CMS-HIN-19-014-002-COMMENT-001

Type A:
L1: "Lorentz-boosted"

- Done

L24-25: "UPC collisions". "C" in "UPC" already stands for "collisions", therefore the fully-spelled "collisions" is a repetition. Please remove or change it.

- Changed “UPC collisions” to “UPCs”

L51: "less of an effect" -> "a smaller effect".

- Done

L99: "same charge sign" -> "same sign" (for consistency with L97 above, where you just write "opposite sign").

- Done

L101: "and rapidity |y^mumu| < 2.4." -> "and |y^mumu| < 2.4, where y^mumu is the dimuon rapidity."

- Done

L135: "WHERE N_s represents THE signal yield."

- Done

L210: "quark-gluon-plasma-induced" -> "QGP-induced"

- Done

Type B:
L30: "in hadronic events". As far as I can tell, the papers cited in refs. [29] and [30] propose possible explanations or the "broadening effect" that do not involve hadronic interactions. Therefore, it is not clear to me whether one can say with certainty that the "broadening effect" is tied to hadronic events. Maybe it would be better to change this sentence to "in more central events", or something similar?

- The “broadening effect” is not caused by or does not involve hadronic interactions. Photon-photon interactions can also happen in hadronic collisions (b<2R_{A}) and the “broadening effect” means the lepton pair <pT> or <acoplanority> from photon photon scattering observed in hadronic collisions is larger (“broadening effect”) than that in UPC. The reason of emphasizing hardronic events is that QGP shows up in hadronic events and the observed broadening of lepton pairs might be caused by the EM interaction between lepton and QGP (final-state effect).

L30: "broadening effect". What is this effect? Does it mean that one gets a lower degree of angular correlation when photon-photon scattering in more central events is

considered along with the UPC? In general, this formulation looks like jargon and I think that it should be changed to something more explicative (mentioning at the very least which distribution is "broadened" by this effect).

- The pT or acoplanority (azimuthal angular correlation) distribution of lepton pairs from photon-photon scattering in more central events (hadronic events) is wider than that in UPC.
- We rephrased this sentence to make it clear.

L51: are the phi angles defined in the lab (i.e. CMS detector) rest frame? Can you please specify in the text?

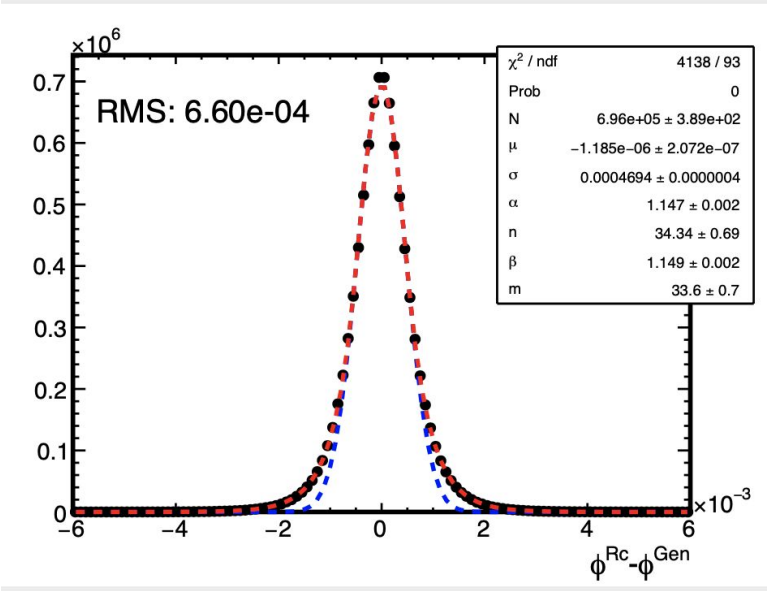
- Added “in the lab frame”.

L55: what do you mean by "the CMS"? Is it the center-of-mass reference system, or the CMS detector? In the latter case, maybe it would be better to write "with the CMS detector"?

- Changed to “with the CMS detector”

L66: where do you get the azimuthal resolution from? Can you give a reference for that too?

- We measured the azimuthal resolution by ourselves.

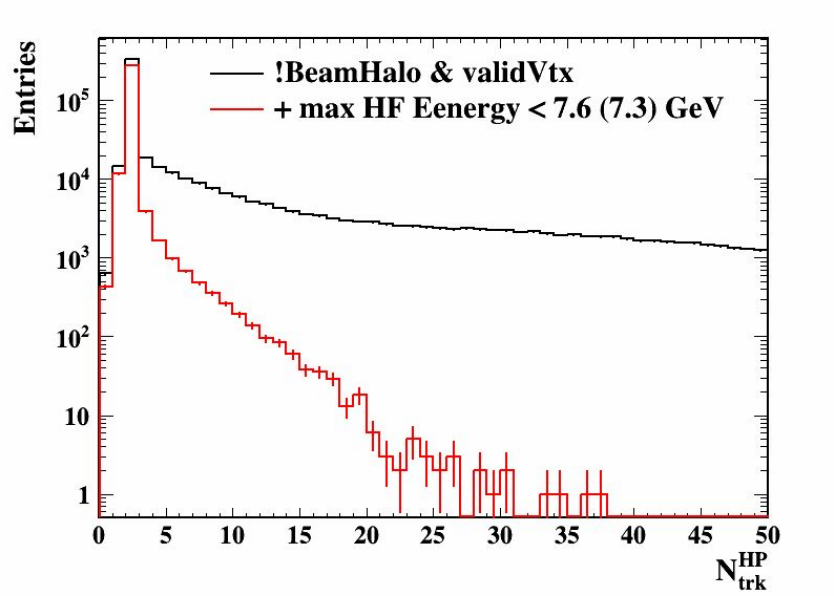


L77-79: I guess that these are standard cuts applied over a range of different HIN analyses. Is it correct? If so, can you provide a reference where the reader can find these selections?

- In the following sentences, we explained the standard cuts with corresponding reference.

L82: how do the thresholds of 7.3GeV and 7.6GeV (on + and - sides, respectively) compare with those applied in the trigger (see L76-77)?

- From the N_{trk}^{HP} distributions of single muon UPC triggered data without and with offline HF veto (7.3GeV and 7.6 GeV), one can clearly see online triggered events (black histogram) have significant hadronic contributions compared to HF vetoed events. Therefore the online thresholds are much looser than offline ones.

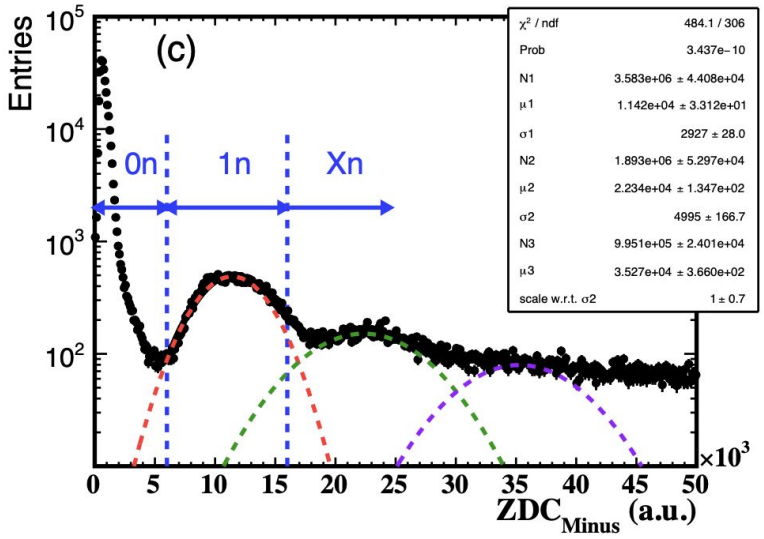


L88: "on each side with energy thresholds". I understand that you mean that you use different energy thresholds on each side to make the classification. Is it correct? This is rather cumbersome and looks like it should belong to the sentence written above, in LL85-86. Maybe you can drop "with energy thresholds" from here and mention the different (side-depending) thresholds in the sentence in LL. 85-86?

- Correct. Removed “with energy thresholds” in LL. 87-88 and added “with different thresholds” in LL. 85-86.

L88:

- How do you define the purity? Is it S/(S+B) or something else?
- "a multi-Gaussian function fit to the energy distribution." It does not explain what you are modeling with these multi-Gaussians. I'd propose to either add more information or drop the "multiGaussian" mention.



- We use one plot here to answer these two questions: 1) For each selected neutron multiplicity class (determined by straight cuts/thresholds), we have contamination from adjacent multiplicity classes, e.g. for 0n class, it has contamination from 1n. The purity is calculated by (N_{measure} - N_{contamination}) / N_{measure}. N_{contamination} is estimated by the gaussian fit. 2) Multi-gaussian function is widely used in the heavy-ion field to decouple neutron numbers. Each gaussian component represents one neutron multiplicity.

L90: do you understand the drop of purity for the 1n class, with respect to both the 0n and the >=2n?

- 1) The statistics of 0n and >=2n classes are much larger than 1n class 2) 1n class has contaminations from both sides (0n, =2n).

L100: "triggered muon": it's not the muon that is "triggered", but the event. Can you please rephrase?

- We mean this muon triggered the event.

L101: I would drop "limited to" (it conveys the idea that the range could have been bigger, but something limited that).

- Done

L101-103: the mass range considered does not suppress bottomonium. I think that you should spend some words here on the fact that one expects anyway to have Y contamination in the sample.

- This is true. We discussed the Y contamination to the measured acoplanarity results and Y subtraction to the measured mass results in the systematic uncertainty discussion.

L119: Is the "b" in the EMD value the impact parameter? If so, it should be made (typographically) consistent with the symbol defined in LL3-4 (i.e. put everything in italics or everything in roman).

- No, the “b” means barn and represents the EMD cross-section.

L129: are you using any regularization to invert the migration probability matrix?

- We used the Invert() function of TMatrixD in root, saying the most general (Gauss-Jordan) matrix inverse.

Figure 1 and related discussion: I notice two features in Fig. 1 that do not seem to be discussed in the text:

1. The tail in the 0n0n case has a "bumpy" structure at around 0.02<alpha<0.07. In other high-statistics bins (like 0nXn and XnXn) the fit describes much better the data points. Are there any specific processes that could contribute to these bumps?

- We believe these bumps are caused by unfortunate fluctuation. We don’t have a clue of other specific processes causing these bumps.

2. The tail for the 1n1n case is almost absent. Is it an artifact due to the low statistics or a real effect? In the former case, how can one trust the steeply-falling exponential returned by the fit for the tail component? Have you considered the systematic uncertainty to the core yield in this bin coming from the less-than-perfect modelization of the tail part?

- The absence of the tail for 1n1n class should be caused by the low statistics. The core and tail functions in this paper are empirical, so we report the acoplanarity spectra to the community just in case our empirical LO and HO models are not perfect. In this sense, we do not consider the systematic uncertainty related to the core and tail functions which is artificial. With respect to the trust of 1n1n tail function selection, we barely have other choices because of the low statistics. However, the systematic uncertainty of 1n1n is somewhat overestimated, compared to that in other neutron multiplicity classes, because we cannot fully decouple systematic uncertainty from large statistical uncertainty.

Please comment on these features (if needed also in the paper text.)

CMS-HIN-19-014-002-COMMENT-002

Dear proponents

Thanks for your paper on the observation of forward neutron multiplicity dependence of dimuon acoplanarity in UPCs in lead-lead collisions.

Excellent that we are reporting a first observation of this experimental correlation/effect measured with CMS data!

