Comments on the paper:

l2: I propose to replace large charges (Z) by another symbol, maybe N_Z, as the reader could be confused with the usual Z notation of the Z boson.

- If possible, authors would like to keep symbol Z because denoting charge by Z is also a convention in this community. We directly use “Z boson” in this letter (e.g. in line 102)

l6-7: you could add that the dilepton production via photon collisions is also a background of the Z/DY to dilepton process (that increases with mass) and one of the main systematic for searches in dilepton final states.

- We totally agree this point is important. After a second thought, we would like to leave it as is because this aspect does not sound appealing as a general introduction. We can definitely come back to discuss if you do not satisfy this response.

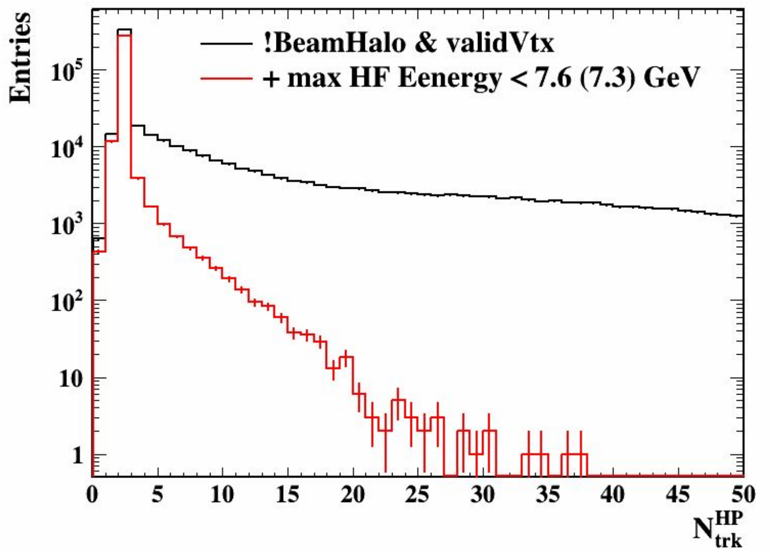
3rd paragraph: How to add to the picture the recent results by CMS+TOTEM about photon scattering? There is no mention of them currently: JHEP 07 (2018) 153 Since this analysis is a CMS analysis (I mean not only HI) It will be important to complete the full picture by mentioning the pp results and eventually see if a useful comparison could be made (cross-section ?)

- As we discussed in the ARC meeting, we would like to focus on the main physics point because the results will target a letter.

l78: « Events are required to have a primary vertex of interaction with at least two tracks », does not seem correct as in the note you say that you only select events with two high purity tracks exclusively.

- After discussing with Alice in vidyo meeting, we decided to remove “with at least two tracks” to avoid possible confusion.
- These two requirements do not conflict with each other. The official ”pprimaryVertexFilter” filter, applied in this analysis, requires “not fake vertex” && |Vz| <= 25 cm && |Vr| <= 2 cm (Vr is transverse radius of vertex) && tracksSize >= 2 (here, track is general track). The events with N_{trk}^{HP} == 2 meet the track size requirement of ”pprimaryVertexFilter” filter.

- The N_{trk}^{HP} distribution after applying "pprimaryVertexFilter" together with other event selection cuts is shown in the plot below



4rd paragraph: Why do you only look at Mass below 60 GeV? Don't you have events above 120 GeV? I did not see any mass plots going above 80 GeV but it could be interesting to know how many events you have above. In the pp results that I have mentioned they are looking at mass greater than 110 GeV. They have very few events but we could eventually do a comparison.

