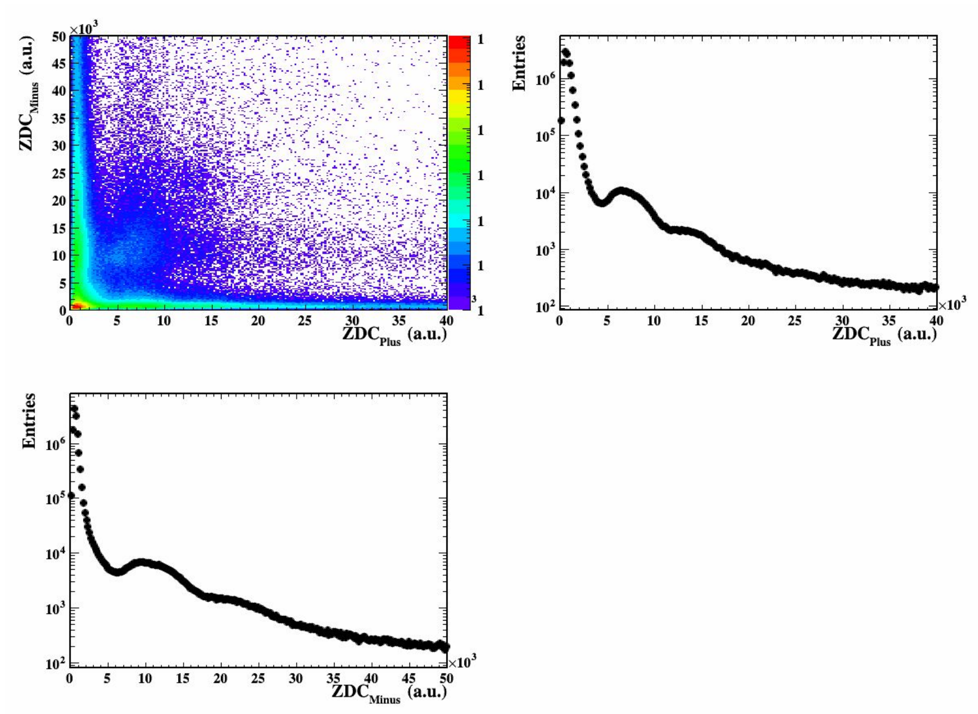
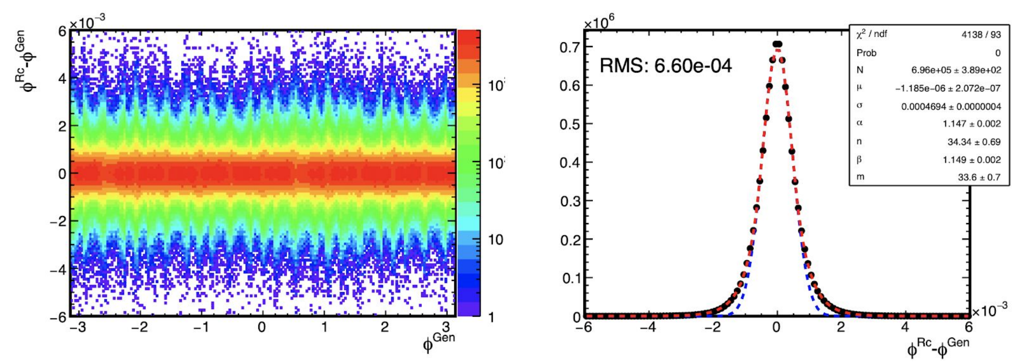
HIN-19-014 approval