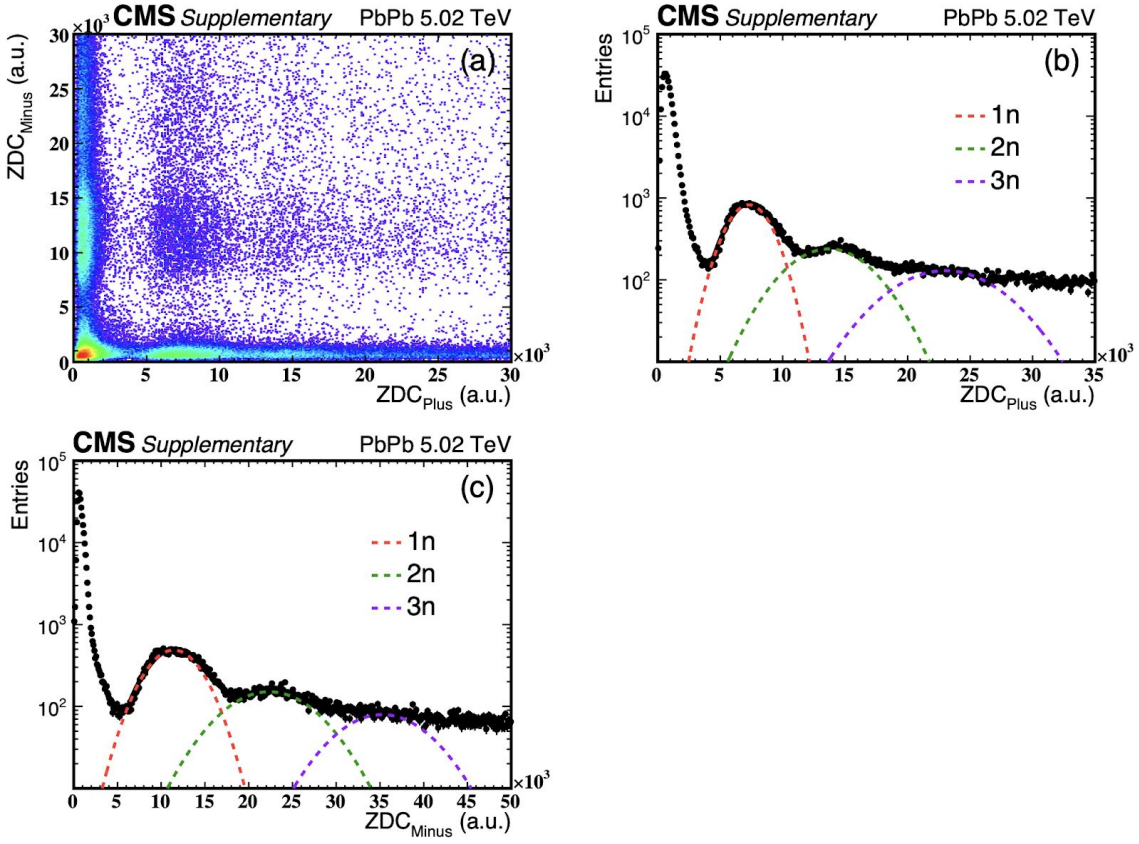
In all I found it a bit of an unusual paper:
- We show no basic experimental distributions for the ZDC. This is still a relatively scarcely used detector in CMS so we really need to show for example an energy spectrum of the ZDC and how visible these different multi-muon event categories are, ie inform the reader how easy/difficult it is to separate these. (I believe it is kinda easy to see, once you show the plot)

In general we have to demonstrate clearly that we have a detector that can measure the signal we look for.

It is not that I personally doubt that, but I am talking for any reader in general.

NB I think eg on some the plot(s) as presented by ALICE in ref [38] for their ZDC detector response.

- We now put the ZDC performance plot in supplemental materials, which will also be submitted to journal

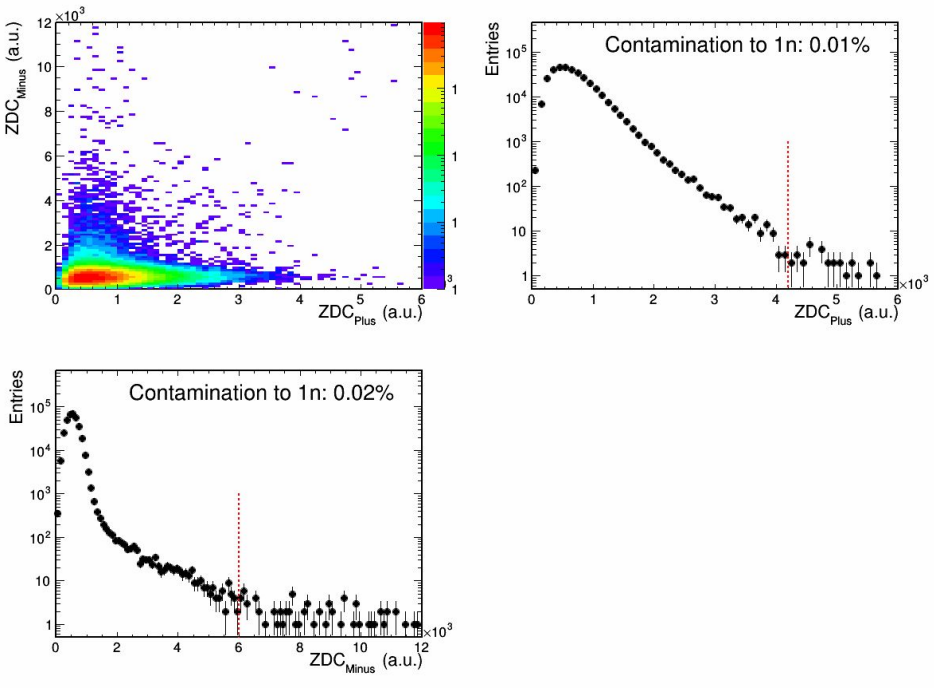


- What is the background you get in the ZDC that is related to the proton beam (secondaries etc)?

What can we say on that? Is that no issue/worry for the analysis?

Do we have eg some data taken with just a single beam circulating (which has for sure no UPC/EMD physics process background in it)?

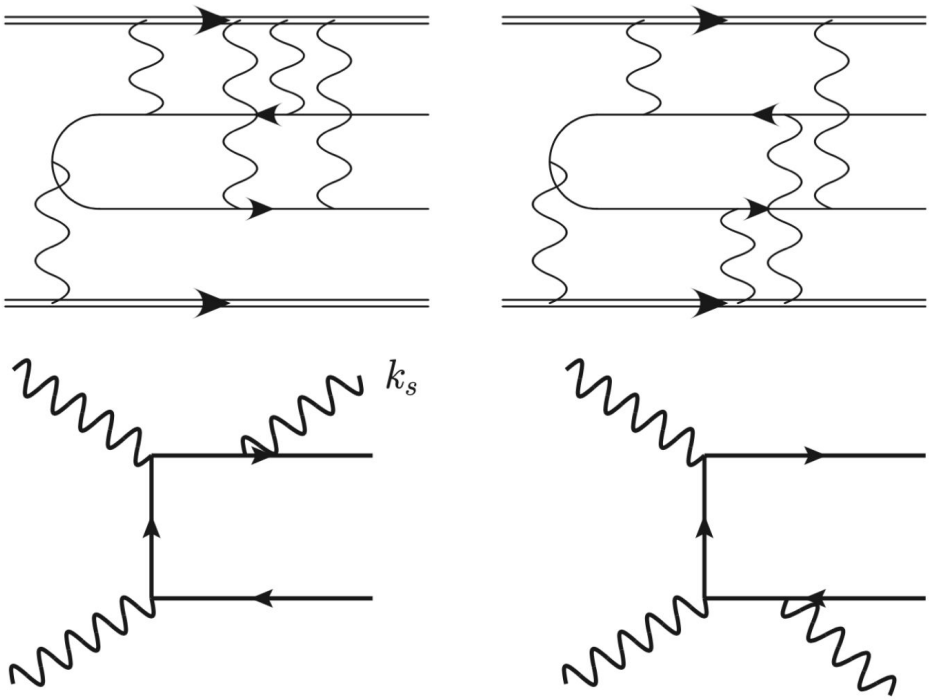
- In ultra-peripheral collisions (b>2R_{A}, two ions physically miss each other), the forward activity ONLY comes from the emitted neutron with beam energy (2.51 TeV in this analysis) from electromagnetic dissociation, we do not need to worry about the other physics background , e.g. photon.
- We checked the ZDC distributions using the empty BX-s between bunch trains. The ZDC noise distributions can be found below and the noise tail contribution to neutron peaks is found to be negligible



- On the detector level: please explain how do you define a neutron as compared to a photon with the ZDC? Are photons included in the general energy spectrum used to find the neutron multiplicity, or are these anti-selected? Do they play no role because of the much lower forward high energy photon rate expected?
  - As we explained in your last comment, we do not expect forward photons produced in ultra-peripheral collisions. Meanwhile, the energy of neutrons detected by ZDC is ~2.51 TeV and the rate of such high energy forward photons must be negligible even in hadronic PbPb collisions.
- Did you try to use any other MC program than STARlight? What about the program referred to in ref 13, which I guess is a product of the ALICE collaboration? Would that be usable for the kind of studies reported here?
  - No, we did not. The program referred to in ref 13 is only for vector meson produced via photon-nuclear interactions with neutrons emitted from electromagnetic dissociation.

- For the non-expert reader: a diagram of the process that we discuss here could be helpful.(and perhaps an exam please of the hadronic background, see detailed comments below)
- I strongly hope we will not use the lack of space as a reason to avoid adding such information :)

- We really would love to add those diagrams as we did in our AN v7 (Fig. 40) for clear illustration. However, we do not think it is necessary for a PRL paper. These are well-known QED diagrams.



This paper has really a nice and important measurement but it gets somewhat undersold by the way out is presented now and the guesswork a non-expert reader has to do... I therefore call upon that we try to improve the presentation. I assume you will get more detailed comments on wording from other readers.

Details

- line 13-17: so it means that for our collisions here "b" is larger than the size of the nucleus, right?

- Right, b > 2R_{A}

- line 22: ... of the projectile and target nuclei.." what is meant exactly? the gamma factor of each of these?

- The projectile and target nuclei means the two ions participating in the UPC and they have exactly the same gamma factor in this analysis.

- line 30: What exactly is the broadening effect? Does it mean that the <pT^2> you find in data is larger than in expected for UPCs, and changes with centrality? This is what I would guess here but readers should not need to guess... Nor should they have to go to other papers to find out the very essence of this study presented in this paper [24,25,28], so please add a clarifying 1/2 sentence…
    - Yes. The <pT> (<acoplanarity>) of the leptons pairs from photon-photon scattering in hadronic collisions is larger or pT (acoplanarity) distribution is wider than that in UPCs.
    - We rephrased this sentence to make it clear
- line 36: So this gives broadening here without the need for a QGP, right? If I misunderstood, please explain better in the text.
    - Right, your understanding is correct. Line 36 means the broadening might just come from initial effect instead of EM interactions with QGP.
- line 42-43: Apart from the light-by-light scattering can we just list a few of these BSM opportunities that this process can help us to shed light on? I assume these are in ref [7], but that is a document of more that 200 pages :) (and no table of contents :(!) Do you mean with BSM physics here eg new particle production (like the axions in LbL) or new effects in QCD (like QGP, modified parton distributions, ridge effects in correlations...)? I have no clue.
    - Our study directly improves the initial photon kinematic modeling and then benefits all photon induced interactions. The most relevant BSM opportunity mentioned here is the axion search using LbL process. Considering this sentence won't add much physics message, we now removed the sentence "and searches for physics beyond the standard model [7, 9–11]".
- line 48: We claim here to use a luminosity of 1.5 nb-1, which is less than the 1.7 nb-1 in the two other papers in CWR from the HIN group right now. Does it mean we don't use all data or do we have less data due to eg detectors not ready, or more severe data quality requirements? Eg if the ZDC was not on the whole time this could happen of course. Perhaps we explain in a few words, or we use the same integrated luminosity as in the other papers.
    - Right, the luminosity 1.5 nb-1 used in this analysis is lower than the 1.7 nb-1 in the other two HIN papers because ZDC was not functional at the beginning of data taking in 2018. We removed the ZDC related information during ARC review and now added it back.
- line 55 "in the CMS" -> "in CMS" (I assume you mean the CMS experiment and not the centre of mass system)
    - Changed "in the CMS" to "with the CMS detector"
- line 56: What is the resolution in pT for the typical phase space that we use in this analysis? Is it the 1% mentioned in line 56, i.e. is that the relevant number here? (then it

is ok). And for the azimuthal angle measurement we have 0.7 mrad (or better) resolution for the objects in this analysis?

- Yes, pT resolutions for the typical phase space in this analysis is 1%.
- Yes, we derived the azimuthal angle resolution (<0.7 mrad) in this analysi, see Fig. 5 in AN v7.

