Short comment on Yun He et al. paper

This is an interesting study, with a substantial contribution to scientific progress within the scope of AMT. It is based on ground-based lidar observations from Wuhan and provides regionally-tailored parameterizations for dust and cloud-relevant lidar-retrievals. The new methodology proposed, could be applied to different regions to provide regionally-tailored parameterizations for dust and cloud-relevant concentration retrievals. The paper is well written and the overall presentation is well structured and clear. It is recommended for publication in AMT after a few revisions. The specific comments are given below. The more important comments are connected with the discussion of the associated uncertainties of the retrieved products from the presented methodology.

Specific comment:

Page 2, line 38: "...except for the occurrence of ice multiplication mechanism (also named Hallett-Mossop process) at temperatures of -3 to -8 °C [Hallett and Mossop, 1974]". As more secondary ice production mechanisms have been proposed in the literature (see for example Field et al., 2017), consider revising this sentence accordingly.

Page 2, line 38: "This agreement was substantially verified by a closure study …". the same agreement was found in Marinou et al., (2019) paper using spaceborne lidar-radar retrievals. Consider including their findings in the reference also.

Page 3, line 2: "Urban air pollution generally cannot affect the atmospheric INPC [Chen et al., 2018]". Consider including also the recent references of Kanji et al. (2020) and Schill et al. (2020) which found out that soot in a bad immersion freezing INP (at temperatures > -30 C) with < 10% contribution to the total INPC.

Page 4, line 104: "The fine mode fraction (FMF) of 500 nm..". Till this point you provided the uncertainties of all the other products discussed. Consider including in the text the uncertainty on this product also.

Page 5, line 130: "In this study, the CALIOP Level-2 vertical feature mask (VFM) product was used to validate the presence of dust layers over Wuhan". As this validation could be done with the depolarization measurements of the ground-based system only, consider rephrasing to highlight the synergy of the two measurements, from ground and space, to provide the 3D dust presence.

Page 6, line 160: "The uncertainty for □_□ is on the order of 20% [Mamouri and Ansmann, 2014; Tesche et al., 2009]". This uncertainty is provided in relation to the total extinction coefficient and not in relation to the retrieved dust extinction coefficient from the formula, and is representative for the uncertainty on the α_d in case of dust dominated aerosol layer (with dp approximatelly 30% or higher, aka 100% presence of dust). When the pure dust component is less, the uncertainty of the retrieved a_d is higher. Indicatively a_d uncertainty is reaching >90% in layers with d_p = 10% (see for example section 3.2 in Marinou et al. 2019). I suggest describing the uncertainties more accurately.

Page 6, line 165 and line 171: “The uncertainty of ⍰_d is ≤ 60% [Mamouri and Ansmann, 2014]” ... “The overall uncertainty for ⍰_250, is estimated to be on the order of 30% [Mamouri and Ansmann, 2015]”. Similarly with the above comment, these uncertainties are representative for a dust dominated case (100% dust presence). Please rephrase accordingly.

Page 8, line 1, and Figure 3 and 4: The AE of the first time period this day (09:00-11:00) is even lower than the time used (12:00-16:00). Why is only the second time period considered dust relevant and is used? Could the authors provide additionally the size distribution of the first period in Figure 4? Or the first period is not included in this methodology due to some criterias it cannot fulfill?

Figure 2: As the δ and a_d profiles from this case are discussed in the manuscript to demonstrate the methodology, it is advised that the authors include in this figure these profiles also, during the period used. It would be good, for completeness, also if the authors mention the D selected for the demonstration case (which height region is averaged for the mean a_d in this case).

Page 8, line 128: “In total, we screened 32 dust-intrusion days from the sun photometer observation during 2011-2013.” Shouldn’t this be rephrased as: “In total, we used 32 dust-intrusion days from joint lidar and sun photometer observations during 2011-2013.” ?

Figure 5: You should skip “by sun photometer” in the end of the sentence. See also the above question.

Page 8, line 234: Please also provide the uncertainty or, if not possible, the standard deviation of the proposed conversion factor.

Page 8, line 239: “Moreover, two other AERONET sites were reported to also have..”. Consider rephrasing as: “These results are in line with conversion factors reported in two other Aeronet sites….”

Page 9, line 249: “Each point in Figure 6 …” please comment in the manuscript if these points are from the same set of lidar layers and Aeronet retrievals as the ones in Figure 5.

Page 9, line 253: Please also provide the error or, if not possible, the standard deviation of this conversion factor.

