track over the range $|\eta|<$ 2.5”
- l89: the fact that YnZn means Y neutrons on 1 side and Z on the other side could be clearer
  - Here, we follow the conventions in UPC measurements, but we are open to modify the text if you insist
- l95: what is this SS background? Also, any background from pomeron exchange (CEP)?
  - Added “combinatorial” before background. The same-sign background covers combinatorial background from hadronic collisions, which produce both correct-sign and wrong-sign pairs. In this analysis, we only have 2 same-sign pairs compared to 87066 correct-sign pairs. Meanwhile we have also considered systematic source from hadronic collisions
  - There are three main physical background producing correct-sign pairs in UPC events: 1) photon-photon produced neutron mesons (e.g. pi0, eta, f0, f2, eta_c etc) followed by decaying into charged meson pairs. The mass of those mesons is far below the lower mass limit (8 GeV) applied in this analysis; 2) two-photon produced charged meson pairs (e.g. pi+pi-, k+k-, D+D-). Considering the cross-section of direct pi+pi- pair (largest compared to other sources, Prog. Part. Nucl. Phys. 39 (1997) 503) and the muon misidentification probability, the contamination of this source can be negligible; 3) vector mesons produced from photon pomeron fusion. Only upsilon sits in the analyzed mass region (8-60) and has been considered in this analysis.
- l101: any evidence for photoproduced Z? What happens if you look at the high mass region?

    - No, there are only 30 entries between 70 and 100 GeV and no resonance peak structure is observed
- l112: you mean each muon pair in data?
    - Yes, the efficiencies are applied for each dimuon pair in data
- l119: uncertainty in EMD xsec?
    - Added the uncertainties
- l126: not clear how the migration is estimated. So you have empty events with some signal in the ZDC. How do you know the true number of neutrons?
    - This part is discussed in detail in AN sec. 5.5. We estimate the migration probability for true neutron multiplicity to **measured neutron multiplicity (photon-photon collision + possible pure dissociative event without any activity in CMS tracker)** in selected zero-bias events and build a migration probability matrix as shown in Eq.5 in AN. This neutron multiplicity migration causes that the alpha (mass) distribution in one neutron multiplicity class is classified into a higher neutron multiplicity class. Instead of estimating the true number of neutrons event by event (not doable), we convolute the measured differential alpha (mass) distributions to the true distribution for each analyzed neutron multiplicity class using the **inverse matrix** shown in Eq.5 in AN.
- Fig. 2: what is going on with the very large chi2 for 0n0n, and very small for XnXn?
    - The choice of fit function is empirical. Small alpha region has very high precision so it is not a surprise that the function gives a high chi2/ndof. Meanwhile, our physics conclusion (nominal values and rising trend of <alpha^LO>) is insensitive to the choice of fit function, which was checked in our early studies [1] (p6). We also directly calculated the <alpha> vs. neutron multiplicity for **data with apha<0.006**, which contains a small fraction of HO contribution. The <alpha> for data with apha<0.006 has the same rising trend but with slightly nominal values compared to that from fit, which gives us confidence of the empirical function.
    - [1]: Comparisons between different LO and HO models
- Eq. 2: why the 1/4 exponent, have different values been tried? Any reference for the use of these functions?
    - The fit function has no theoretical guidance so far and one correction term ($c_3 \times \alpha^{0.25}$) is added to the widely used exponential function [2] for LO photon-photon interaction to account for destructive structure around alpha$\approx$0 in data. Meanwhile, we tested this modified function using the alpha distribution of leading order photon-photon scattering generated by STARlight and this function can fit to the distribution reasonably well. The <alpha> from this function is 1.347e-3 while that from STARlight is 1.349e-3.
    - We tried different exponential values, saying from 0.1 to 0.5. The results (<alpha^LO> and chi2/ndf) are not sensitive to the scanned values. For instance, inclusive (all neutron classes combined) <alpha^LO> = 1.26e-3 +/- 6.9e-6 (stat.) +/- 8.2e-6 (sys.) for 1/4 exponent. The difference of nominal values for the exponents between 0.1 and 0.5 exponents with respect to 1/4 is within 5e-6.
    - [2]: ATLAS, ATLAS-CONF-2016-025 (Figure 1); CMS, FSQ-16-012, AN2016_464_v14 (Figure 41)