- line 79: what is defined to be a "valid primary vertex" for this analysis? is one track coming from the IP enough?

- This is a standard definition for HIN analysis, we have one reference (ref 33) for it. “valid primary vertex” means “not fake vertex” && |Vz| <= 25 cm && |Vr| <= 2 cm (Vr is transverse radius of vertex) && tracksSize >= 2 (here, track is general track). In this analysis, we require exactly 2 tracks.

- line 109 Geant4 is written in the wrong format, for what we usually do in CMS papers. ps: we use: \new-command{\GEANTfour} {{\textsc{Geant4}}\xspace}

- Done

- line 126: Does the absence of a valid track suppress all other backgrounds apart from EMD events? How do we have a control on that?

- Besides the absence of track and valid vertex, we also applied HF veto. Those requirements asure to suppress all other backgrounds. We cannot image that the other process(es) produces no track and no vertex but forward neutrons.

- check line 141: So if I understand properly we expect more multiple photon interactions at smaller 'b' values, is that the effect of these HO effects on these two different neutron classes here?

- Yes, in smaller ‘b’ values, the contribution of multiple photon interactions or photo radiation from produced leptons are expected to be larger, as shown in the diagrams to your aforementioned comments

- line 148: I understand these formulae are empirical but they look rather complicated for what does not seem to be very complicated distributions in Figure 1. It would be good to motivate these formulas a bit, i.e. are the fit parameters related to some physics quantities? Or where do these formulae come from?

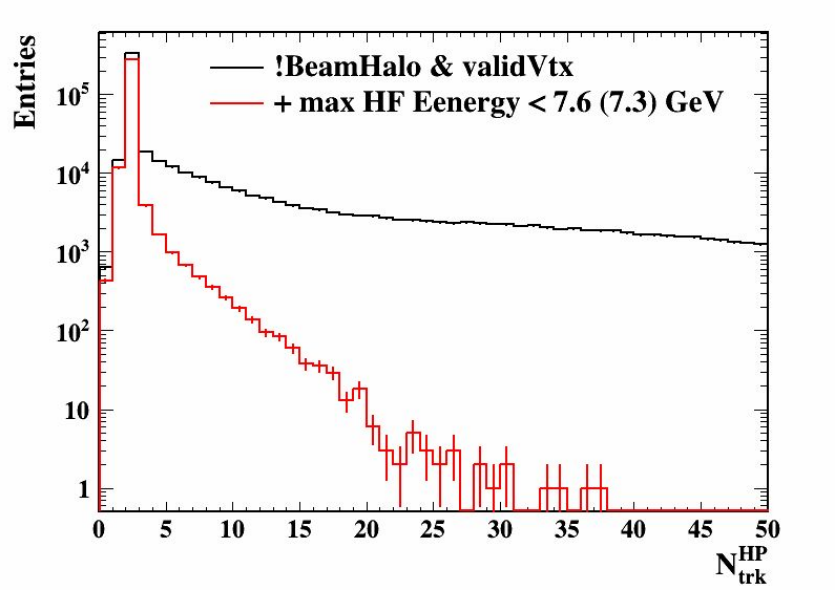
- The core function only adds one correction term (c_3 \times alpha^{0.25}) to the commonly used exponential function to describe the destructive structure at an extremely low acoplanarity regime (e.g. < 5 * 10^{-4}).
- The tail function was used by H1 experiments (EPJC 73 (2013) 2466) to describe the distribution from photoproduced Jpsi with nucleon dissociation.

- line 150: "..given the limited number of events" This comment leads to the question: how many events do we actually have in the bins? Fig 1 does not show that (Figure 2 gives some idea on the relative population between the different classes). I think we could say something on the used event numbers in the paper (eg here) ..

- After efficiency, pileup, neutron contamination corrections, the yields of 0n0n are 65768 counts while the yields of 1n1n are just 506 counts. Because we report the luminosity number, and will publish all the data points in HepData, we would like to keep as it is.

- line 158: If we allow for more muon candidates, as this selection for hadronic events suggests, what fraction of the hadronic events do we actually select/see with that? Do hadronic events have typically 3 muons or more? (maybe my understanding of a hadronic event is incorrect, and to that end it would be useful to show a diagram of a typical hadronic event, perhaps next to a typical signal event as requested already in the general comments above.)

- It doesn't necessarily introduce more muons, it could just include more charged hadron tracks. The red histogram in the figure below, shows the N^{HP}_{trk} distributions after applying HF veto selection. The fraction of N^{HP}_{trk} > 2 is ~2%, which means the maximum contribution from hadronic collisions with b<2R_{A} is < 2%. This is because dijet events in UPC also could produce more than two tracks. What we exactly did here is change the event selection criteria to include other processes including hadronic events.



- line 184: It looks a bit weird to try to fit a straight line through the points of figure 2 which I think nobody would consider to be points lying on a straight line :) But I understand you want to use a metric to be able to talk about a larger than a 5-sigma effect.. I just wonder if this is the best we can do for that. Maybe it is just they way we word it in the text that I found weird.

- This constant fit is recommended by the statistics committee
- We cannot perform a linear model fit, because the X-axis is an event category which is not a physics quantity like impact parameter. However, we can definitely

perform a constant fit to check the chi2/ndf and p-value to estimate the significance of deviation from constant.

- check 190: no brackets needed around <M_mumu>
  - Removed

good luck
Albert

CMS-HIN-19-014-002-COMMENT-003

Institutional Review Comments on HIN-19-014 from Eotvos University (ELTE), Budapest

TYPE A COMMENTS:

Abstract: we would prefer to remove the word "structure"
- Done

L1: we think "Lorentz-boosted" in this case needs a dash
- Done

L4: "so-called" -> ", these are the so-called" (verb seems to be missing)
- CCLE confirmed “so-called” is fine.

L8: "in the nucleon or nucleus" -> "in a nucleon or the nucleus" (?)
- CCLE confirmed “in the nucleon or nucleus” is fine.

L55 in the CMS -> in the CMS experiment
- Changed to “with the CMS experiment”

L115: "and pTmu" -> "and at pTmu"
- Done

L126: "thresholds as the" -> "thresholds as for the"
- Done

L130: 11.0% -> 11% (if you give a value to 3 digits, it is not any more just "about", it's too precise)
- Done

Fig 1: remove a, b, c,.... they are not used.
- Done

L168: less than 4.0% -> less than 4% (since less than 4% means exactly the same as less than 4.0% and the same as less than 4.00000000%).
- Done

L169: quadrature sum -> quadratic sum

- Done

L191: "also" may not be needed.

- Removed

TYPE B COMMENTS:

Title: Shorten title to “Dimuon acoplanarity in ultra-peripheral PbPb collisions at sNN= 5.02 TeV”, target journal is PRL which is for a more general audience. Also a bit obscure and tedious if it stays with the neutron multiplicity dependence. Why is the acoplanarity as a function of fwd n multiplicity interesting to measure? In the above shortened title the "UPC" is more prominent and easier to recognize and get the paper’s topic quickly for a general reader.

- The authors would like to keep the original title because forward neutron multiplicity, classifying the impact parameter range in UPC, is the whole point of this paper. Otherwise, a general reader cannot recognize the importance of this measurement at the first glance because dimuon acoplanarity measurement in UPC is trivial.

Abstract: "transverse momentum and energy of photons emitted from relativistic ions have impact parameter dependence" The photon emission cannot have impact parameter dependence (it is related just to the investigated ion itself, not to the other one), probably this sentence would need rewording.

- No, we do not mean the photon emission has impact parameter dependence. What we emphasize is the kinematic of emitted photons as a function of impact parameter (pT(***b***) or energy(***b***)) is not constant, especially for the photon pT(***b***), which is treated as constant in previous measurements and model calculation based on EPA. This paper first experimentally demonstrates the emitter photon pT has strong impact parameter dependence.

Abstract: it is not clear from what is written here, how this could serve as a baseline for medium effects, where the muons come from much more central collisions, and from many different (strong interaction-related) sources and decays, not only gamma-gamma....

- No, people decoupled the photon-photon produced dimuons from other sources in central collisions [PRL 121 (2018) 132301, PRL 121 (2018) 212301]. The dimuon from photon-photon scattering in central collisions might be modified by the medium, this study can provide a clean baseline to quantify the medium effect(s)

L48-49: "correlations of muon pairs, or acoplanarity" -> "correlations of muon pairs, quantified by the acoplanarity"

- Done

L49-50: Acoplanarity is probably defined in such a way that Phi+ - Phi- is always smaller than pi (i.e. alpha>0); this could be added here somewhere

- This formula is widely used in LHC UPC community. DeltaPhi means the azimuthal angle difference in transverse direction, which is always <= pi

L58: Would be probably good to elaborate more on why alpha is a better observable than pT - how does the alpha resolution compare to pT resolution (angle resolution is mentioned on L68)? Also, what does "less of an effect" mean here?

- Yes, the pT and alpha resolution can be found in detector paragraph
- Rephrased this sentence to avoid confusion

L76: There is no explicit selection on the muon pT. This is a bit confusing as L95 states that pT_mu > 3.5 GeV. We guess you refer to no explicit requirement on the muon pT at the trigger level, but this should be clarified in the text.

- Done

L76: Maybe it would be beneficial to note the pT dependence of muon acceptance (c.f. "There is no explicit selection on the muon pT")

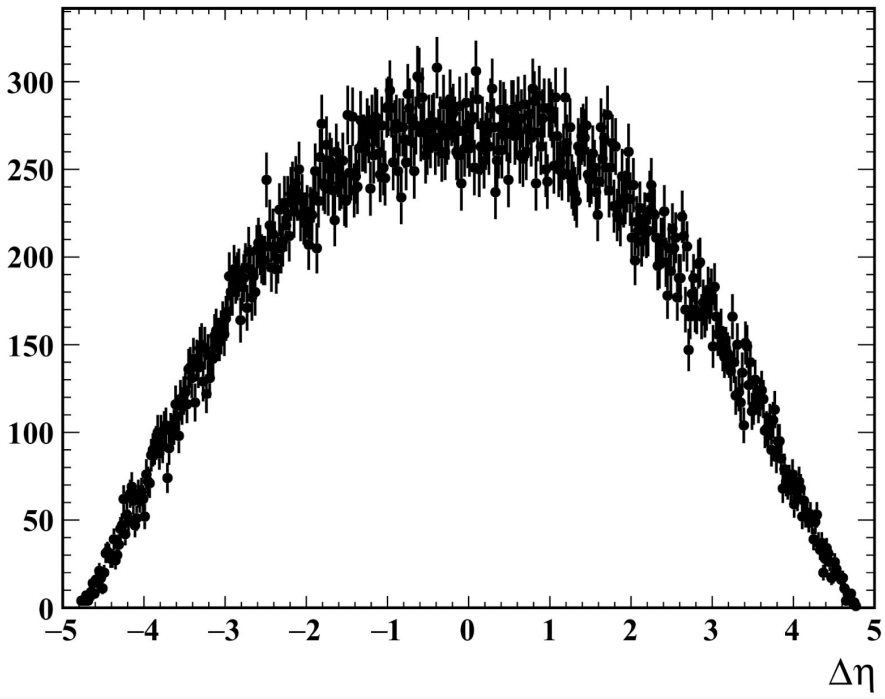
- We prefer to keep the current sentence

L76: The HF energy veto is probably done online, could be clarified here to remove ambiguity with L82

- Online HF veto thresholds are much looser than offline

L78: do you have any event selection criteria to get rid of cosmic muons? We did a similar ZDC study for the PbPb Light-by-light analysis, studying gamma-gamma -> mu+mu- and gamma-gamma -> e+e- processes and found that there might be a substantial number of cosmic muons, detected as a muon pair when going through the detector. We get rid of them by applying a cut on dxy. Do you use such a cut? If one of the existing cuts also remove this contribution (like the selection criteria used to reject beam-gas collisions and beam scraping events), then it might be a good idea to mention that events with cosmic muons are also rejected.

- 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 contributions, we should see a spike around 0 in DeltaEta distributions. The DeltaEta distribution of our data, as shown below, is smooth, which demonstrates the contribution of cosmic muons is negligible.