Indico for approval presentation
Paper v3
AN v5

General

- What is the neutron detection efficiency? If an interaction emits a neutron in our ZDC acceptance, what percentage of the time we can detect it?
    - As the ZDC group suggested, the efficiency is essentially 100% considering the neutron energy is ~ 2.51 TeV when the neutron falls into the large ZDC acceptance. Oliver Suranyi did a simulation of ZDC acceptance for GDR neutrons in p8 and p9 of geometrical acceptance of ZDC
- For the strength of physics message in the paper
    - Do you have access to a plot of (impact parameter) vs (n-neutron detected)?
        - No, we don't. It is up to the theorists' model.
- We should not use LO vs HO. This is wrong.
    - Couldn't "HO" include very peripheral Drell-Yan, especially for XnXn? And maybe other backgrounds.
        - Considering the N_trk^HP == 2 and HF veto requirements, the contribution from inelastic collisions should be negligible, otherwise, we should see a sizable same-sign background.
    - Also, LO-HO interference, even if small
        - Changed "LO vs. HO" to "Core vs. Tail"
- Please provide ZDC distribution for no-beam events
    - We used the empty data in /HIEmptyBX/HIRun2018A-27Feb2019-v1/AOD with HLT_HIL1NotBptxOR_v trigger to check the ZDC distributions, as shown below. Because the ZDC uses the charge in both the current BX and the following BX and the HLT_HIL1NotBptxOR_v trigger only avoids beams in the current BX, there is still a chance to get collisions in the following BX. Therefore, we still observe some neutron signals in empty BX data. We discussed the pure noise distributions with ZDC group a little bit, it is not straightforward to get them. For instance, we need to reject the triggered empty BX just before a BX with two beams showing up, so we just stop here.
    -

- 

- Can you point to the study of the phi detector resolution check? (The 0.7% you mentioned in the preapproval homework) This should also be commented on somewhere in the paper.
    - We commented the phi resolution in detector introduction section
    - 

- Same sign: why combinatorial? Aren't there only two tracks in the event?
    - There could be two types of background for the same sign pair: one is our detector flip the charge sign of one track to generate the same-sign pair, the other is our detector loses some tracks of inelastic collisions during reconstruction. Alice (muon POG convener) commented that the probability of charge sign flip for low-pt muon (<~30 GeV/c in this analysis) is extremely low (<10^{-5} for pt<100 GeV/c). Therefore, the observed 2 same-sign pairs are likely from inelastic collisions, so called combinatorial background.
- Please do a beam intensity check to make sure your PU correction closes. Take data, split into high PU half and low PU half. Check that the results are consistent.

    - We did this study and the results are consistent in low and high luminosities. The results can be found in the backup slides of approval talk.
- Have you cleared the significance calculation with the statistics committee?
    - Contacted the statistics committee for our estimation procedure. The same method used in HIN-19-003 (conversion from a chi2 probability to a significance based on a gaussian) has been approved by the statistics committee.

Other items

- Luminosity group released a new luminosity calibration for 2018 HI run (https://hypernews.cern.ch/HyperNews/CMS/get/luminosity/948.html), This calibration represents a decrease of approximately 3% relative to the preliminary recommendation. For instance, from 1.57 nb^{-1} to 1.52 nb^{-1} for our analysis. Because we did not report absolute cross-section in this analysis, our results won't be affected at all by this luminosity change, but need to update the luminosity number in the plots (from 1.6 nb^{-1} to 1.5 nb^{-1}).

- We introduced a weight procedure for the pileup correction (p19-20 in https://indico.cern.ch/event/910781/contributions/3831632/attachments/2026535/3390298/20200424_HIN19014_Approval.pdf) after GL, we believe this is the most correct way to make sure the pileup probabilities between ZB and UPC triggered data are the same. In the approval presentation, I thought the ZB sampled luminosity is roughly "flat" because of the constant DAQ rate (right plot in p19 of approval slides) and used the "flat" distribution to be as the denominator to generate weight number for each analyzed LS. However, the prescale of the ZB trigger is NOT relevant to luminosity but only related to the LHC clock, which tells us **the prescale of the ZB trigger is more or less constant from high luminosity to low luminosity periods. If everything ran perfectly in 2018 HI data taking, the ZB trigger sampled luminosity should have the same luminosity shape but with a large constant prescale compared to UPC trigger sampled luminosity.** Now we use brilcalc tool to grab the ZB trigger sampled luminosity as we did for the UPC trigger and then generate weight for each analyzed LS. The weight now mainly takes care of the fine luminosity structure difference between UPC and ZB triggers. The updated results are almost the same as that in green light. The details can be found in https://drive.google.com/file/d/1fC4EE5tGegdxwBQxMOSDe_qAKENoSDMw/view?usp=sharing. I am truly sorry for this back and forth, if you need an audio discussion, please let me know.