- l145: fit procedure is not ideal. Better integrate the function in each bin (if using ROOT, use option "I"), instead of iterating. Also, what kind of fit is used? If systematic uncertainties are included, you can't really use a likelihood fit, so it should be a chi2 fit. However, it would be better to do an unbinned likelihood fit (RooFit), and for systematic uncertainties to repeat the analysis with different variations.
    - Yes, the fit is a chi2 fit. We switched to “I” option according to your good suggestion, and the results are basically the same as that from iterative fit.
    - The unbinned likelihood fit is our original plan. However, it’s technically challenge (even not doable) to use unbinned fit in this analysis because of the following reasons: 1) To correct for the dissociative pileup, the measured differential alpha distributions need to be convoluted into true distribution for each analyzed neutron multiplicity class. During the convolution, negative weights are used for some measured alpha distributions. This situation becomes even worse for deriving the true alpha distribution in higher neutron multiplicity classes. 2) For the neutron multiplicity bin with asymmetry neutron numbers on both sides, e.g. 1nXn, the dissociative pileup correction needs to be performed for 1pXm and Xp1m (p represents plus, m represents minus) separately, then combined into 1nXn. 3) For neutron contamination correction (AN Sec. 6.3), we have similar difficulties as pileup correction. Doing these two corrections at the same time makes unbinned fit even harder or unrealistic.
- l157: why 5GeV?
    - This is a pretty conservative choice. There are 87066 correct-sign pairs but only 2 same-sign pairs for the default HF veto thresholds (keep 99% UPC events), which already tells us the hadronic contribution is negligible. Thus, the physical results should not be sensitive to the further tightened HF veto thresholds. To conservatively estimate the impact of HF veto thresholds, we tighten the HF veto thresholds to remove 20% more events resulting in a 5 GeV value. Meanwhile we are open to hear your suggestions if you are not satisfied with this choice.
- Fig.2 bottom: given that you report <M>, it would be useful to show a similar figure as Fig.1, for M instead of alpha
    - We agree providing mass spectra for analyzed neutron multiplicity bins is helpful. Considering the PRL length requirement, we will prepare a supplementary plot instead of putting them in the paper. Meanwhile, we would like to point out the core physics message is from alpha result while mass is kind of supporting result as the model expects some impact parameter dependence of photon energy.
- have you measured the integrated <alpha^LO> (all neutron classes combined)? is it well described by Starlight?
    - The integrated <alpha^LO> from data is 1.26e-3 +/- 6.9e-6 (stat.) +/- 8.2e-6 (sys.) while STARlight prediction is 1.35e-3.
    - Added the integrated results in AN v4 Sec. 7.1
- l177: 5.4 sigma is poorly phrased. Suggest replacing sigma with standard deviation, or mentioning a p-value for the hypothesis that both are generated with the same true value

    - Changed to 5.4 standard deviation
- l188: show this fit in the paper!
    - As we explained in your previous comment, we will provide mass spectra as a supplementary plot
- l193: 12 sigmas: disfavoured in CMS. replace with "much more than 5 standard deviations" (https://twiki.cern.ch/twiki/bin/view/CMS/LargeSignificances)
    - Changed to “with a significance clearly exceeding 5 standard deviations”

AN Comments (v3)

AN should be standalone. Please copy the main results from the linked talks into the body or appendix of AN. For example line 94, line 121

- We added the HF veto thresholds determination part in the updated AN
- For 3-D single track efficiencies, they have 100 pages. The readability is not good if we put them in the AN. We prefer to put them in a separate link or just remove them.