L79 "Events are required to have a valid primary interaction vertex" this is undefined for the reader. You can define 'valid' any way you want, and the reader does not know what is required. If you have only two muons, is that already a valid vertex? What is the chance that two muons, that come from the same IP, will be rejected by this requirement? How many fakes do you have (valid vertex but lacking two particles)?

- We cannot put such details in this letter. This is a standard definition for HIN analysis, we have one reference (ref 33) for it. “valid primary vertex” means “not fake vertex” && |Vz| <= 25 cm && |Vr| <= 2 cm (Vr is transverse radius of vertex) && tracksSize >= 2 (here, track is general track). In this analysis, we require exactly 2 tracks.

L82: Why don’t you use vetos in the ECAL and HCAL to get a cleaner sample?

- The HFveto and N^{HP} == 2 already do a great job, we only have 2 same-sign pairs compared to 87066 correct-sign pairs using current selection

L84: what is meant by muon candidates? How does a PRL reader know that? what do you require besides them being charged tracks in the tracker? Do you loosen usual high-pT muon selections for the sake of these events, with mostly low pT muons?

- Removed candidate(s) in this paper draft except in L75 of draft v10
- We use the standard “soft muon” recommended by muon POG and have a reference in L97 of draft v10

L95: Would be good to explain why exactly the 3.5 GeV pT cut was used, where this value comes from.

- This number is driven by detector acceptance. To make correction and analysis easier, we use unified pT cut instead of an eta dependent pT cut.

L96: is this the same as "muon candidates" at line 84? or stricter selection?

- Same selection, removed “candidates”

L97: "distribution"... of which quantity?

- It includes all the quantities discussed in this paper

L98-99: There is no further discussion on the background in the paper (and no further mention of the SS sample). What do you know about the inclusive background in the OS sample: mu+mu- + some unreconstructed particles?

- We only have 2 same-sign pairs compared to 87066 correct-sign pairs, our distribution is almost background free
- For the OS sample: In UPC, photon interactions either produce vector mesons or dileptons. Other processes, like scalar meson and di-hadrons are many orders of magnitude lower than the aforementioned two processes. We believe there is no other inclusive background you referred to.

L99: why is SS a good estimate of background? For example, a mu+ and mu- and a pi+ in the same event is background. Let's say this happens very often. But say, mu- mu- events happen very rarely. Why would one estimate the other?

- Like we explained in your last comment, the OS distribution is almost background free and SS mainly accounts for the combinatorial background from possible hadronic collisions. The example you gave does not exist in this analysis by our exclusive event selections.

L100: If there can be maximally two tracks, and there have to be two muons, and the triggering relies on having one muon, then how important is this requirement of "must contain at least one muon candidate matched to a triggered muon"? It would be good to clarify this here.

- We discuss the trigger efficiency correction in next paragraph

L101: Some explanation on the choice of dimuon invariant mass range would be beneficial for the reader.

- The next two lines serve this purpose

L102 are you not bothered by Y? Why not increase the lower limit from 8 GeV to 10?

- 1) The upsilon yield fraction is tiny (~0.6%) in this analysis. 2) To avoid Y, we need to increase the low limit to 11 GeV. 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).

L105: STARlight is produced with no restrictions on Coulomb breakup. However, STARlight has a pretty advanced method to model the breakup of nuclei, it would be nice to compare STARlight predictions with the results of this measurement. (Though

STARlight uses slightly different event classes, like XnXn, Xn0n+0nXn+XnXn and Xn0n+0nXn, where X >= 1.) Later in the paper it is vaguely suggested that the measured quantity <alpha_core> does not depend on the Coulomb breakup in STARlight, but this should be clarified then.

- We explicitly point out STARlight doesn't consider differential impact parameter dependence of initial photon pT in LL108-109 of draft v10, which will give us the constant <alpha_core> for different impact parameter regions.

L106: What do you mean by leading order and higher order gamma-gamma processes? The tail of the acoplanarity distribution is usually considered as dominated by inclusive background.

- Please see the diagrams in AN v7 (Fig. 40). The leading order means two photon produce two leptons and higher order process include soft photon radiations off the produced lepton and scattering of multiple photons
- What kind of inclusive background you refer to？We do not expect inclusive background in our analysis

L121: "may contain concurrent EMD events": probably it would be even more clear if "(in-time pileup)" or similar would be added. Or simultaneous interactions. What is the average inelastic hadronic pileup value in this dataset? It would be nice to mention just for info.

- We would like to keep the original sentence which is clear. The inelastic pileup probability with photon interaction is ~0.006

L125: "No valid collision vertex ..." -> This means an offline veto on valid vertex and any tracks within |eta|<2.4? Would be good to clarify.

- Yeah

L129: A simple matrix inversion does the job, and no Bayesian unfolding is needed here?

- No

L130: "About 11% ..." -> "From this analysis, it can be estimated that about 11% ..."

- Changed to "In this analysis, about"

L132-133: "pairs in PbPb collisions for ... within the CMS acceptance (pT ...)" -> "pairs in PbPb collisions within the CMS acceptance (pT ...) for ..."
(bringing the "within acceptance" forward in the sentence may make this long statement more clear)

- Done

L134 normalized...: why don't we measure xsection as well?

- 1) The lack of xsection doesn't affect the physics message in this paper 2) We don't have an official T&P efficiency correction factor but we demonstrate the T&P correction (private T&P nominal curve) doesn't affect the reported results (sensitive to shape) in this analysis.

L138 if you have such a photon-radiation, don't you lose efficiency (and have a bias) if these photons convert in the tracker and look like a third track?

- The radiated photon is super soft, barley bending the muon track

Fig 1 caption: alpha spectra -> acoplanarity distributions (if someone just reads the captions, not the paper, alpha will be undefined for them).

- Done

Fig 1: why is there no tail for 1n1n? What is the physics reason? If not enough statistics, this is still very misleading (especially with the tail fit function)!

- The absence of the tail for 1n1n class should be caused by the low statistics. The core and tail functions in this paper are empirical, so we report the acoplanarity spectra to the community just in case our empirical LO and HO models are not perfect. In this sense, we do not consider the systematic uncertainty related to the core and tail functions which is artificial. With respect to the trust of 1n1n tail function selection, we barely have other choices because of the low statistics. However, the systematic uncertainty of 1n1n is somewhat overestimated, compared to that in other neutron multiplicity classes, because we cannot fully decouple systematic uncertainty from large statistical uncertainty.

Fig 1: how does this fig look like for SS, and for inclusive bkgd?

- There are only 2 SS pairs in this analysis

L139-144: It is not mentioned in the text that the 1n1n case has an extremely short tail, but of course this finding is very much limited by statistics. Nevertheless, it may be worth pointing this out.

- We pointed it out

Eq (2): Could you give any justification and/or reference why these certain formulas are used to fit the acoplanarity distributions?

- The functions are empirical, unfortunately, we do not have good justification.
- The core function only adds one correction term (c_3 \times alpha^{0.25}) to the commonly used exponential function to describe the destructive structure at an extremely low acoplanarity regime. The tail function was used by H1 experiments (EPJC 73 (2013) 2466) to describe the distribution from photoproduced Jpsi with nucleon dissociation. Nevertheless, we report the acoplanarity spectra to the community just in case our empirical LO and HO models are not perfect.

L152: It could be explained here why the average core acoplanarity is discussed here, i.e. why the focus is on leading order gamma-gamma scattering is considered more important. So, why is only the core important? and if that is the case, why do you plot Fig 1 in a way that the core is barely visible, while the tail is pronounced and takes most of the space on the figure?

- We report the acoplanarity spectra to the community just in case our empirical LO and HO models are not perfect. All the data point will go to HepData

L157 what do you mean by removing this requirement? Do you allow a third muon, or you allow any number of any type of particles besides the two muons?

- We allow any type of particle besides the two muons

L159: it is not the "leading HF energy deposit" that is tightened, but some selection cut!

- Yeah, this is what we meant.

L164: Assuming "photoproduced Y" means Upsilon mesons, why only these were considered, and not other quarkonia/VM? And why not increase the mass threshold above the Y mass?

- In 8<M<60 GeV, we only have contamination from Y vector meson. Please see our reply to your L102 comment

L168: "less than 4.0%": this varies with the event class? Probably it does, since later a quadratic sum of 1.3-5.1% is mentioned. Mentioning only the higher limit here and in the previously mentioned cases is a bit confusing.

- Yeah, it varies with event class. Changed to be “less than 5.1%”

L170: <Mmumu> appears here quite suddenly; would be good to somehow introduce it, as this has much-much less details than <alpha core>.

- The <Mmumu> results are not so important as alpha results

L171: Given the Y mass of 9.5 GeV, the range 9-11 GeV may need some explanation.

- The mass window takes into account for all three Y states and the detector resolution

L171: can you include a figure into the paper that shows the dimuon mass distribution?

- We have this figure in AN and we do not have space for the mass distribution

L179: Does this value of 1.348e-3 have an uncertainty?

- No

L179: By saying “constant” you mean that alpha is the same for all neutron multiplicity classes?

- Yes

Fig 2 caption: I would suggest to spell out the meaning of mean_alpha_core here.

Fig 2: this alpha-core result is not much larger than the experimental resolution. Is this unfolded?

- No, 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).

L181: It may be beneficial to show this 1.227e-3 value in Fig. 2(a), as a line with uncertainty bands, or as another bin in the horizontal axis. Also, instead of (or in addition to) "9.0% lower", the number of standard deviations could be more useful.

- We tried to do so at the very beginning but it will be very misleading. We finally decided to spell it in the text

L184 what sense does it make to fit a constant to something that is a value measured in categories, and you could make as many of them as you want in principle? We would suggest removing this.

- This was suggested by statistical committee

L202-203: Is "strong multiplicity dependence" referring to the same observation as "significant broadening", or this is something else? If the same, it would be good to connect the two sentences.

- Yes, they refer to the same observation. We removed the first sentence.

L210, 212 "many aspects" is repeated and gives the impression that we do not really know what those aspects are.

- Removed “and make fundamental impacts in many aspects. For instance,”, replaced “future” with “Future”

CMS-HIN-19-014-002-COMMENT-004

Institutional Review Committee: IOANNINA (Nikos Manthos)
Observation of forward neutron multiplicity dependence of dimuon acoplanarity in ultra-peripheral PbPb collisions at sqrt{s_NN}=5.02 TeV

TYPE A

L84 the “exactly” is not necessary and the “contain” can be replaced by “include”

- We mean this event only have two muons and nothing else

L88 the “with” is better to be replaced by “according to the”

- Removed “with energy thresholds” and added corresponding information to last sentence

L105 replace “with” by “by”

- Done

L106 replace “for” with “of”

- Done

L106, 109 remove “in STARlight”, replace “only” with “Only”

- Remove the L106 one and keep the 109 one

L112-113 remove the symbol x in the forms

- If possible, we would like to keep them

L126 replace “as” with “as in”

- Done

L137 replace “predominate” with “dominate”

- Done

Figure 1 caption replace “from” with “of”, remove “within the CMS acceptance”
Form (2) remove symbol x

- We would like to keep the original ones for these two places

TYPE B
L74, L92 I think that “study” is better than “measurement”

- Done

L78 the “beam scraping events” is not self-explained, maybe is better to be replaced by “beam distorted events”

- This term is widely used in HIN papers

L116-117 better justification.

- <0.1%

L143 what is the supplemental material?

- In https://gitlab.cern.ch/tdr/notes/HIN-19-014/-/tree/master/figures/pas_supp, but we added an official appendix for this paper

L209-210 remove “and make fundamental impacts in many aspects. For instance,”, replace “future” with “Future”

- Done

CMS-HIN-19-014-002-COMMENT-005

Institutional Review: Catholic University of America

We congratulate the authors on a well written paper and interesting analysis.

Type A comments:
Line 47: you define LHC but not CMS

- Conventionally, there is no definition for CMS. We can change if you insist

Line 204: Use acoplanarity instead of back to back azimuthal corrections. Acoplanarity was already defined earlier in the paper and as acoplanarity appears in the title it’s nice to have it in the conclusion.

- We prefer to keep it to emphasize this distinctive back-to-back feature

Type B: comments
General: We understand that you have limited pages for a PRL but it seems that the authors spend a lot of time on reviewing the analysis motivation and not the analysis itself. We would suggest eliminating some references and removing details from the

introduction in favor of including more details for the analysis itself. Making the analysis central to the paper will make it a stronger paper.