Paper

- abstract, line 2: remove comma after TeV
    - Done

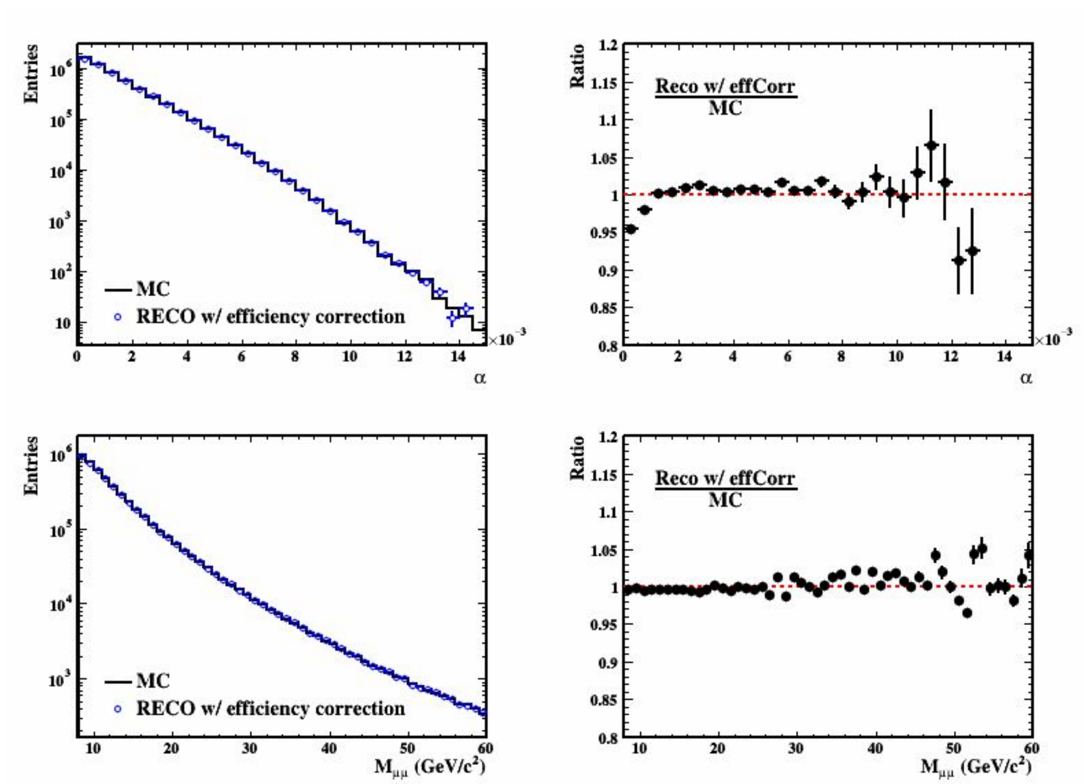
- abstract, line 3: azimuthal correlation -> azimuthal angle difference?
    - We would like to keep this term because the explicit definition can be found in the text
- Line 13: very low relative momentum.
    - Done
- Line 21: lorentz factor gamma and the photon gamma have the exact same font. Another font might be good for the boost gamma to avoid potential confusion.
    - Seems \gammaup or \upgamma doesn't work in tdr, currently we still use the \gamma
- line 23: use \ell for leptons
    - Done
- line 26: remove "regions"
    - Done
- line 26: the Relativistic…
    - Done
- line 47: people may be confused by the 1.6 nb-1 (as I was at first)... need to explain quickly that the lumi is lower than in other CMS papers using 2018 PbPb: because you require functioning ZDCs
    - Done
- line 48: remove comma at end of line
    - Done
- line 66: a few words on ZDC technology would be useful. From PubDetector: "The Zero Degree [and CASTOR] calorimeters are made of quartz fibers and plates embedded in tungsten absorbers."
    - Done
- line 70: the trigger system should have been introduced in the previous paragraph. See PubDetector for standard text.
    - We would like to keep the current description because the standard text does not work well for our case. Meanwhile, we checked the previous CMS papers and found lots of paper put the trigger description and event selection together in the paragraph after detector introduction
- line 74, offline selection: need to make the connection with the following sentences.
    - Rephrased the sentence to "For the offline analysis, events have to pass a set of selection criteria designed to reject beam-related background processes (beam-gas collisions and beam scraping events) and hadronic collisions. Events…"
- line 79: while the -> where these? or rephrase to better explain that these thresholds correspond to X% noise rejection.
    - Changed to "where these"
- Line 87: Following the YnZn convention is fine, but you need a short sentence explaining it.
    - The following sentence explains the meaning of YnZn convention
- line 99: upsilon: not removed?