Main items to work on

- Neutron peak extraction
    - We need more information on the neutron peak fitting. Pull distribution, etc. Also noise contribution in the fits?
        - Added pull distributions and noise contribution discussion in AN v4
        - We have communication with the ZDC group regarding the noise contribution and they provided the following information: ZDC used new readout electronics (QIE 10), the same electronics as HF, in 2018 PbPb data taking. The noise for ZDC is very low due to the large dynamic range of the QIE10. The total sum of charge of noise is around ~500fC, compared to the typical 1n peak around ~7000fC (plus) or 10000fC (minus). The noise contribution to 1n peak is negligible, which has been confirmed using the empty bunch crossing events by ZDC group (/HIEmptyBX/HIRun2018A-27Feb2019-v1/AOD with HLT_HIL1NotBptxOR_v* trigger).
        - We also did an estimation of the noise contribution using our single muon UPC triggered data. The 0n peak (first peak in Figs. 7(b) and 7(c) in AN v4) represents the ZDC noise distributions, which are well separated from 1n peak. We used modified exponential functions to fit the 0n tails for both sides (see in [3]) and found the contamination of noise is <0.5%, which is consistent with ZDC group’s study.
        - [3]: Estimation of noise contribution to 1n peak
    - Why is Gaussian adequate for each peak? How do you know there is no tail for each of the xN peak?
        - Gaussian function is recommended by ZDC group and widely used in our community to disentangle different neutron peaks [4-6]. The peak position and width of xN distribution scale as expected based on the multi-Gaussian fit although the fit does not describe the data perfectly.

Unfortunately, we cannot directly demonstrate there is no tail for each neutron peak.

- We would like to point out that we only use multi-Gaussian fit to estimate the purity of each selected neutron sample and then to estimate the effect caused by neutron contamination, which is treated as systematic uncertainty in this analysis. The contamination effect is <1.1% for <alpha^LO> in 1n1n class which has the largest contamination (6-7%). This phenomenon is not surprising, because the acoplanarity distributions between 1n1n and 1n2n **(not Xn)** is very similar which will largely suppress the contamination effect.
- [4] M. Chiu et al., PRL 89 (2002) 012302
- [5] ALICE, PRL 109 (2012) 252302
- [6] STAR, PRC 96 (2017) 054904

- Perhaps one way to show this is to find data under different beam conditions. This way we get different relative sizes of xN peaks. And if the same function can fit all of them equally well, it's a nice demonstration.
  - We use the same function fit to ZDC distributions from the pure dissociative events (no activity in CMS tracker) [7] selected in zero-bias data. The peak positions are nicely reproduced (<2.3%), however, the widths of xN peaks have sizable discrepancy (~15%) compared to that from photon-photon interaction events. This changes the 1n contamination probability from 6-7% down to 2-3%
  - [7]: Multi-Gaussian fit to ZDC distribution in zero-bias data, Ratio plot, Pull distribution
- In any case, we need some uncertainty about this.
  - We treat the neutron contamination itself as an uncertainty source. We artificially increase the contamination fraction from 6%(7%) to 10%, the corresponding systematic uncertainty to 1n1n increase from 1.1% to 1.9%, which is still not a leading contribution. That tells us our results are not sensitive to the neutron peak fitting. Assigning additional uncertainty on the neutron peaking fitting is not so helpful in this analysis.

- LO vs HO. I see three types of diagrams: (a) LO (b) same final state as LO but HO (c) extra photons in the final state.
  - What are the relative sizes between the three types? Do you have an alpha distribution with these three separate?
    - Regarding relative sizes or contributions, there is no theoretical calculation. STARLight has only LO. Both (b) and (c) appear in the tail of alpha distribution so it is not possible to disentangle them. Our main focus is to extract (a), which has more distinct feature in alpha than the other two
  - Depending on the relative sizes, what do you do to the interference between (a) and (b)?