- We optimized this draft according to your nice suggestion

Line 85 and 101: you have eta for 2.4 and 2.5 why the mismatch?

- Changed to 2.4

Line 101: you switch from eta to y. Doesn't this cause a problem for triggers, tracker coverage, muon detector coverage etc?

- No, eta is for daughter and y is for pair

Line 143: please add a reference for the supplemental materials

- We added an appendix for this paper

CMS-HIN-19-014-002-COMMENT-006

Dear Authors and the ARC Members,
Congratulations on an interesting paper of the exclusive dimuon production in lead-lead collision and the dependence of the muon psi coplanarity on the forward neutron multiplicity! The analysis appears to have been done carefully, and the paper is generally well written, but there is a lot of confusing statements in the paper, which do not help clarity. You should keep in mind that PRL has a broad audience in mind and therefore the paper should be understandable in general terms to, e.g., your CME colleague.

Please, find our detailed comments below, split in the Type B and Type A categories.
Type B Comments:
- General: the term acoplanarity you use throughout the paper is not the best wording choice. While acoplanarity sometimes is used to denote the azimuthal opening angle between the two outgoing particles in a 2â†'2 process (at leading order), this is not an infrared-safe definition, and the one that is, based on the momentum tensor of the outgoing particles would give 0 acoplanarity for events with two outgoing particles. Why not switch to the "azimuthal correlation" instead of the "acoplanarty", as we do for our dijet angular distribution papers, which probes a similar phenomenon?

- Acoplanarity has been quite widely used, esp for this subject, and clearly defined. Therefore, we do not think there is any issue or ambiguity with what we measure.

- General: the paper continuously confuses photon energy (and the related transverse momentum) with the diphoton transverse momentum, which are not generally related. Both photons could be very energetic, and yet produced back-to-back in the lab frame,

implying very little transverse momentum of the photon pair. Thus, the statement on LL196-199, which implicitly correlates the acoplanarity of the muon pair with the dimuon invariant mass is incorrect; for example the Higgs boson produced at small pT decays into two photons with very large energy each, but fairly small diphoton transverse momentum. This confusion continues throughout the paper, and needs to be resolved, see detailed comments below.

- We may not fully understand the comment. Of course, photon energy and diphoton pT are not related. We didn't find any confusion on this in the paper. LL196-199 discusses the average invariant mass of the dimuon pairs, which is directly related to the initial photon energy. We observed that initial photon energy increases with neutron multiplicity, similar to what is observed for initial photon pT. These are based on two independent measurements. Additionally, for a given diphoton pT, photon energy does have a correlation with acoplanarity.

- General: the dimuon invariant mass region, 8 to 60 GeV, appears to be completely randomly chosen and is not really justified in the paper. The justification given on LL101-103 does not appear to be correct. First of all, you can't see charmonium resonances with acoplanarity close to zero because of the trigger requirements;

- In fact we can see clearly charmonium, at forward rapidity if lowing the muon pT cut. You are right for midrapidity.

second, you don't have any sensitivity to the Z boson photo- or hadro-produced, so the justification of the 60 GeV cutoff doesn't seem to apply.

- This is probably correct in practice but in principle there could be Zs, esp. With higher luminosity data in the future. It is better to avoid that ambiguity. There are no entries beyond 60 GeV anyway so practically it doesn't make any difference to the analysis.

Presumably, the lower limit has something to do with the trigger, which should allow two back-to-back muons in order not to affect the acoplanarity. However, given that you use a single-muon trigger without minimum pT requirement, presumably 8 GeV mass threshold (and the 3.5 GeV offline muon pT threshold) appear to be quite conservative, and you could have lowered the search range to, say 6 GeV. So, why 8 Get was chosen?

- The cut is not at all conservative. Efficiency starts to drop significantly below 8 GeV. It is certainly our interest to include as much m_inv space as possible.

More importantly, it's not at all clear from the paper what fraction of events actually correspond to the Y(nS) contribution. While Y(nS) would not be produced in quasi-real

gamma-gamma collisions because of the Landau--Yang theorem, you still mention the contamination from Y(nS) events, which presumably could be produced hadronically and thus are an irreducible background to the exclusive production. This part is completely not clear from the paper, and needs to be clearly explained. Looking at the AN2019/268 describing the analysis, it's clear that the Y(nS) contamination is not negligible (Fig. 14, lower right). [As a side note, Fig. 14, lower left, also clearly show that the Z boson contribution is completely negligible in this analysis.]

- Y(nS) can be produced by photon-pomeron interactions. It is indeed an irreducible background. Its cross section is not negligible (0.6%). However, Its effect on the acoplanarity is estimated and treated as systematic uncertainty, as described at L163.

The other thing that is clear is that there is a significant turn-on near the lowest mass threshold, presumably from the offline selection. The paper doesn't say anything about the turn-on and how the efficiency of the turn-on has been estimated. Presumably, the turn-on would bring an extra systematic uncertainty component, which is not mentioned in the paper either.

- Efficiency, including online and offline, as well as the correction is described in the paragraph L104-117. The systematic uncertainty is negligible as we are not measuring the cross section, as stated in L115-117.

That brings an obvious question: why not start the analysis above the Y meson mass (i.e., at 10 GeV), which would avoid both the turn-on and the contamination problems? While it would reduce the number of usable events, you do not seem to be statistically limited at all at low masses, as is evident from Fig. 2 (lower) of the paper. We believe this would make it a much more clean and robust approach and urge you to consider doing so. In any case, if you don't the above points need to be clearly explained in the paper.

- We would lose statistics significantly if we start at 10 GeV, as the mass distribution quickly falls.

Abstract

- L7: the use of ‘broadened for events producing a larger number of neutrons’ isn’t clear. The use of "broadened for events producing a larger number of neutrons" isn't clear. To say "broadened" suggests that the neutrons actively generate the broadening. How about "broader"? Also, "events producing" is somewhat awkward: 'events' don't 'produce'; it could be "collisions producing" or "events with". Maybe: "... is significantly broader for events with a larger number of neutrons from ..."

- Modified as suggested.

- LL8-9: "This observation demonstrates ..." Does it? Perhaps the statement is too sweeping, for relativistic ions, in general. Isn't the demonstration made in a specific kinematic corner of phase space (ultra-peripheral)? How about "...can have impact parameter dependence."?
    - We think the statement is appropriate. Ultra-peripheral events are our tool to isolate hadronic effects so that we can explicitly study properties of photons emitted from the ions.

- L12: what does "controllable baseline" mean? Maybe say just "provide a baseline for searches" [rather than 'to search']
    - Removed controllable.

Text
- L9: provide a citation
    - Citation would be Ref. 6-8.
- LL1-2: technically speaking, the charge is Ze, not Z; thus please refer to Z as an "atomic number", not charge.
    - Removed Z as per other comments.
- LL11-12: "other higher excited states" - higher than what? Suggest dropping "higher", as you introduce it in a more obvious way just two sentences later.
    - Done.
- L21: momentum (pT) of order w/rL, where ... and rL is ... [Note that subscript "L" should be in Roman; also you should explain that you refer to the Lorentz factor in the lab frame.]
    - Done.
- LL51-54: these two sentences make little sense, as at leading order the scattering system by definition does not have any transverse momentum and the two muons are produced precisely back-to-back; the transverse boost comes from higher-order effects, such as extra gluon radiation off the incoming nuclei. You use the "leading order" in this context in several places, which is wrong. Please, rephrase to fix this problem. [Perhaps you mean leading order in QED, not QCD, but it's not clear at all from the paper.] Moreover, "the total pT of initial photons" is a misleading phrase, as it is not clear that you mean the diphoton pT, and not, say, the scalar sum of the two photon pT's. Please, rephrase as: "the magnitude of the vectorial sum of the transverse momentum vectors of the two photons".
    - This is incorrect. Colliding photons do have a finite pT, which is exactly the subject studied in this paper. There is no issue with “extra gluon radiation off the

incoming nuclei". We agree that "the total pT of initial photons" is misleading and rephrased it.

- LL53-54: what do the angle bracket mean here - presumably mean, but then over what set is the mean being evaluated? Consider elaborating on the nomenclature ('Thus, for a given neutron multiplicity class,'…), or removing the brackets, phrasing the sentence in the context of single events.
  - Rephrased in the context of single events.

- L71: add a standard sentence about the CMS trigger system with a reference to our trigger paper before the ultimate sentence of this paragraph.
  - We would like to keep the current description because the standard text does not work well for our case.

- LL74-77: the description of the trigger is quite confusing. You mentioned the hardware trigger on a single muon; this is at L1. What about the HLT? - Is it just a pass-through of the L1 trigger? Furthermore, is the HF veto requirement part of the L1 tagger, HLT, or offline? Please, expand on the trigger description in the paper text.
  - All trigger selections are L1 based, which is described as hardware trigger. HLT is simply passed through. There is no need to introduce the L1 and HLT here for readers not familiar with CMS.

- L79: a definition of what is meant by a 'valid primary interaction vertex' might be needed
  - Added "consisting of at least two reconstructed tracks."

- L82: is this an offline requirement on top of the trigger requirement, or this is an expansion of the sentence on LL76-77?
  - As said in L77, this is an offline requirement on top of the hardware trigger requirement.

- LL85-87: how do you know that what you measure in the ZDC are neutrons? Are the protons swept away by the magnetic field?
  - Yes. Protons will not make into the ZDC.

- LL97-98: what is the background due to? Presumably combinatorics, but it would be better to state it explicitly.
  - Added "combinatorial background"

- LL104-9 (and 179-80): It would be useful to include more details about the STARlight simulation, and what features are and are not expected to be modeled. How is the initial photon pT modeled (aside from not including impact parameter dependence)? It is mentioned that STARlight prediction is constant across neutron multiplicity bins. Is the generator expected to model this behavior?
    - Yes, STARLight is a leading order QED model without considering any impact parameter dependence of photon pT. Therefore, this behavior is expected.

- LL106-107: how do you simulate the transverse boost of the gamma-gamma system, if the simulation is done at leading order, which is a 2->2 scattering? See also the comment on LL51-54.
    - As explained above, colliding photons have a finite initial pT. This has nothing to do with whether it is leading order or not. It is typically inversely proportional to the size of the emitting source, from uncertainty principle. What's missing in STARLight is the impact parameter dependence of photon pT.

- LL110-112: is the 'strong kinematic correlation' just a straightforward calculation or is there some dependence on the theory? It may help the reader to explain briefly.
    - We deleted "To account for the strong kinematic correlation between \mu^{+} and \mu^{-} produced from \gamma\gamma scattering,". Strong correlation is one of the distinctive features of leading order photon-photon scattering.

- LL115-117: is that really so, even given quite a sizable mass turn-on you see, which can't cancel in the mass observable?
    - Yes. We are not sure what you mean by "sizable". We chose a mass region to avoid the fast turn-on part.

- Figure 1:
- the line representing the tail can not really be seen in the plots. Please, either improve the plotting or drop this legend.
    - Fixed.
- delete the labels (a), (b). It is CMS style not to label figure this way. See Guidelines for Authors. In any case, these labels are not used either in the caption or the text.
    - Done
- separate out the figure so it includes 6 plots, each with both axes with labels. See example plots. They can be referred to as 'upper left, upper middle, upper right, …'
    - We think the plot is better with shared axes.
- LL142-143: here you refer to the supplemental material, which does not exist in the paper. You should either include it in the text of the paper (and go through a second

short CWR, so that people could review it) or do not mention it at all. If you are going to provide supplemental material, it would be a very good idea to include the dimuon mass spectrum there. It's odd that the paper that talks about this spectrum never actually shows what it looks like.

- Supplemental materials are added.

- LL146-153: This paragraph presents the core and tail fits to the neutron multiplicity dependence shown in Fig. 1. There should be a reference to the figure in the paragraph.

- Done.

- L154: did you consider systematic uncertainties associated with the choice of the functional form for the empirical core and tail functions, as you do for the dimuon mass fits? For example, the core exponent (-alpha/c2 + c3 * alpha^0.25) seems rather specific. Is it governed by some a priori principle, or could, e.g., the 0.25 be allowed to float in the fit?