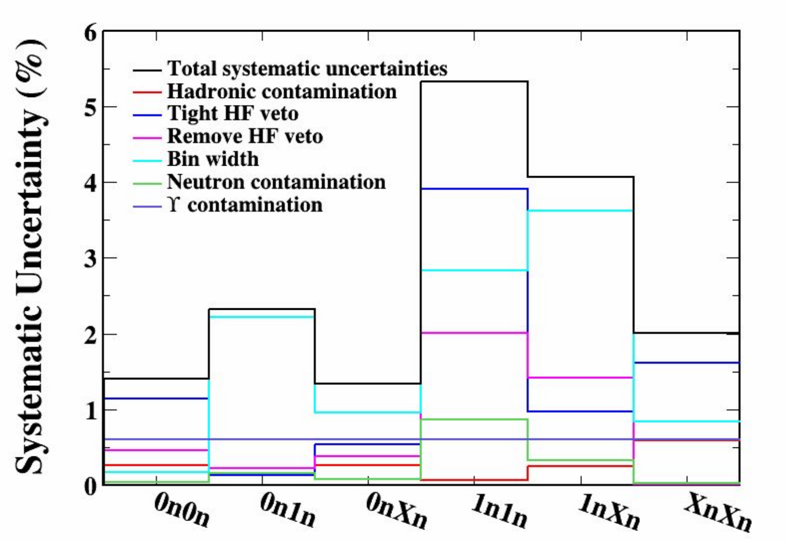
- The mass is selected to be below 60 GeV because we would like to suppress the possible contribution from Z bosons though there are only 30 entries and no resonance peak structure is observed between 70 and 100 GeV. There are only 8 counts beyond 120 GeV.
- Due to different collision systems, different center mass energy and possible different kinematics, it is not straightforward to do direct comparison between pp and AA.

Comments on the note

In the note you say that you applied a 99% threshold on the Leading tower energy in the HF. Could you say more about this? It is not really clear for me how you have decided and how you know that you keep high signal efficiency. Is it something that you have checked on simulation as well?

- The HF veto thresholds are purely determined using empty bunch crossing ("NO BPTX") events, which have no beam passing through the interaction point and thus no collisions taking place. **Therefore, the response of HF in "NO BPTX" events is purely from noise**, a cut is determined by keeping 99% "NO BPTX" events. In true UPC collisions, the HF should also have no signal response because no particle is generated within HF acceptance in UPC events generating two exclusive muons within CMS tracker acceptance. Therefore the same noise thresholds are applied to reject hadronic collisions and keep high UPC signal efficiency (noise response in HF).