        - Not sure I understand the interference you are referring to. Do you mean (a) and (b) happen in the same collision? If not, would you please elaborate a little bit more? In this analysis, we only allow one dimuon pair existing in one single collision and treat (a) and (b)+(c) independently during the disentanglement.
    - Is the empirical functional form of the alpha fit theoretically motivated?
        - No. The leading order function form is based on widely used exponential function [2], but with additional correction term ($c_3 \times \alpha^{0.25}$) to catch the destructive structure around alpha ≈ 0 (Fig. 46 in AN v4). For the alpha distribution from leading order photon-photon scattering, exponential function is commonly used [2]. For the high order, we borrow the function form used for describing nucleon-dissociative t (p_T^2) distribution.
        - [2]: ATLAS, ATLAS-CONF-2016-025 (Figure 1); CMS, FSQ-16-012, AN2016_464_v14 (Figure 41)
    - Based on the discussion during the pre-approval, it seems that is purely empirical. In this case we can't claim that what we are measuring is the leading order contribution. Suggest rephrase to something like "core width" or similar. I leave the exact choice to authors and ARC.
        - Thank you for offering this choice.
        - To avoid some potential issues you are concerned about, we also publish the whole acoplanarity spectra for each neutron multiplicity classes which allow people to use advanced theoretically motivated functions to extract physics message.

=== Section 2 ===

- Table 2: is the MC official?
    - No, we are currently using private MC samples. The official MC request has been submitted in 11/2019.

=== Section 3 ===

- l92: offline thresholds much higher than online. Is online cutting into the noise? Any possible bias? Have you analysed ZB with the same offline selection?
    - I made a mistake in the AN v3 (fixed in AN v4). I confirmed with Andre that the "2" in HLT_HIUPC_SingleMuOpen_NotMBHF2AND trigger does not mean 2 GeV. **The NotMBHF2AND means !(HF+ > 15 ADC Counts AND HF- > 19 ADC Counts).** From the N_{trk}^{HP} distributions [8] (black histogram) of single muon UPC triggered data, one can clearly see hadronic contributions. Therefore the online thresholds are much looser than offline ones, and there is no bias.
    - [8] NtrkHP distribution
- l94: this study is important and should go into the AN.
    - Added this part in the AN v4
- l97: do you check that ZDC is operating properly in all later runs and LS? Do you apply a DCS bit or something similar in the JSON file for ZDC?

- ZDC is not included in the run registry, so it is not considered in the JSON file. ZDC was monitored through DCS by the ZDC group and the HCal group during the data taking. Since run 326776, the ZDC performance was stable till the end of the PbPb data taking.

- l116: hmm, this does not factorise... I think Eq. 1 should use reco quantities in the numerator. Another way to see it is that you use this factor for a correction, which depends on what you measure, i.e. reco quantities. This is equivalent to a bin-by-bin unfolding, discouraged in general but ok in this case, because low pt muons have an excellent resolution.

- The reason for using GEN quantities in the numerator is to avoid the efficiency larger than 1 in relatively higher muon pt bin because of the smearing effect (even the effect is tiny, but our muon pt falls exponentially). As you commented, this is not an issue in this analysis because of the excellent resolution for low pt muon. We also did a closure test using the MC simulation and confirmed our current method can correct the reconstructed distributions back to true ones.

- ZDC noise. How noisy is it?
  - Check EmptyBX data to see if it is a problem.
    - The ZDC noise has negligible effect to 1n peak, ZDC group confirmed this using emptyBX data, please also see our response to the main items part for AN
  - Careful with out-of-time pileup though (see Mike Murray's comment during the preapproval presentation).
    - After a second thought, we think this out-of-time pileup, if any, has already been taken care during the pileup correction (AN Sec. 5.5), because this out-of-time pileup is independent with trigger and also shows up in zero-bias triggered data. Therefore, the probability of neutron migration caused by out-of-time pileup is included in our migration matrix.
- (In)efficiency from HF noise filter? (Do you apply HF noise filter?)
  - This is not an issue, From [8], one can clearly see the HF noise filter rejects almost all the hadronic (red distribution) events. Additionally, we require the event only contains 2 muon candidates and nothing else, which further suppress the possible hadronic contributions. There are only 2 same-sign pairs compared to 87066 correct-sign pairs, which tell us the hadronic contribution is negligible after applying HF noise filter and depositing Nmu == 2 requirements.
  - We also considered the systematic uncertainty of hadronic contribution caused by inefficiency from HF noise filter.
  - [8] NtrkHP distribution
- Rate of cosmic muons?
  - The contribution of cosmic muons is negligible. We **reversed the momentum vector of one muon candidate** and then calculated the DeltaEta and DeltaPhi distributions of two muon candidates in our analyzed events. If cosmic muons have sizable contribution, we should see a spike around 0 in DeltaEta distributions. The DeltaEta distribution of our data is smooth [9],

which demonstrates the contribution of cosmic muons is negligible. For DeltaPhi, it concentrates on 0 no matter from photon-photon interaction or cosmic muons.
  - [9] DeltaEtavsDeltaPhi; DeltaEta distribution

