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Comment on acp-2020-1241

Anonymous Referee #1

Referee comment on "The Asian tropopause aerosol layer within the 2017 monsoon anticyclone: microphysical properties derived from aircraft-borne in situ measurements" by Christoph Mahnke et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1241-RC1, 2021

The manuscript describes the characterization of aerosols within the Asian Tropopause Aerosol Layer measured with a combination of in situ instruments onboard the M55 Geophysica research aircraft during the StratoClim field experiment 2017. The in situ data is compared to data from two near-range remote sensing instruments as well as satellite-borne lidar observations. The data presented here represent a valuable contribution elucidating aspects of the nature of this phenomenon so far detected only by means of remote sensing methods and balloon experiments.

The manuscript is well written and concise, the research is sound and in line with the overall subject areas of ACP. I would recommend the manuscript to be published after some minor points have been addressed.

General points:

It would be good to add a slightly more detailed characterization of the UHSAS-A measurement and the data analysis (potentially as a supplement to the paper) given that it represents the central measurement for this study. This instrument fairly complex and the results are sensitive to environmental influences such as low temperature and pressure as well as the assumptions on refractive index of the aerosol. The authors discuss tests of the stability of the sample flow in a low pressure chamber. Were the uncertainties in the flow during ascents and descents introduced by the layout of the flow system investigated as well (see Kupc et al 2018, doi:10.5194/amt-11-369-2018)? Have there been any experiments checking the counting efficiency in different size ranges? And which uncertainty is "estimated to be up to 25%" (l.124)
By default the instrument can measure sizes in up to 99 size bins, what were the considerations for the binning used to represent the size distributions?
Similarly, the size information from the NIXE-CAS instrument was not used fully but only as a single bin (Figure 6).

For the derivation of optical properties such as the backscatter ratio assumptions made for the shape of the input particle size distribution might be important. Therefore I would ask the authors to extend the description of those calculations and give an estimate for the uncertainties introduced by those assumptions.

I was a little confused by the term "Scattering Ratio". To my knowledge the lidar community commonly uses the more explicit term "Backscattering Ratio" for this quantity. Although I see that the cited reference also uses this term I would suggest renaming this throughout the text for clarity.

Specific points:

Title:
I would highly recommend writing out the acronym ATAL in the title so readers not directly familiar with the topic have a chance of understanding what this paper is about.

Abstract:
The abstract is relatively long for a not very long paper. It might be good to shorten that a bit. The measurement values in the abstract are given with a precision that is not likely reasonable. Throughout the paper, the authors should carefully revise all numerical values for stating a reasonable number of significant digits.

p7, l192. Check the precision of numerical values of MR. See above.

p7-8,l 210ff: I am not convinced that the total number of 1Hz-data points makes the median more robust here: At a given theta the possible values of those data points are not continuous but limited to certain values of MR because of the integer nature of the underlying count values which follow a Poisson statistics. The median cannot take any other value than one of the "stripes", therefore the slope of the median MR with theta in this upper region above 440K is primarily determined by the pressure/temperature structure of the atmosphere and even below that, between 420 and 440K, it will already be affected by the insufficient counting statistics. For the comparison to other instruments later in the manuscript this caveat should be added.
Possibly, resampling the data to longer time intervals might help to improve that statistics, though that depends on the detailed flight conditions in how far that would be meaningful. Resampling requires that the atmospheric conditions are quasi-homogeneous over that longer sample interval.

Fig 3: I am not sure this figure needs two panels given that the UHSAS-A and COPAS 2017 line are identical in both plots anyway and the UHSAS data is a repetition of Fig 1. By enlarging the figure the comparison to the other experiments should be sufficiently visible in just one panel.

p8, l239: I think "noticeable" might be the wrong word here.

Fig 4: Figure labels and the text in the legend are very small and hard to read. Please enlarge the labels and legend. Possibly the information inside the legend could be put elsewhere to reduce the size of the legend overall.

p9, l265ff: The comparison to data from other campaigns in this section is certainly interesting from a point of view of atmospheric physics but is not a strong argument to prove the performance of the modified UHSAS-A since those measurements were taken at different times and locations. I think the statements in this direction should be removed from this section, the findings regarding the agreement with previous measurements and the size distributions added by this measurement should be the main topic of this paragraph.

p10, l302: Although the last sentence in this section might be true it seems out of place here.

Sec 6.1: Also referring to the comment above about the choice of size bins for UHSAS and NIXE-CAS it would be good to see how well those instruments match in the overlap regions of both size ranges. In addition, as mentioned above, a discussion of the uncertainty in the backscatter ratio introduced by the assumption of those fairly large bins should be added here. The discussion in the paper by Cairo et al 2011 cited here refers to measurements of cirrus cloud particles which have different size ranges and optical properties and may not be directly transferable.
The refractive indices used to derive the size distributions assume purely scattering particles. Given the influence of convection on the ATAL discussed later the presence of absorbing material such as BC cannot be excluded. How would the uncertainty estimates on the derived quantities change if this cannot be ruled out?

p11, l319ff: Check precision of numerical values (see above).

Fig 6: Like for Fig 4, consider enlarging the axis labels.

Sec 6.2, Fig 7: Are there any uncertainty estimates for the various lidar measurements that could be added in this plot? In the range above 19km the yellow line of the MAL measurements is obscured by the in-situ-derived data.

p13, l374: The in-situ-derived scattering data are potentially affected by the sampling

issue mentioned above. Although the trend is likely robust the exact slope might not be. This should be stated as a caveat.

Sec 6.3: The authors should state how many CALIOP profiles are available in this region for the given time period and show a measure of variability in addition to the mean for those as well. Can the CALIOP profiles be split in time corresponding time periods as well?

Fig 8: Make sure the labels are sufficiently large to be readable in the final production. If there is only a single CALIOP profile to show consider merging the panels into a single figure.

Sec 6.4: As mentioned above detection of CO makes the presence of BC in the aerosol layer conceivable. How would the size distributions change if the assumption of a purely scattering refractive index is relaxed and would that have an effect on the derived scattering properties?

p15, l450: were in situ measured -> were measured in situ ...

p15, l473. The statement of slow vertical ascent is conceivable but not shown by the data presented here. Therefore, a reference should be given to this statement.

p16, l475: Is there an "and" missing before "removal"?

p16, l480: The mention of the box model come somewhat surprising here given that it has not been mentioned in the main part of the paper. Maybe rephrase the sentence to place the reference to Weigel et al 2020a more prominently.