    - For alpha results, the upsilon contribution is NOT removed because we cannot add addition upsilon component to the fit because of tiny contribution
- line 102: give STARlight version
    - Done
- line 108: efficiencies estimated how?
    - Removed “estimated and” because the first few sentences of this paragraph mentions the detector performance is estimated by MC simulation (STARlight + GEANT4)
- line 154: why 5 GeV?
    - This is a little bit arbitrary and pretty conservative choice. There are 87066 correct-sign pairs but only 2 same-sign pairs for the default HF veto thresholds, 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 also tried to completely remove the HF veto, giving us similar systematic uncertainty values. We are open to hear your suggestions if you are not satisfied with our method.
- line 157: corresponding to what?
    - Removed “corresponding”. Here we meant to subtract 2n contamination contribution
- line 174: significance estimated from the chi2 between the 2 points? How about correlations between systematic uncertainties? Same question about the constant fit.
    - Yes, significance between the 2 points is estimated from the sqrt(chi2). The systematic uncertainties of these two points are treated 100% independently.
    - The significance of the constant fit is estimated from p-value and the systematic uncertainties are also treated 100% uncorrelated. The p-value = fNormalGaussian->Integral(**X*sigma**, infinity) / fNormalGaussian->Integral(-infinity, infinity), where X is the significance. These formula gives us the same p-values for different significances listed in CMS large significance page (https://twiki.cern.ch/twiki/bin/view/CMS/LargeSignificances)
- Line 186: not clear. What about the upsilon? People will wonder what is the pdf - since you don't mention anything about excluding mass range.
    - Changed to “A second-order polynomial function fit to the mass spectra excluding mass region 9 $< M_{\mu\mu} < $11 GeV, is employed to extrapolate the contribution of $\gamma\gamma$ scattering underneath $\PgU$ mass region”
- Line 194-198: why is mass determined by energy? If you give a kick to some system as a whole, the energy increases, but so is the momentum, and mass stays the same.
    - Because of the back-to-back feature of dimuon from gamma gamma scattering, the pt of the muon pair is close to zero. Meanwhile, the pt of equivalent photons

is close to zero. The dimuon mass is approximately equal to the sum of scattering photon energies.

- Need a comment somewhere in the paper about the expected impact of dimuon mass resolution on <m>
    - The mass resolution comes from the pt resolution and the number is ~1% in this analysis and the pt resolution has been commented in the detector introduction section. Meanwhile, the <M> difference between GEN and RECO is ~0.02% in mass region 7<M<60 GeV (GEN: 12.864217 GeV vs. RECO: 12.866455 GeV)

AN

- Fig. 14: there is a wrong-sign event at ~28GeV in the bottom left panel but not top right?
    - Because the pt of one wrong-sign event is larger than 1 GeV
- Figs 22-23: if you weight reconstructed muons by 1/eff (parametrised for single muon pt, eta, phi), do you recover the generated distribution? This should be added to these plots.
    - We did the closure test and demonstrated the current efficiency correction method can recover the RECO distributions to GEN distributions. The closure test results can be found in the figure below, the small derivation for extremely small alpha is caused by detector resolution, which causes 0.7% on LO <alpha> result (GEN <alpha> = 1.35e-3 vs. RECO <alpha> = 1.36e-3).
    - 