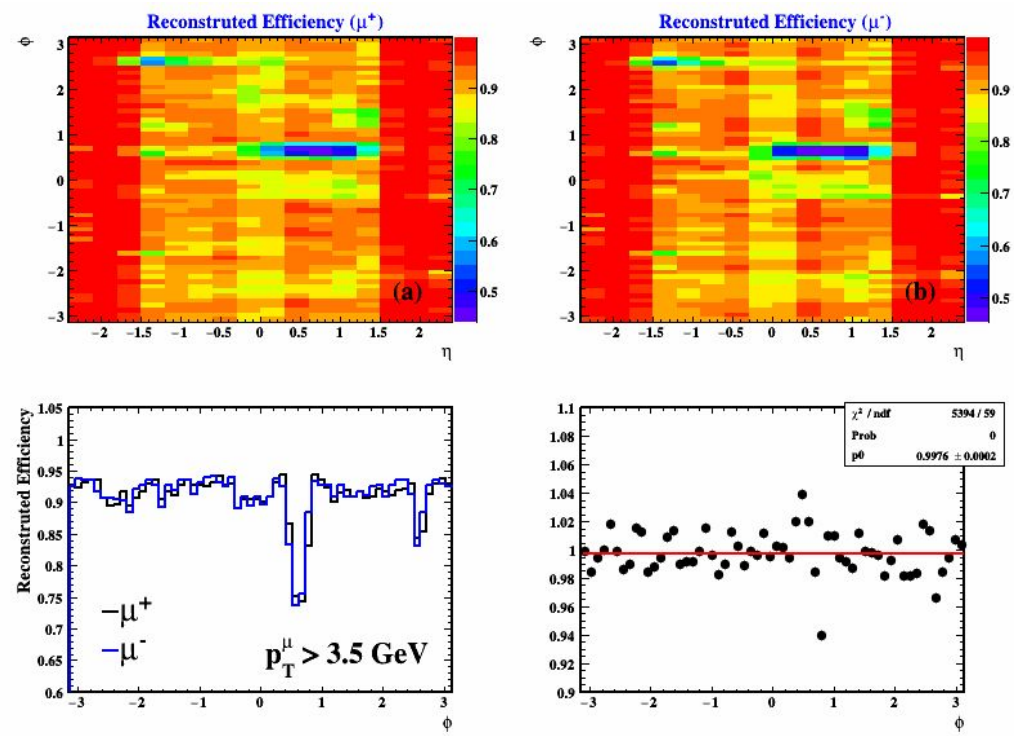
- The HF energy does not seem well modelled in simulation according to Emilien's message, so we did not checked this number on simulation
    - Alice follow-up comment: NOBPTX events are different from UPC events and surely some UPC events might be lost with this cut. We can not rely on simulation to account for the loss of efficiency as it is not able to reproduce well data.
        - Answer: We performed two exercises: 1) tight HF thresholds from default 7.3 (7.6) GeV to 5 GeV (already included in current systematic uncertainty) 2) completely remove HF veto selection. Compared to our default HF veto thresholds, the tight scenario rejects 22.8% more events while the elimination of HF veto gains 12.8% events, which tells us the default HF veto threshold does reject UPC events because of possible different beam background between NOBPTX and UPC events. Regarding to physics results (<alpha> and <M>) reported in this paper, the possible effect of removing HF veto (Magenta line) is well covered or represented by **current included systematic from tight HF veto** (Blue line), as shown in the figures below for <alpha> results.