- We do not as that is only one choice of fit function. We tried other functions and even directly integrated the alpha distribution, and the main conclusion does not change. As we present the full distribution, anyone can fit it with their favorite function. And we hope that full theoretical calculations will be motivated to describe the full distributions we presented.

- LL163-166: why do you only include Y meson photoproduction, and not normal hadroproduction that passes the veto. The cross section of the former is much larger, and even in the photoproduction the second ion breaks up, so it's not an exclusive process and would have to pass the HF veto.

- Hadronic contamination has been estimated and found to be negligible. Keep in mind that our selection requires two muons and nothing else.

Also, why do you limit yourself to Y(1S) and not the three close-by Y(nS) resonances, with n=1..3?

- Y(2S) and Y(3S) are completely negligible. Their peaks are not visible at all.

- Figure 2:

- Mention the STARlight line in the figure caption.

- We prefer not to because of length limit of PRL

- please remove the labels (a), (b), and refer to the plots as “upper” and “lower”

- Done

- separate out the two plots with a space between them as shown in the CMS example plots, each with their own axeâ€™s labels

- perhaps the figure would be better if the two plots were side-by-side. They would be referred as to ‘left’ and ‘right’

- We think shared axes are better.

- caption, line 1: ‘a’ > ‘upper’, ‘b’ > ‘lower’

- Done

- in the paper text : line 177 ‘upper’, line 180: ‘lower’, line 190: ‘lower’

- Done

- L179: Is there any uncertainty on the <\alpha^(core)> predicted by STARlight, e.g., from scale factors or the fitting procedure?

- Not really. STARLight is a simple leading order generator. We’ve generated sufficient events that statistical uncertainty is negligible.

- LL184-187: again, here you seem to be mixing the individual photon pT with the diphoton system pT, which is what is reflected in the dimuon acoplanarity. Please, rephrase to make this point clear.

- We do not think the phrasing here is confusing. Diphoton pT, individual photon pT, and dimuon pT are closely related, all reflected in the dimuon acoplanarity. We are open to specific suggestions on how to rephrase.

- LL198-199: here again, there is no connection between the acoplanarity and the photon pT, which makes the entire sentence scientifically incorrect.

- As explained above, there is a direct connection between acoplanarity and photon pT. This sentence is about photon energy.

- L208: might benefit from re-iterating the relation between b and acoplanarity (mentioned L17) since the dependence is not obvious from the figures. In general, it would be nice to have further discussion on how the coplanarity dependence on b may or may not be different than for hard scatter.

- We are not sure we understand this comment. There is no (QCD, if it is what’s referred to) hard scatter involved in this study.

- LL207-208: again you are mixing the photon pT with the diphoton pT here.

- See responses above.

- LL209-210: we find the sentence too self-congratulatory and vain: whether this measurement makes fundamental impacts in many aspects is not for us to judge, but for other people to say. Please, remove this clause.

- Rephrased.

- LL211-212: the sentence is quite awkward: first we should not tell other people what to do; second there is unnecessary grandeur in the idea of comparing a specific model "against many aspects of high-energy nuclear physics". Given that the sentence is not very meaningful, you should either tone it down or remove it completely, which won't affect the message carried by the paper at all.

- Rephrased.

Type A Comments:

General: a comma should be added before ‘and’ in several places, when the conjunction joins two independent clauses. For example, L79-80, 93, 119, 158.

Title: in utra-peripheral lead-lead collisions;

Abstract:

L1: of the forward

- Done

L2: in lead-lead collisions;

- Done

L3: between the two muons;

- Done

L6: correlation structure expected from leading order photon-photon scattering;

- Done

L8: to interactions with a smaller impact parameter.

- Done.

L12: of a quark-gluon plasma

- Done.

P1:

L1: The Lorentz-boosted electromagnetic;

- Done.

L2: with the flux;

- Done.

L4: namely in so-called

- Done.

L4: (UPCs).

- Done.

L6: are fundamental processes that can be used

- Done.

L9: at the CERN LHC energies.

- Done.

L11: photon absorption

- Done.

LL11-12: you don't really need to introduce the acronym GDR, as you only use it once more in the paper.

- Done

L14: parent ions, therefore approximately retaining

- Done

L17: remove ‘commonly’

- Done

LL20-21: The momentum … is predominantly along the beam direction and the transverse momentum on the scale

- Done

L24: in the azimuthal angle (\phi).

- Done

LL25-26: in the UPCs [14-23]. [The "C" in "UPC" already stands for "collisions"!]

- Done

L26: remove ‘also’

- We think keeping it better as it was not expected.

LL26-27: at the BNL RHIC and at the LHC.

- Done

L28: the quark-gluon

- Done

L29: and the azimuthal angular correlations [25];

- Done

L32: remove the comma before the ‘or’

- Done

LL34-36: a QED calculation [29] indicates ... initial-state photons, as the ... hadronic collisions. Another;

- Done

L40: Establishing a possible … dependence of the initial …

- Done

LL40-41: of the initial photon energy;

L44: This Letter reports;

- Done

P2:

LL49-51: awkward introduction of a new variable. Consider making a new paragraph with a transition explaining the physical significance of the azimuthal correlations before introducing the equation.

LL52,53-54: leading order scattering(s);

- The first one should be singular and the second one should be plural

L55: precision in CMS;

- Modified according to comment from other colleague

L55 : ‘less of an effect’ is a bit confusing (what is the effect on?). Consider a more direct phrasing such as staging that azimuthal angle is preferred over pT because of its superior resolution.

- Changed to “The azimuthal angle is used instead of pT because of its superior resolution”

L60: either add ‘sub detectors, including…’ or ‘sub detector: a silicon…’

- Done

LL64-65: resistive-plate chambers.

- Done

L67: The CMS experiment has extensive [can't start a sentence with an acronym!];

- Done

L74: remove ‘fast’

- Done

L79: remove ‘valid’

- Done

L80: must be compatible

- Done

L80: the cluster shapes

- Done

LL82-83: below 7.3 and 7.6 GeV on the positive and negative;

- We think the original sentence is fine

L85: no additional tracks

- Done

L86: by the energies

- Done

LL87-88: is divided via energy thresholds into three ... on each side. The corresponding;

- We prefer the old expression

P3:

L96: and muon detectors, using the algorithm for so-called ‘soft muons’ defined in Ref. [31].

- We think the old expression is fine

L97: The opposite-sign (OS);

L99: the same-sign (SS) muons;

- Rephrased

L101: and the rapidity;

- Removed “rapidity”

L102: photo-produced?

- ‘photoproduced’ is a widely used term in UPC physics

L103: pairs and to avoid

- Rephrased

L104: estimated using

- Done

LL105,106,109: \textsc{STARlight};

- Done

L106: break-up of each nucleus.

- Done

LL106-107: leading order gamma-gamma scattering;

- Done

L108: of the initial photon
- Done

L109: \GEANTFour [35].
- Done

LL112-113: subscripts "trig", "reco" should all be in Roman;
- Done

LL115-116: with the efficiency corrections;
- Done

L116: in the final observables;
- Done

L118: single EM dissociation (EMD);
- We think the old express is fine

L120: to the stronger EM fields.
- Done

LL120-121: single-EMD cross section;
- Done

LL122-123: consider changing ‘migrate’ to ‘increase’
- We think “migrate” is fine

LL123-124: by measuring the ZDC energy;
- Done

L124 that require only
- Done

L125: No valid collision vertex or tracks are allowed;
- Done

L126: as for the gamma-gamma -> mu^{+}mu^{-} events;
L126: as imposed on the .. events are applied.
- Done

L128: event being classified in an incorrect
- We think “into” is better

L129: of the pileup effects.
- Done

L130: About 11\% of;
- Done

L131: same as L122-123, consider changing to ‘increase’ instead of ‘migration’
L131: by the EMD pileup.
- Done

L133: CMS detector
- Here we intend to define the CMS acceptance

LL134,135: subscript "s" in Roman;

- Done

LL136-137: leading order gamma-gamma scattering, while in the tail component, higher-order gamma-gamma processes dominate.

- Done

L138: include, e.g., soft-photon radiation off;

- Done

L140: of higher-order gamma-gamma processes;

- Done

L150: component, given the limited number

- Done

L155: from the contamination

- Done

L156: the neutron

- Done

L156: The uncertainty associated with the hadronic

- Done

L157: add space before ‘1.1%’

- Done

L159: consider changing ‘tightened’ for ‘increased’

- Keep “tightened”

P4:

Fig. 1 caption, L1: dependence on the acoplanarity \alpha for gamma-gamma -> mu^{+}mu^{-} events within; L3: The dot-dot-dashed line; L4: add a comma before "while";

- Done

Eq. (2): end th first line with a comma;

- Done

LL150-151: performed using the integral of the function across each bin;

- Done

L156: The uncertainty in the hadronic;

- Done

P5:

LL161,62,64: add a space after ‘<‘

- Done

LL162-163: tight energy requirements, according to;

- Done

L164: photoproduced Y mesons (~ 0.6%);

- Done

L166: Y(1S), with the relative

- Done

LL165,179: \textsc{STARlight};

- Done

L168: distributions, and is found to be

- Done

L169: from a quadratic sum of the uncertainties from the aforementioned sources;

- Rephrased

L170: found to be 1.3--5.2\% in <\alpha_core>.

- Done

L172: in the Y meson mass region.

- We would like to keep the original one

LL173-174: one obtained in a nominal fit to a third-order polynomial.

- We would like to keep the original one

L175: add a comma after ‘1.8%’

- Done

L179: is a constant

- Done

LL179-180: indicated by the dot-dot-dashed line;

- Done

P6:

L180: delete ‘Meanwhile’

- Done

L181: \textsc{STARlight};

- Done

L183: as \sigma is usually reserved for the cross section, suggest not introducing this term and simply stay with "standard deviations" in two places where you use this;

L184: a p-value corresponding to 5.7 standard deviations.

- There is no ambiguity of using \sigma after number

L188: EM effects arising from a hot

- Done

L191: is shown as a function of

- Done

L192: in the mass range 8<M_mumu<60 GeV.

- Done

L194: add comma after ‘events’

- Done

L195: add comma before ‘with a significance’

L196: than five standard deviations.

- Removed this sentence in LL195-196

L200: In summary, the first measurements … as a function of the forward …

- Done

L202: A strong neutron

L203: produced in gamma-gamma scattering;

- Removed the sentence in LL202,203

L204: leading order gamma-gamma -> mu^{+}mu^{-} process;

L207-208: suggest move the phrase ‘heavy ion collisions’ earlier in the sentence so it reads like: ‘demonstrate that in heavy ion collisions the initial photon energy and… interactions have impact parameter dependence.’

- We would like to keep it as is

L210: quark-gluon plasma induced;

- Changed to QGP-induced

Acknowledgements:

Replace "centres" with "centers".

- Copy from the official acknowledgements, and used by other CMS paper

References:

Ref. [2]: it's odd that this STAR paper has not been published in eight months. Are we sure there is nothing wrong with it?

- We do not know but we assume it is still in the journal review process.

Ref. [13]: published in Comput. Phys. Commun. {\bf 253} (2020) 107181; please add the journal and the doi references.

- Done

That's all. Good luck with the remaining editorial work on the paper and timely submission!

CMS-HIN-19-014-002-COMMENT-007

UNIANDES INSTITUTIONAL REVIEW

We congratulate the authors for the interesting physics result obtained. The paper is clear and well written. We add below a few minor comments that we think might contribute to clarity.

Type B comments:

- L54-55: We suggest to include the values of the resolution of the azimuthal angle and dilepton P_T in this sentence. Since this is the first time, the emphasis on the resolutions is made. The values reported on Lines 66-67 can be quoted in lines 54-57.

- We think it is better to keep it in the paragraph which describes the CMS detector.

-L74-76: The sentence: “There is no explicit selection on the muon pT.” is confusing. In order to determine there is a muon, a minimum pT threshold must be taken into consideration. We assume that what you mean is that no extra requirements is applied after muon reconstruction. We suggest to clarify.

- It just means pT>0. We added that “... on the minimum muon pT”.

-L87-88: Please specify the energy thresholds used to determine the neutron multiplicity classes.

- Neutrons are identified from single neutron peaks. The ZDC distribution is now added as supplemental materials to clearly show it.