- line 285: 54.06 mub in data is also for alpha<0.006?
    - Yes, the data and MC are apple-to-apple comparison
- Figs 24-26 would be nice supp.mat. plots? Maybe plotting dN/dX rather than dsigma/dX (since we're missing some systs for a true cross section measurement)
    - We would like to keep them as internal cross-check materials. Those four plots are not applied T&P SF and event selection efficiencies (MC cross section is filtered by event selection criteria).
- Eq. 5: the matrix only includes migrations due to EMD "pileup", and matrix elements above the diagonal are empty. But there is also the effect discussed in Section 5.2: the resolution of the neutron peaks also induces migrations (up or down). Why not account for both in the same matrix?
    - It is not trivial to add the neutron migrations caused by detector resolution in the matrix, because Xn in the matrix means >=2n and the resolution induced migrations comes from exact 2n.
- Section 5.8: this is not a subtraction, you simply replace the 9<m<10.6GeV mass region with the extrapolation from the fit… Text in the paper should also be corrected accordingly.
    - Done, please see our reply to your paper l186 comment
- Section 6: confused about what is done for the systematic uncertainties. Which of these two are you doing:
    - enlarge bin-by-bin uncertainties in alpha distributions by the red line in e.g. Fig. 40, then redo the fit to get <alpha> with a slightly larger uncertainty (stat + had.col. syst)
    - redo the whole analysis with the varied procedure and check the new <alpha> and uncertainty
        - For the systematic uncertainty estimation of all the measured results (alpha spectrum, <alpha>, and <M>), we redo the whole analysis and then compare the new results to that of the default case to estimate the systematic uncertainties. Two examples are listed below,
            - 1) Normalized alpha spectrum: We calculate the difference of bin contents between variation and default scenario in each bin and treat the difference as systematic uncertainty. The data points in Fig. 40 (AN v5) are the differences of normalized alpha spectra between variation and default scenarios. The error of each data point is estimated according to the recommendations for the estimation of systematic uncertainties in the HIN PAG. Then, a linear function (red line in Fig. 40 of AN v5) is used to flat the systematic uncertainties to suppress the effect of statistical fluctuation.
            - 2) <alpha>: We directly fit the alpha distributions of variation and default scenarios to get <alpha> for each case. The difference between two <alpha>s is treated as the systematic uncertainty arising from this source.

- Section 6.2:
    - HF veto threshold variation is arbitrary.
        - See our reply to your paper l154 comment, we also tried to completely remove the HF veto, giving us similar systematic uncertainty values.
    - Have you compared the HF distribution in emptyBX and selected events?
        - We did not directly compare the HF distributions in emptyBX and selected events but counted the fraction of events with leading HF tower energy below HF veto thresholds in both datasets. The fraction is ~98.5% in emptyBX while the fraction is ~93% in selected UPC events. That tells us there exists some beam induced background in HF calorimeters in UPC triggered data. Nevertheless, we checked the results of tight HF veto thresholds (5GeV), removing HF veto thresholds (not included in final results because of the comparable uncertainty as tight thresholds), and removing N_trk^HP == 2 which should well cover the effect caused by HF veto selections.
    - What is this uncertainty addressing exactly? If hadronic contamination, there already is another syst. If possible activity in the HF for dissociative events, couldn’t it be checked in another way?
        - Because N_trk^HP == 2 is required in this analysis, the tight HF veto thresholds should not affect the hadronic contamination thing. This source mainly accounts for the systematic uncertainty of pileup correction (the migration probabilities shown in Fig. 39 in AN v5) caused by different event selections. We changed those HF veto thresholds in UPC and ZB data at the same time, and then redid analysis to extract the final results.
    - With the current estimation of this uncertainty, differences are likely to be (partly or fully) due to a statistical fluctuation (smaller data sample with the tighter threshold)
        - See our reply to your last bullet
- Section 6.3: correction for ZDC resolution should be default! Also (see above), it could be included in the EMD correction
    - It is not trivial to add the neutron migrations caused by detector resolution in the matrix, because Xn in the matrix means >=2n and the resolution induced migrations comes from exact 2n.
    - We demonstrated that the effect caused by neutron contamination is negligible. Although multi-Gaussian function is widely used in our community to decouple neutron peaks and does a reasonably good job, we can see there is 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.
- Section 6.4: changing from 2nd to 3rd order polynomial changes almost nothing, but how about changing the mass window for the extrapolation? (e.g. extending it, or restricting to 1S only)