=== Section 4 ===
It would be good to cross-check these MC muon efficiencies with tag-and-probe. You could use your gammagamma to mumu signal, and fit the LO acoplanarity, similar to what was done in FSQ-16-012 for electrons.

- Thank you for your nice suggestions. We will do this cross-check of L1 trigger efficiency with tag-and-probe using the gamma+gamma to mumu signals from single muon UPC triggered data sample during the ARC review if you agree. Meanwhile, I naively think we can simply count the correct-sign pairs, which are true signals produced by LO+HO photon-photon interactions.
- We are currently using the official TnP efficiency for hyper soft muons [10] in 2018 peripheral PbPb collisions to check the possible effect caused by MC simulation. In this analysis, only soft muon and L1 trigger are involved, thus, the official TnP efficiency scale factors listed in [10] (for hyper soft muons in hadronic collisions) serve the up-limit from TnP correction. Regarding the trigger efficiency, the TnP scaling factor for hyper soft L2 trigger efficiency is basically flat in the kinematic regions used in this analysis, we expect the effect for soft muon L1 efficiency is even smaller. **The maximum effect to <alpha^LO> using the official TnP scaling factor of hyper soft muon is 0.3% and to <M> is 0.05%. Thus, the MC simulation in our UPC analysis is reliable.**
- [10] Official TnP efficiency SF for hyper soft muons in peripheral PbPb collisions

- there are some plots in the preapp slides, this study should be documented in the AN.

- Added them in the AN v4 section 4.3

- What background sources are the SS covering?
  - Combinatorial background from hadronic collisions. Please see our detailed response to l95 comment for paper draft
- Muon phi resolution might affect the extracted alpha width
  - Even with just the core resolution, it's on-par with the spread in the final result.
  - There are also the tails
    - We checked the effect caused by detector resolution through MC simulation, the effect is 0.7% (GEN <alpha> = 1.35e-3 vs. RECO <alpha> = 1.36e-3).

== Section 5 ==
- l163: hyper-soft?

- To enhance statistics during ZDC response analysis, at least two muon candidates are required regradingless muon types because the ZDC response should be independent with muon types.

- l168: sqrt(2)-2?

- Fixed in AN v4. sqrt(2)-2 means between sqrt(2) and 2. If the detector resolution is super good, the width of 2n distributions should be sqrt(2) wider than that of 1n.

When the detector resolution is not that good, the width of 2n peak is roughly a factor of 2 wider than that of 1n. Our ZDC energy resolution is ~ 22% to 25% according to the multi-gaussian fit results.

- Fig. 7: what you call "efficiency" is the efficiency for the cut, but what about the efficiency of detection? Could it be that some neutrons fall in a region where the detector is inefficient and leave no ZDC signal (or consistent with 0n)?

  - We discussed this topic with the ZDC group at the very beginning. The answer is ZDC has ~100% detection efficiency for 1n which are dominantly from Giant Dipole Resonance (GDR). The neutron energy from electromagnetic dissociation is close to beam energy (2.51 TeV), it will be detected if neutron falls into ZDC geometrical acceptance. Oliver Suranyi did a simulation of ZDC acceptance for GDR neutrons in [11] (p8 and p9), the acceptance is >99.9% based on the slides and private communication with Oliver.
  - [11] Geometrical acceptance of ZDC

- l222: how do you estimate the EMD xsec for 5TeV?