Type A comments:

- Suggest to change “heavy ion” for “heavy-ion” Last line of abstract, L18, L26, L188, L208

- We typically use heavy ion

-Recommend to change “back-to-back” by “antiparallel”

- We think back-to-back is better and more intuitive.

- L14: “…and therefore approximately retain the…” Suggest rephrasing as: “…and consequently their rapidity remains along the beam direction.”

- We think the original and proposed wording are both OK.

- L23: “..back-to-back..”—> “antiparallel (back-to-back)”

- We think back-to-back is better and more intuitive.

- L32-34 Suggest to reorganize the sentence as: “Final-state EM modifications of lepton pairs in the presence of a QGP medium (e.g, Coulomb rescattering, or deflection by magnetic fields trapped in the QGP) have been proposed ..”

- Done

-L49 “Azimuthal correlations of muon pairs, or acoplanarity (\alpha) defined as “ --> “Azimuthal correlations of muon pairs, or acoplanarity (\alpha), defined as”

- Done

-L51: “\phi^pm represent the azimuthal angles of two muons“ --> “\phi^\pm represents the azimuthal angle of each muon”

- Done

-L53-54 We suggest to rewrite as: “The large mean \alpha (<\alpha) … corresponds to large average p_T (<p_T>)…”

- We removed <>.

- We find a bit confusing L85 where you first mention you select two muons and then mention that no other tracks with |eta| < 2.5 are allowed. One gets the impression that |eta| < 2.5 is also applied for the muons. This is only clear on L95. We suggest to move L95-97 right after L85.

- It should be |eta|<2.4 instead of 2.5. We've corrected. L95-97 describes muon selections while previous text describes event selections.

-L97: "The opposite sign (OS) distribution…" We suggest to rephrase as:
"The selected muon pair is required to have opposite sign (OS) electric charge. To estimate the background, events where the muons have the same charge sign (SS) are used."

- We rephrased as per others' comments.

- L102 -103, "This mass region is selected to suppress the contribution from photoproduced resonances (charmonia and Z bosons) decaying to .." : we suggest to cite a reference to support this sentence.

- We rephrased as per others' comments.

- L111: "the muon reconstruction and trigger efficiencies are applied" --> "the muon reconstruction (\epsilon_{reco}) and trigger (\epsilon_{trig}) efficiencies are applied"

- Done

-L126: "No valid collision vertex and track is allowed to be present in the event" --> "No valid collision vertex and track are allowed to be present in the event"

- Done

-L137-138: "These high-order processes include, for example" --> "These high-order processes include, for example, "

- Done

-L148 : "using a two-component empirical fit function," --> "using a two-component empirical fit function (where ci's and ti's are the fit parameters),

- Done

-169 - 174 : The word "Aforementioned" is used in these two lines, we suggest to change for a synonym in one of them.

- Done

-L170: "second order polynomial" -> "second-degree polynomial"

- We think second order is better

-L191: "…acceptance is also studied as.." --> " acceptance is studied as"

- Done

-L194: "..that in 0n0n events with a significance clearly exceeding 5\sigma" -->: "..that in 0n0n events with a significance exceeding 5\sigma"

- Done

CMS-HIN-19-014-002-COMMENT-008

Dear authors and ARC of HIN-19-014,

This is the institutional review on behalf of the Bologna group. We would like to thank and congratulate you for this concise paper on a new observation. Please find our detailed comments below.
Cheers
Andrea, Elisa and Francesco

General comments
There are certainly space limitations given that this is a letter, but some details that are presently missing should hopefully be added, e.g. about the ZDC usage to select the classes. Also some use could be made of Supplemental material, which is mentioned in the text but not appended to v10.

- Supplemental materials are added.

Type B
L48: It is probably worth mentioning somehow that the apparent lower integrated luminosity with respect to other analyses which are based on the same dataset is due to the limited availability of the ZDC during the data taking.

- At this point of the paper, ZDC has not been introduced yet. It is not uncommon that not all analyses use the same Lint due to detector conditions, triggers used etc. We don't think it will cause any confusion.

L76-77 Perhaps some number should be mentioned here, otherwise one may wonder what it is when reading the detailed description of the offline selection at L81-84.

- The online L1 threshold is given in ADC counts and it is not very easy to directly connect to the absolute energy. However, we do see that it is much looser than the offline selection so we think it is OK to not specify values here.

L85-86 Probably a more detailed explanation of how you identify one or more neutrons in the ZDCs would be welcome.

- ZDC distribution is added as supplemental materials to provide additional information.

L88-89 "multi-Gaussian function fit": w/o seeing the energy distributions it is hard to see what you are doing.

- ZDC distribution is added as supplemental materials to provide additional information.

L90 93--95 % purity for the 1n class implies 86--90 % purity for the 1n1n class, or are there correlations?

- Yes, this is about correct and systematic uncertainties from impurity have been estimated for each class.

L101-103 This sentence contains two separate parts, which naively don't seem to be related. It should probably be split, and the second part should be commented upon. Also the Y(nS) is not cut out, this is done only later in the text, when fitting the mass distribution.

- We rephrased the sentence. It is correct that Y(nS) is not cut out. That's why we say suppress with charmonia and Z in parenthesis. We cannot eliminate Y(nS) directly as it cuts away too many of our signal events.

L118-119 As it is written it is not clear whether the cross section is a result presented here or was obtained in Ref. 38. Therefore at L119:
is measured -> was measured; [38] and is -> [38]: it is

- Done.

L123-131: It is rather obscure how the matrix to compute the EMD pileup effect is obtained starting from the "zero bias" triggered events. And I must admit that the AN does not help that much in describing it either (probably a much more careful reading of the AN than what I did is needed). On the other hand, this is a crucial point in the analysis, and the reader is certainly curious to understand how a prescaled "zero bias" sample can be used for such an estimate. The neutron multiplicities in ZDC are correlated with the actual machine luminosities: how is this taken into account? Or maybe it is exactly such a dependency which is used to estimate the migration? By the way, do you take the instantaneous luminosity of the machine into account to parameterize the migration probability?

- The zero-bias trigger took data at a constant rate with a large pre-scale number, which is not relevant to luminosity but only related to the LHC clock. Therefore the pre-scale number of zero-bias trigger is more or less constant from high luminosity to low luminosity periods. If everything ran perfectly in 2018 HI data taking, the zero-bias trigger sampled luminosity has the same luminosity shape but with a large constant pre-scale number compared to UPC trigger sampled luminosity. Nevertheless, to precisely estimate the luminosity dependent pileup effects in UPC trigger, the zero-bias trigger sampled luminosity is weighted to UPC trigger sampled luminosity for every single luminosity section in each analyzed run to account for the fine luminosity structure difference between UPC and zero-bias triggers.

L130-131 How does this statement compare with the purities stated at L89-90: the migration will increase the number of events in categories with a larger number of neutrons, so why is Xn not affected?

- L130-131 and L89-90 are two separate effects: former due to EM pileup and latter due to finite ZDC energy resolution. Xn is affected but almost negligible as it requires a pileup of two 1n events, which are rare. Nevertheless, the migration effect is fully corrected.

Figure 1. Maybe it is just a visual effect, but the striking difference in the tail of the 1n1n category compared to all the others bangs in the eye. Not knowing the numbers a quantitative understanding is impossible. It seems almost that the 1n1n category has no tail, so no soft photons etc. in this case. In addition about one seventh of the events are not genuine 1n1n but should be migration from the 0n0n category if I interpret the text,

- The integrated yield (L*xSection) 0n0n has 65768 counts while 1n1n only 506 counts after efficiency and EMD pileup correction.

L130-131, correctly: this 15% should have a sizeable tail. What is hard to believe is that there's no comment whatsoever about this effect in the letter. Please explain. The text contains a discussion of the comparison of the 0n0n tail vs the XnXn, somehow the 1n1n, which is intermediate, is excluded from the discussion. What do you expect a referee or reader will conclude?

- As we commented in your last comment, we already remove this 11% electromagnetic dissociation pileup effect, the reported spectra are the true distributions. For the 1n1n, because of the limited statistics as commented in the text, we cannot say much about it.

Figure 1 caption. A box has two sides. Along the horizontal one seems just to be the bin size rather than a systematic uncertainty.

- Done

L143 "supplemental material": this is bizarre to say the least, there's no supplemental material!

- Supplemental materials are added

L143-145 Not clear what you want to say here, there's no attempt of an explanation, and no tentative conclusion.

- Added the discussions

L148 "empirical": is there any motivation behind the choice of the fitting function: this should probably be mentioned.

- As said in the text, this is just an empirical function to separate the core and a long tail of the distribution. Other functions are tried, which do not change the main conclusion of the paper. Eventually, we hope a full theoretical calculation can describe the full measured distributions.

L149-150 This is the only place where you address the 1n1n alpha distribution, but just to say that you substantially fit an exponential to two points (binned)!

- This is true. Because of the low statistics, we barely found a better way. The tail function is highly impacted by the last two point but the freedom does include the core points

L156: Neutron multiplicity classification: again it isn't clear how the migration between classes is treated. At L161-162 only 2n => 1n is considered. With a selection based on

the energy deposit in the ZDC, as far as one can understand, it is hardly imaginable that you don't have tails of the resolution and therefore migration between classes.

- As explained earlier, you are referring to two different effects. L161-162 is impurity due to finite ZDC energy resolution. It is most significant to 1n class while it is negligible to 0n and Xn classes. Migration from pileup is calibrated by ZB events and fully corrected.

L170-171 You just mentioned en passant at L56-57 that you want to measure also <M_mumu>, without saying why, so, as it is said here, it comes as a surprise.
-> To measure <M_mumu>, a second …

- We made the change as suggested and also explain the motivation of measuring <M_mumu> at L56-57

There are a few other points. The mass spectra are not shown, so it is very hard for anybody to have an idea of what you are doing: are they going to be part of the Supplemental material to be added?

- We are simply measuring a mean value of M_mumu so showing the distributions do not provide much information. It is a smooth distribution except for Upsilon peaks.

Another remark concerns the fitted mass region, 8 < M_mumu < 60 GeV, to start with, in which you hide the 9-11 GeV band. This looks artificial: no charmonium states appears beyond 4.6-4.7 GeV, so you could start at 5 GeV; on the other hand the cut for bottomonium is adequate but not so generous. Why this difference in treatment?

- The reason for the lower cut of M at 8 GeV is that muon efficiency drops dramatically.

L194-196 I am probably lost, but this second sentence doesn't seem much different from the previous one.

- We agree. Second sentence is removed.

L210-212 "many aspects" appears twice w/o much of an explanation or detailing.

- We agree and rephrase the last two sentences.

Type A
Abstract
The integrated luminosity could be mentioned also in the Abstract.

- Done.

line7-8 "larger" ... "smaller" imply a term of comparison, which is not explicitly stated, perhaps -> large ... small

- Done.

L11 -> initial-photon induced (if I understand correctly what is meant)

- Changed to “models of photon-induced interactions”

Text

L4 so-called -> the so-called (otherwise there's no link with the previous part of the sentence and one would need a verb)

- Done

L4 UPC -> UPCs (otherwise UPC at L16, 31, 108, 141 will not make sense; the alternative is to define 'ultra-peripheral (UP)' and then add each time 'collisions' or 'events')

- Done

L8 [6--8], -> [6--8] to be performed,

- Done

L9 Large Hadron Collider (LHC) -> LHC

- Done

L18-19 "Higher neutron multiplicity ... " can probably be omitted. Also, this would be the fifth time that you quote "[6--8]"!

- Done

L24-25 UPC collisions. -> UPCs.

- Done

L40 Same as above.

- Done

L42-43 Here "[6--]", quoted for the sixth time, are separated from "[7, 9-11]", contrary to what was done in L6-7. Somehow this is a repetition of L6-7.

- Done

L45-46 ultra-peripheral ... collisions -> lead-lead (PbPb) UPCs

- Done

L48 "1.5 nb^-1": according to https://twiki.cern.ch/twiki/bin/view/CMSPublic/LumiPublicResults#2018_lead_lead_collisions we collected rather 1.8 nb^-1. Where does the difference come from? Is it due to ZDC availability?

- Yes, it is due to ZDC availability.