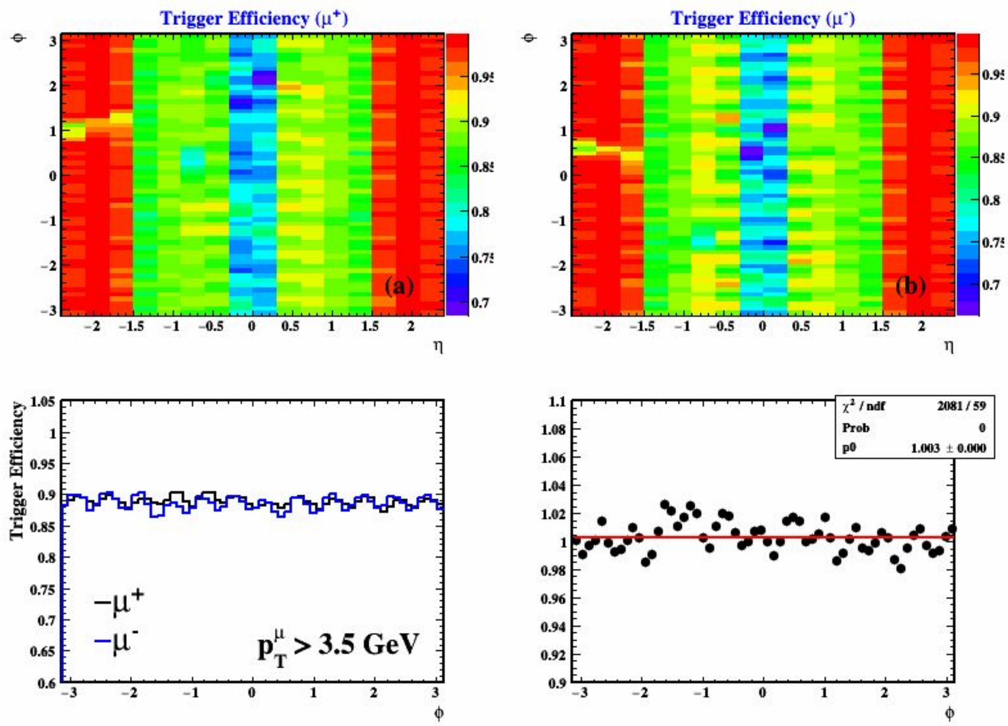


        -

On figures 2 and 3 of the note, it seems that there is slightly more inefficiency for mu+ than mu- as a function of phi. It is not something that we expect from a reconstruction + ID point of view. Could you provide a plot as a function of eta versus phi (instead of pT versus phi) and maybe make a ratio between mu+ and mu- efficiency as a function of phi in order to know if the difference is actually relevant.

- The efficiencies between mu+ and mu- are pretty consistent with each other. The slight difference between mu+ and mu- as a function of phi should be caused by detector acceptance. The two plots below are for muon pt > 3.5 GeV, and the dip at phi~0.5 also shows up in data, and the consistency comparisons between data and MC can be found in p5-6 of UPC TnP Study





Why do you take the low pT hybrid soft muons in pbpb collisions TnP results as upper limit? It will make more sense to look at SF from pp collisions, provided by POG for 2016 and 2017 years at least for soft ID (not for the trigger).

- This question is extensively discussed in the muon object questionnaires. We did a further study using our own TnP efficiencies in UPC and demonstrated the TnP SF effect is negligible. Alice and Ota approved this analysis does not need TnP SF (Thank you)
- The requirements of **soft muon and trigger (L1) in this analysis are looser than that of hybrid soft muon (L2 or L3 trigger)**. Therefore, the relevant efficiencies of soft muon are higher, very likely to result in a better agreement between data and simulation. For instance, the agreement between data simulation is generally better for soft muons than tight muons. We would like to emphasize that the hybrid soft muon TnP SFs only result in <0.3% difference on <alpha> and <0.05% on <M> results reported in this analysis. We expect similar level effects (or smaller?) from soft muon TnP SFs.
- I think we need to use the TnP SF from the collision species in the same run year to make sure the detector condition and trigger requirements are the same.

Actually do you know if the SF for hybrid soft ID in pbpb was approved by muon POG (I don’t remember to have seen these plots)?

- No, they have not been approved yet, and Batoul will present it on March 2nd in the Muon POG meeting. These plots are generated by myself according to the tables in [1].
- [1]: CMS-HIN-dilepton TnP SF for hybrid soft muon

On figure 9 it is hard to see if there is any entry on the wrong signed plot. Is there any?

- There are two entries for wrong-sign pairs (see bottom-left panel of Fig. 9, the 1D projection of top-right panel)

How can you conclude that the corrected sign is background free? Is it thanks to the same sign plot on the right?

- Yes, there are only 2 same-sign pairs compared to 87066 correct-sign pairs.

It is also unclear for me if the wrong sign plot means the same sign reconstructed pair (like a background event) or if it is actually a wrong charge sign assignment (an opposite sign pair reconstructed as same sign).

- In this analysis, the muon pt is more or less below ~30 GeV, I believe the probability of wrong charge sign assignment in this pt region is close to 0.
- Another reason for interpreting the wrong sign pairs as background is there might have hadronic events leaking into this analysis.
- Instead of using wrong-sign, we use same-sign in AN and paper now, because wrong-sign generally refers to wrong charge sign assignment
- We learned from Alice the sign-flip probability for global muon with pt < 100 GeV is 10^{-5}, and the probability should be even smaller (even for soft muon) for pt<~30 GeV, so we do not worry the charge sign flip issue

What is the signal efficiency of the requirement of the common vertex with a probability greater than 10-6? Is it really useful ?

- For the default event selections, the efficiency is ~100%. Thus, this cut is not very useful but does not hurt. We are totally fine to remove this cut.
- Removed the vertex probability thing in the paper.