Page 9, line 255: “In particular, those more dispersed points below the dashed line seem to be more affected by anthropogenic aerosols”. Would it make sense to calculate one conversion factor for the elevated aerosol layers and a seperate one for the PBL layers? Would the retrieved factors be better representative for the different mixing conditions expected in the PBL and in the free troposphere?

Page 9, line 263: “Another slight dust layer with an enhanced δ of ~0.04 occurred up to ~8 km”. Please include the time of observation in the plot.

Figure 8: Consider including additionally the CALIPSO feature type plot of this case, for a complete overview of the scene. The elevated aerosol/cloud layer above the station at 7-9 km is visible here also.

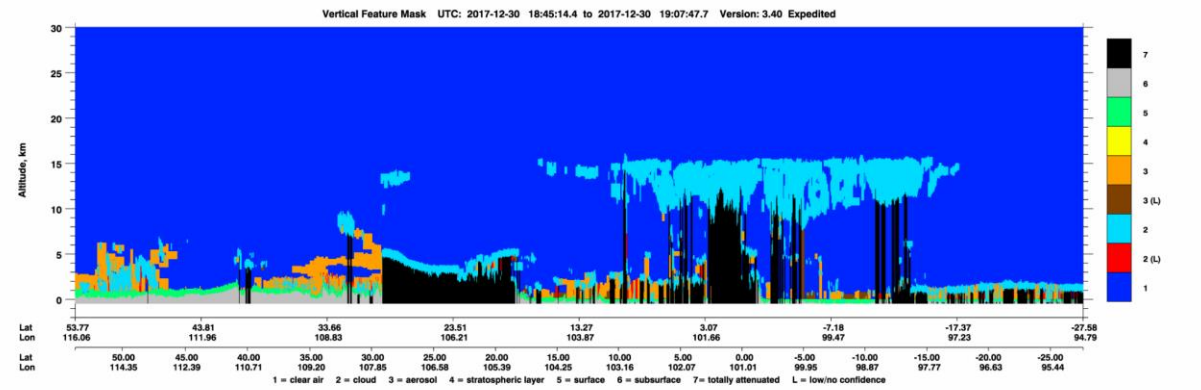

Figure 9 & 10: Please include also the error bars of the derived parameters.

Summary and conclusions: The authors could consider including in their conclusions a comment on the evaluation approaches that could be used/followed to validate the proposed methodology.

Technical corrections:

All manuscript: The reference brackets for ACP is () instead of [].
Page 2, line 47: “...Marinou et al..”
Page 2, line 51: “..in sit**u**...”
Page 3, line 83 & 91: “..particle **linear** depolarization ratio..”
Page 6, line 147: “...dy multiplying with a typical dust..”
Page 7, line 189: “ ..the filtered out all.. “ out should be deleted, as these are the values kept.
Page 7, line 190: “Considering the AERONET sites selected in Ansmann et al. [2019b] mostly located in/near the desert regions, the pure dust cases following the criteria given above can be found more easily(with adequate data sets >2500 for each site”. Consider rephrasing to: The pure dust cases following the criteria given above can be found more easily (with adequate data sets >2500 for each site) in/near the desert regions, as presented in Ansmann et al. (2019b).

Page 7, line 195: “...observed here generally reflect a characteristic of mixed dust (dust particles mix.. ”. Consider rephrasing to “...observed to generally reflect characteristics of mixed dust (dust particles mixed... ”.
Page 7, line 197, 198: “ ...properties **above** Wuhan…” “... different from those **in** near-desert sites”.
Page 11, line 324 “is **derived** at 2.0 L^-1”

References:

Field, P. R., Lawson, R. P., Brown, P. R. A., Lloyd, G., Westbrook, C., Moisseev, D., Miltenberger, A., Nenes, A., Blyth, A., Choularton, T., Connolly, P., Buehl, J., Crosier, J., Cui, Z., Dearden, C., DeMott, P., Flossmann, A., Heymsfield, A., Huang, Y., Kalesse, H., Kanji, Z. A., Korolev, A., Kirchgaessner, A., Lasher-Trapp, S., Leisner, T., McFarquhar, G., Phillips, V., Stith, J., & Sullivan, S. (2017). Secondary Ice Production: Current State of the Science and Recommendations for the Future, *Meteorological Monographs*, *58*, 7.1-7.20. Retrieved Jun 20, 2021, from https://journals.ametsoc.org/view/journals/amsm/58/1/amsmonographs-d-16-0014.1.xml

Kanji, Z. A., Welti, A., Corbin, J. C., & Mensah, A. A. (2020). Black carbon particles do not matter for immersion mode ice nucleation. Geophysical Research Letters, 46, e2019GL086764. https://doi.org/10.1029/2019GL086764

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