  - This number is just used for illustrating the pileup effect could be large. To avoid confusion, we removed this number and just mention the EMC xsec for 5 TeV is larger than that of 2.76 TeV which is measured to be 187.4 b, as we did in our paper draft. The number of 220b is from Oliver's forward PInG presentation in [12] (p11).
  - [12] EMD xSec for 5 TeV

- Section 5.5: I am not sure I fully understood this section. So you're assuming that all events passing your ZB selection are in the 0p0m true bin, right? If bin migrations are due to pileup, then the migration matrix elements should depend (linearly?) on the instantaneous luminosity, right? Have you compared two run periods or sets of LSs to see if the scaling with instantaneous luminosity is as expected?

  - Right, all the events passing our ZB selections have no hadronic, photon-photon or photon-nuclear interaction. If there is no electromagnetic dissociation, the ZDC should have no response (0p0m).
  - Right, the neutron multiplicity migrations due to dissociative pileup should have luminosity dependence. However, the zero-bias data have the same run period as our UPC data, the inclusive migration matrix should already take care of the luminosity effect and represents the true migration probability happened in UPC data.
  - To check the luminosity dependence, we divided the data into two run periods (low luminosity: 326776-327126, high luminosity: 327147-327564) according to the official luminosity webpage [13] and instantaneous luminosity of each analyzed LS [14]. We do see the migration probability (9.86%, [15]) of low luminosity period is lower than that of high luminosity period (10.89%, [15]). Anyway, as we explained in the last bullet, the luminosity thing is not an issue because UPC and zero-bias data are from the same run period.
  - [13] Peak Luminosity vs. day
  - [14] Instantaneous luminosity for each analyzed LS
  - [15] Migration for low luminosity; Migration for high luminosity

- Section 5.7: I'm not sure about this procedure. Here you completely ignore the data in the upsilon region and replace it with the fit. Maybe a slightly better way would be to do a local fit of the continuum+peaks, and subtract the peaks from the raw distribution?

- We agree that a more standard way is using continuum + peaks to fit the distributions. However, these two methods should give consistent continuum contribution underneath upsilon peaks if both methods have good fit quality. In this analysis, the overall Upsilon significance is <10 sigma and the significance in differential bin is worse. We cannot even observe upsilon peak in some neutron multiplicity bins, which makes continuum + peaks fit much harder. Last, the maximum mean mass difference between **inclusive measured mass spectrum (continuum+upsilon) and upsilon subtracted mass spectrum in 8<M<60 GeV** is <1.4%. We believe the difference between our method (extrapolation underneath upsilon) and your proposed method is **far less than 1.4%**, which can be negligible.

\- Fig. 25: there seems to be 4 parameters in your function... Probably not a 2nd order polynomial then.

- Good catch! Fixed them in AN v4. We did use a 2nd order polynomial function, meanwhile we used a 3rd order polynomial function fit to the same distribution to estimate the systematic uncertainty. All the fit curves in this plot are correct but the FitOpt boxes are for pol3.
- Is ZB adequate to extract the pileup probabilities? Naively I would expect that things will look different because in one case we require two muons, and in another, we veto any tracks.
    - Zero-bias data is sufficient to extract the pileup probabilities. The pure electromagnetic dissociation (without any tracks) is independent with the photon-photon interaction (two muons). Here, we use the select zero-bias events without any tracks to estimate the occured probabilities of various EMD processes and then derive the neutron multiplicity migration probabilities.
    - Also, what's the uncertainty on those ZB numbers?
        - The uncertainty of 0p0m is 8.4e-5 while the XpXm uncertainty is 1.9e-3
- Figure 12
    - Why is the muon pair efficiency not flat around alpha = 0?
        - At this extremely low alpha region, the slightly unflat structure should be cased by the detector resolution.
    - Are there any corrections to efficiency to account for the data-MC difference?
        - We added relevant discussion in AN v4 section 4.3
        - We used the official TnP scaling factors (tracking and muon identification) for hyper-soft muon [10] to check the possible effect caused by data-MC difference and found to be less than 0.3% to <alpha> and <0.05% to <M>. The effect on soft muon is expected to be smaller than that to hyper soft muon. Meanwhile, we are doing the cross-check for the L1 trigger efficiency using gamma+gamma to mumu pairs according to your suggestion.
        - [10] Official TnP efficiency SF for hyper soft muons in peripheral PbPb collisions

=== Section 6 ===

\- l330: why 5 GeV?