    - Now, when we change from 2nd to 3rd order polynomial function, we also change the pol3 fit range (8-16 GeV -> 8-14 GeV) at the same time
- Section 6.5: confused about what is done here. The variation is a variable bin width where each bin has >3sigma? does it mean 10 events / bin?
    - Require the mean/error of each bin is > 3
- Section 7.1: shouldn't we quote this integrated result in the text of the paper?
    - Good suggestion. We added the integrated result in the paper text
- Line 506 and following: need to explain how you obtained these "significances". For instance, what did you assume for the correlation of systematic uncertainties?
    - Added the reply to your paper l174 comment into updated AN (v6)

Presentation and discussion

- Page 26: Theorists say that the last two diagrams (real emissions) are dominant
- Yenjie: Why no muon SF?
    - Shuai: Practical reasons
- Yenjie: Unfolding systematics?
    - Shuai: HF veto threshold contains part of this
    - Yenjie: is there some run number by run number dependence?
    - Shuai: we do weight by lumi section
- Yenjie: what's the deal with the interference effect?
    - Shuai: Last two diagram have different final state, so no worry about interference
    - Yenjie: There are some small fraction from ii, iii, iv. So there are still some interference. What is the difference betwen <alpha> and <alpha^core>
    - Shuai: higher order contribution is very different for different bins. Also no data for alpha > 0.25
    - Yenjie: we can say for example mean alpha up to 0.01 or so
    - Shuai: otherwise theorists can't compare LO calculations to these. Many models on the market are LO.
    - Yi: LO is wrong. Could use e.g. core vs tail.
    - Shuai: will work with ARC
- Michael: why do you call it closure test of LO model in page 28
    - Shuai: lowest order QED process we don't expect dependence.
- Wolfgang: You fit outside range and take the mean from the fit right? The procedure is fine, but the plot is misleading: remove markers on extrapolation under upsilons
- Wolfgang: what is done with upsilons for <alpha>
    - no subtraction. small effect, included in systematic uncertainties
- Daniel: alice gamma gamma dimuon measurement. Can we compare 0n0n with them?

    - Shuai: it's not quite similar
    - Daniel: deviation from STARLIGHT could be good to mention in paper
- Daniel: EM PU?
    - Shuai: Yes, UPC PU
    - Daniel: trigger level bias from this?
    - Shuai: should be ok.
- Daniel: impact parameter. Do you have a photon flux estimation?
    - Shuai: not trivial to do. Nothing in the market does impact parameter dependence.
    - to be addressed offline
- Quan: page 16. It's not ADC, it should be charge. But usually in public plots we use "arbitrary units"
    - Shuai: will change
- Michael: nice neutron migration analysis. Also, we see a strong correlation between ZDC response and inst. lumi.
- Yenjie: fit quality of neutron peak? Are those approved? Are all the details understood and well studied?
    - Shuai: shape of neutron peaks = energy resolution + intrinsic momentum spread of neutrons.
    - Michael: acceptance is huge (ZDC big compared to neutron transverse momentum kick), also efficiency (neutron energy ~ beam energy).
    - Yenjie: how do we model the shapes?
    - Michael: energy resolution of ZDC. Momentum spread contributes significant to the spread of the neutron peak.
    - Yenjie: any tail part?
    - Shuai: we don't. Gaussians are used everywhere for neutron peaks, like alice.
    - Wolfgang: have you produced total gaussian / data (ie data/fit ratio)? Would be good to see.
    - Yenjie: would be good to plot the extracted spectra etc.
    - Shuai: there is yield in AN.
- Yi: have you checked high lumi vs low lumi?
    - Shuai: yes, cf backup slides 43 and following
- Yenjie: what physics we can extract? In light of ATLAS etc. initial state vs final state
    - Shuai: don't know initial state vs final state. We need theorists to match to our data, then learn something.
    - Yenjie: are there calculations already on the UPC?
    - Shuai: not yet.

ARC (Begona)

- Interaction is quick and analysis is in good shape.
- Review fast and pleasant.

- Main point: muon definition, muon SF
- One further comment: whether to consider just one representative plot for alpha, or all in paper.
- Last correction about the lumi weighting came a bit late. But author responds fast.
- Happy!