L52 the large -> a large average \alpha

- Done.

L54: "and vice versa" here is probably not due.

- Removed.

L54-56 Do you want to say that possible effects would be washed out by smearing in p_T? Then say it.

- Done.

less of an effect -> a smaller effect

- Done.

L57 measured -> presented (see however the comments Type B and A to L170)

- Done.

L59 Probably one can omit "primary".

- Done.

L62 Omit "of".

- Done.

L67 A couple of characteristics of this "typical muon" could be mentioned, such as average p_T and E.

- This paragraph is standard CMS texts.

L85 track-> tracks

- Done.

L99 "Each ... or" doesn't match with the trigger requirement of at least one muon.
-> One of the muon candidates in the pairs, OS or SS, should match a triggered muon.

- Done.

L113 Any reason not to write: e_trig = e^+ + e^ - e^+*e^- ?

- Either way is correct and we prefer the current form.

L119: It is kind of curious that the only number reported with uncertainties in this paper is the result of another analysis made in a different experiment. The exact value of the cross section is not really relevant here, one could probably more simply write "is measured to be about 190 b at sqrt(N) = 2.76 TeV [38] and is expected to be even larger at sqrt(N) = 5.02 TeV"

- Since the exact number is available, we prefer to keep as it is.

L135-> where N_s represents the signal yield

- Done.

L138 -> leptons

- Done.

L159 UPC -> UPCs

- Done.

L160 "the leading HF energy deposit is tightened to 5 GeV": not really clear what this means -> the threshold to define the hadronic contamination is lowered to 5 GeV (if this what you want to say)

- Done.

Figure 2 caption. The meaning of the red dashed line should be explained also in the caption.

- There is a legend for this line. We prefer not to add it in the caption because of the word limit of PRL.

L170: The average di-muon mass is introduced quite abroubtly here. No anticipation (yet) about why it is interesting to look at it, what do you expect in term of dependency on the impact parameter... Probably it should have been anticipated in the first part of

the paper with a bit more emphasis than at L56-57 that this is one of the quantities you want to inspect as a function of the neutron multiplicity / impact parameter.

- We've modified the introduction as suggested.

L179 constant -> a constant

- Done.

L191 also studied -> presented

- Done.

L194 Omit "clearly".

- Done

L237-238, 239-240, 244-245, 250, 252-253, 312-313, 318-319, 320, 338-339 All lower case initials apart from that of the first word in the title and those of proper names.

- Done

L267 Ref [13] is published: Comput. Phys. Commun. 253 (2020) 107181, doi:10.1016/j.cpc.2020.107181

- Done

L323 -> experiment

- Done

L331 -> simulation

- Done

CMS-HIN-19-014-002-COMMENT-009

Dear HIN-19-014 editors:
It's nice to see this interesting paper having gone into the almost final stage. I have roughly read through the HIN-19-014-paper-v10.pdf, and would have some (large and small) questions and comments from a non-expert's point of view. I list them below, please make a note of it if any of them would be sound.
In case that these comments would not be displayed well, a simple text file is attached with the identical content.
Thank you and looking forward to hearing from you.
-Sijin
---------- begin ----------

In general
(1) Throughout the paper
(a) If the "gamma(Greek)" is explained at its 1st appearances in main text on L5 and in the summary paragraph on L200, i.e.

(i) L5: (also, it can be used immediately at the end of line to shorten) "photon-photon and photon-nucleus processes [3-8]. Two-photon co-" —> “photon-photon (gammagamma) and photon-nucleus processes [3-8]. The gammagamma co-"

(ii) L200: "first measurements of gammagamma -> mu+mu- production as a function of …” —> "first measurements of photon-photon production (gammagamma -> mu+mu-) as a function of ..."

(b) Then, it can be used to shorten many places from "photon" —> "gamma" on
L10-11 (two places),
L20,
L24-25 (two places),
L27,
L30,
L35,
L37,
L41 (two places),
L53-54 (two places),
L138-139 (two places),
L146,
L185,
L196-197 (three places),
L199,
L207 (two places), and
L209

- We think either photon or gamma is OK.

Page 0, in the Abstract
(2) The 1st two lines, the "gamma" and "Pb" may should be explained at their 1st appearances in the Abstract on these lines; but since the "Pb" has not been used again in the Abstract, so the "Pb" can be simply spelled out, i.e. "dependence of gammagamma -> mu+mu- production in ultra-peripheral PbPb collisions at ..." —> "dependence of photon-photon production (gammagamma -> mu+mu-) in ultra-peripheral lead-lead collisions at ..."

- Upon consulting CCLE, it is not necessary to spell these out.

(3) The 6, 9 and 11th lines then can be shortened from
(a) The 6th line: "from leading-order photon-photon scattering is" —> "from leading-order gammagamma scattering is"
(b) The 9 and 11th lines: (two places) "photon" —> "gamma"

- We think either photon or gamma is OK.

Pages 1-3
(4) L2-3, since the "Z" has been used for only one time on L3 in whole paper, and to avoid the possible confusion with the "Z" boson on L102, the charge "Z" may not be necessary to be introduced on L2, then it can be spelled out on L3, i.e. "charges (Z) can be treated as a flux of linearly polarized quasi-real photons [1, 2] with flux intensity proportional to Z2." —> "charges can be treated as a flux of linearly polarized quasi-real photons [1, 2] with flux intensity proportional to the square of the ion charge."

- Done.

(5) L12, I'm not sure whether the last comma in the bracket should be replaced by a word of "and", i.e. “[6-8, 12, 13]." â†’ “[6-8, 12 and 13]."

- We do not think it’s necessary.

(6) L22-23, L52-54, L106 and L136. I'm not sure whether the "LO" can be introduced at its 1st appearance in text on

(a) L22-23: (together with the item (1b) above to shorten since the "gamma" has been explained on L5; also, I'm not sure whether it would sound better if the verb "possess" is changed to an alternative) "The lepton pairs produced from leading order photon-photon scattering (gammagamma -> l+l-) possess small pair pT and are nearly back-to-back" —> "The lepton pairs produced from leading order (LO) scattering gammagamma -> l+l- have small pair pT and are nearly back-to-back"

(b) L52-54 (two places), L106 and L136: (four places, then can be shortened correspondingly) "leading order" —> "LO"

(7) L29, L48, L55, L60-61, L66, L80, L82, L104 and L118. These lines may be shortened from

(a) L29, L55 and L66: (four places, as the "phi" has been introduced on L24) "azimuthal ang..." —> "phi ang..."

(b) L48: (similar as the item (a) above) "Azimuthal correlations" —> "The phi correlations"

(c) L60-61 and L118: (two places, as the "EM" has been introduced on L1) "electromagnetic" —>”EM"

(d) L80: "and the shapes of the clusters in the pixel detector have to be ..." —> "and the cluster shapes in the pixel detector have to be ..."

(e) L82: "are required to be below 7.3 GeV and 7.6 GeV in" —> "are required to be below 7.3 and 7.6 GeV in"

(f) L104: (as the "MC" has not been used afterward in whole paper) "Monte Carlo (MC) simu-" —> "Monte Carlo simu-"

- We do not think it is good and necessary to use abbreviation everywhere. Otherwise, all done.

(8) L97 and L99. Since the "OS" and "SS" have been used for only one time each on L99 in whole paper, thus they may not be necessary to be introduced on L97 and in the 1st sentence on L99; then they can be simply spelled out in the 2nd sentence on L99, i.e.

(a) L97: "The opposite sign (OS) distribution ..." —> "The opposite sign distribution ..."

(b) L99: (three places) "the same charge sign (SS) muons in the same event. Each OS or SS pair must contain at" —> "the same charge sign muons in the same event. Each opposite or same charge sign pair must contain at"

- Done.

Page 4

(9) L143, it is mentioned that something is "documented in the supplemental material". But I seem have not been able to see any "supplemental material" in this paper yet. Would I have missed somewhere to look?

- Supplemental material is added.

(10) Eq.(2), to be consistent in this paper, an extra space should be removed before the colon on each of two lines of Equation, i.e. "core : c1 x … tail : t1 x (1 ..." —> "core: c1 x … tail: t1 x (1 ..."

- Done.

Pages 5-6

(11) L168, L175 and L196. These lines may be shortened from

(a) L168: "to be less than 4.0%." —> "to be < 4.0%."

(b) L175: "is below 1.8% across all neutron multiplicity classes." —> "is < 1.8% across all neutron multiplicity classes."

(c) L196: (as the "Mmumu" has been introduced on L45) "As the muon pair invariant mass is largely determined by ..." —> "As the Mmumu is largely determined by ..."

- We think either way is OK.

(12) L184, to be consistent with all other CMS and HEP papers, the font of "p" in "p-value" should be changed from (also, I'm not sure whether it is better to quota a Reference for the "p-value")" with a p(non-italic)-value corresponding to 5.7..." —> ”with a p(italic)-value [xx] corresponding to 5.7..."

- Done.

Pages 9-12, in the References Section

(13) The "year" numbers should be given for Refs.[2], [8], [13] and [30]. If there would be problems to display the year number with the default bib file, it may be fixed by changing from "article" to "unpublished" in the bib file.

- Done.

(14) L261, in [11], to be consistent with the main text of this paper, the subscript "NN" in the "sNN" at the end of article title should be changed from "sNN(larger letter size and placed higher)" â†’ "sNN(smaller letter size and placed lower)" Other ones which also need to be changed are

(a) by the same way: [19]-[23], [25]-[26] and [27](two places);

(b) by the similar way: [17] (from "sNN(italic)" --> "sNN(non-italic)"); [18], [24](two places) and [38] (from "sNN(italic and placed higher)" â†’ "sNN(non-italic and placed lower)"); [24] (from "pT(italic)" â†’ "pT(non-italic)"); and [31] (from "pp(italic)" —> "pp(non-italic)").

- Done

(15) L284, in [19], to be consistent in this Section and this paper, two spaces should be added before and after the symbol "=" at the end of article title, i.e. "sqrt(sNN)=2.76 TeV" —> "sqrt(sNN) = 2.76 TeV"

- Done

(16) L331, in [35], to be consistent in this Section, the 3rd word in article title should be in the lower case, i.e. "GEANT4: A Simulation toolkit" ---> "GEANT4: A simulation toolkit"

- Done

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CMS-HIN-19-014-002-COMMENT-010

Dear authors,
Congratulations on advancing this interesting result to be ready for publication. I have reviewed your paper draft on behalf of the statistics committee and have a few comments.
75-76: Perhaps you could say briefly what is explicitly required for the muon, e.g., hits in the muon and tracker?

- This is a L1 muon trigger where no tracker is involved. We change to explicitly that a muon candidate in the muon detectors.

150-151: Did you consider using a Poisson likelihood instead of the chi2. It is typically more appropriate in these circumstances, in particular, when you have bins with few events as seems to be the case with the 1n1n categories.

- Except the last two points in the 1n1n categories, the binconten/error is always larger than 3, so the chi2 fit should do a good job. We tried to use likelihood (option “WL”) and bin integral (option “I”) and found the likelihood fit was not stable.

equation 2: is a similar decomposition done for the M_mumu spectrum?Â It seems not to be the case.

- No, no similar decomposition can be done for M_mumu as it is a smooth, continuous distribution.

170-174: it's not clear to me how using a third-order polynomial really gives an estimate of the uncertainty on the fit procedure. Is the intent to determine the precision with which you measure <M_mumu> given the choice of a second order polynomial?

- The intent is not to estimate the uncertainty of the fit but the extrapolation in the Upsilon mass region we exclude in estimating the <M_mumu>. For this purpose, we also changed the fit range as we commented in AN 6.5 (v7)

179: is there an uncertainty associated with the STARlight estimate?

- No, STARLight is a simple leading order QED calculation for gamma+gamma->mumu events.

184: Could you elaborate the procedure for calculating the p value here? It seems that the dependence is increasing <x> with increasing neutron multiplicity (i.e, increasing impact parameter). Have you thought of carrying out a hypothesis test for this more specific scenario?

- The p value is based on the chi2 fit probability with a straight line, with both statistical and systematic uncertainties incorporated. This is the expectation in the STARLight model we reject. The observation is that alpha increases with neutron multiplicity. There is no quantitative hypothesis to test yet.

Cheers,
Nate