- Please see our response to the l157 comment on the paper draft. We added some discussion for this threshold determination in AN v4

- l346: why is the correction not applied by default?
  - First, we demonstrated that the effect caused by neutron contamination is negligible. Second, although multi-Gaussian function is widely used in our community to decouple neutron peaks and does a reasonably good job, we can see there are still some tension between multi-Gaussian function and our data. The contamination probability is not super accurate, thus we would like to treat this correction as a systematic uncertainty. However, We are open to hearing your further suggestion regarding this question.

- Section 6.4: will need to be updated when upsilons are better accounted for. In particular, you can't rely on STARLIGHT to predict what happens close to upsilons: it would be then more honest to just remove the upsilon region, rather than using simulation and inject it as part of the "data" measurement... or include upsilons as a 3rd component in the alpha extraction?
  - We do not use STARlight to predict the behavior of inclusive dimuon production close to upsilon region, we just use STARlight to predict the acoplanarity of coherent photoproduced upsilon and estimate its effect to the dimuon acoplanaity from LO photon-photon interaction in 8<M<60 GeV.
  - The current method we used is basically the best thing we can do because of the following reasons: 1) the dimuon mass spectrum of gamma gamma interaction roughly follows M^{-4} decreasing. The yield ratio of 8<M<11 GeV over 8<M<60 GeV is ~47%, we cannot afford the statistical loss to remove the upsilon region (8<M<11 GeV). 2) It is almost impossible to add a 3rd component in the alpha extraction because the upsilon yield fraction is tiny (~0.6%) and **the acoplanarity of coherent photoproduced upsilon is comparable to that from LO photon-photon interaction,** thus we do not have distinguish power for adding upsilon component. 3) As we commented in 2nd item, the upsilon yield is tiny compared to dimuon yield from two-photon interaction in 8<M<60 GeV and STARlight does a reasonable job to reproduce the pt spectra of photoproduced vector mesons [16, 17] (If pt spectra can be reproduced, the acoplanarity distribution must be reproduced). Thus it is appropriate to include upsilon mass region to dramatically increase the statistics and treat **the negligible effect** as one systematic uncertainty using the predicted shapes from STARlight.
  - [16] STAR, PRL 102 (2009) 112301, PRL 123 (2019) 132302
  - [17] ALICE, EPJC 73 (2013) 2617, PLB 718 (2013) 1273, PRL 113 (2014) 232504

- Section 6.5:
  - Suggest to try an unbinned fit and get rid of the binning uncertainty -- is there any technical difficulties in doing so?
    - Please see our response for l145 comments on our paper draft

- Fig. 33: you need some smoothing of the systematic uncertainties to remove fluctuations.
  - Done, added them in AN v4

Presentation Comments

- CCLE: announce on HN, cc pubcom
- MM: proposal for a xcheck: there could be interaction of out-of-time pileup (OOT PU). There are cuts to get rid of that. Need to look at it. Basically need to check that in the previous collision (75ns before) there was not a very large signal which could spill over the BX of interest.
    - WL: also affect the ZB trigger?
    - MM: 2 easy ways: 1/ look at time slices 0 and 1, and 2/ look at events that are at the beginning of bunch trains. Will send details offline. Independent of trigger. Tendency = increase the neutron multiplicity, everywhere.
    - WL: but OOT should also show up in the ZB study
    - MM: correct, so it may already be taken into account
        - After a second thought, we believe this effect has already been taken into account in the migration matrix, because this out-of-bunch is independent of trigger
        - We are also working with ZDC group to do this cross-check to see the absolute contamination of this out-of-time pileup
- Which muon trigger is used?
    - * SingleMuOpen + NotMBHF2AND
- MC: private or official?
    - * Private!
    - * Official MC submitted
- Upsilon exclusion?
    - * Maybe remove 8-11 completely?
    - * Or start from 11?
        - Please see our response to the section 6.4 of AN
- Tail under neutron peaks?
    - Now just Gaussian.
    - Should try some uncertainty
        - Please see our detailed response in the main work part of AN.
- LO vs HO shape?
    - Empirical. No theory exists.