Authors response:

We thank the referees for carefully reading and reviewing our manuscript. Your critical point of view and your constructive suggestions have certainly helped us to improve our manuscript. Below, the reviewer's comments are shown and our responses are added in green text.

Response to review comment 3

General comments

The authors present a first aerosol climatology over the measurement site of Mindelo, which is strategically located on the dust transportation pathway from Aftica to America. Dust-related seasons are defined based on the aerosol intensive properties in the boundary layer and within the lofted layers. The study provides useful results for communities related to aerosol typing/mixing, seasonal dynamics, and dust transportation. Important aerosol transport events such as the volcanic aerosol of La Palma can also be easily tracked from such a timeseries. The manuscript is well written and worth publishing. The figures are presented in a compact way that combines a lot of information without causing confusing. Below there are some specific and technical comments for the authors to address.

Thank you very much for your feedback and all your suggestions! With the help of your comments, we could find out these parts of the manuscript which were not, yet, clearly enough explained! We carefully worked through your comments and revised the manuscript accordingly.

Specific comments

Line 98-99 "a minimized systematic relative error of 15 % for the particle backscatter coefficient": Please shortly mention here where does this error come from (e.g. Ångström assumption, scattering ratio at reference height assumption?) and how was it assessed (e.g. analytically or with simulations?)

The error considers both assumptions – of the Ångström and of the scattering ratio at the reference height – and it was assessed analytically.

Line 100-101 "The errors of the lidar ratio and of the Ångström exponent were calculated via the Gaussian error propagation." Please specify wether this was an analytical or a Monte Carlo error propagation

We calculated it analytically and clarified it in the text.

Line 114-115 "Thus, the automatically-retrieved profiles of the aerosol optical properties are averaged over time periods between 15 min and 1 h, depending on the cloud conditions": It is not clear here if 15 min and 1 h averages can co-exists in an aerosol scene or whether one of the two sampling rate is used depending on the cloud conditions. Keep in mind that producing later on climatological values by 15 min and 1 h averages will lead to inconsistencies regarding the random error as 15 min average will always be more noisy. This will lead to larger error bars which is safe but not optimal. How is this handled in the climatology?

The sampling rate can be anything between 15 min and 1 h. The default and always preferred time period is 1 h. If the cloud gaps are shorter, the averaging time will be only as large as the cloud gap, but not shorter than 15 min. We are aware of the inconsistencies regarding the random noise but did not notice that strong differences in the noise for the different averaging periods. The backscatter coefficient and the depolarization ratio are quite stable. Also, the extinction coefficient is not that much affected. Most problems concerning noise we had with the lidar ratio and the extinction-related Ångström exponent. Anyhow, we cut the noise within the visual inspection and only included the quite stable parts of the profiles for the calculation of the layer mean values, which should reduce the inconsistencies between the different sampling rates. However, the larger problem than inconsistencies in the random error we experienced in the retrieval of the optical properties, because too short averaging periods sometimes lead to missing backscatter coefficients at 355 and/or 1064 nm and in very few cases also at 532 nm.

Line 145-150 Please provide here also the number available profiles per extinction and backscatter product. In addition, please specify if particle backscatter refers to pure Raman backscatter for 355 and 532 products (where any Klett profiles also included in the dataset?). Is the particle backscatter at 1064 nm solely produced by the Klett method?

Thank you for the suggestion! We added Fig. A1 to the appendix, which gives a detailed overview of the number of available profiles for all of the optical properties. In the complete study, we used only Raman profiles for 355, 532, and 1064 nm. For reasons of consistency, Klett profiles are not at all included in the dataset. We added this information in LL 145 – 146 for clarification.

Line 165 “The PBL top height was defined in the middle of the first significant gradient of the backscatter coefficient.” Is this also done by manual inspection? Please specify.

Thank you for pointing out that it was not clear enough explained! It was also done manually.

Line 173-175 For the defined aerosol layers, layer mean values were calculated for the intensive properties (lidar ratio, Ångström exponents,particle linear depolarization ratio, and dust fraction), while the extensive optical properties (total and dust backscatter coefficients and particle extinction coefficient) were integrated vertically.

Suggestion: An alternative way to produce the intensive averages in the layers is to produce them from the integrated extensive optical properties. This is more or less equivalent to producing an intensive property average by weighting with the corresponding extensive properties. The benefit compared to averaging the intensive properties directly is that the random error of the average is less affected by weaker parts of the profile that naturally have higher relative uncertainties.

Thank you for the good suggestion! However, we prefer to stay for consistency reasons with the approach we have already implemented in our previous lidar studies. We will consider your suggestion for future updates of averaging schemes.

Line 183-185 “For the uncertainty of the layer mean optical properties, the layer means of the errors described in Sect. 2.1 were calculated, while for the integrated values these errors were used as input for the Gaussian error propagation.”

This part is not very clear. A lot of data-related parameters were introduced in section 2.1. The authors should clarify to which errors they are referring to. In addition, the Gaussian error propagation method was mentioned previously without a specific explanation. Please provide some more information here. Is it based on Monte Carlo simulations or analytical formulas were used?

Thank you for pointing to this lack of clarity! We rephrased the sentence for clarification. The Gaussian error propagation was done analytically also here.

Lines 216 – 229 Are the averages (dots) weekly (Fri/Sat)? It will be helpful to remind the reader once again that each dot correspond to a Fri/Sat average.

This is correct! The single data points (dots) in Fig. 2 are the Fri/Sat cases. We specified it in the text.

Lines 227-229 This part could be written more clearly. The algorithm is simply sometimes confused due to lofted aerosol layers leading to higher uncertainty. In such cases the upper bar should be more trustworthy than the mean because of the presence of the lofted layers, which is the only source of such big error bars.

Thank you for your comment! We added some further explanations about these two large error bars and shifted that paragraph to Sect. 2.3 (LL132 – 240) as reviewer 1 asked for removing the results of the Hofer algorithm from the discussed results.

Figure 2 and lines 221-222 "as well as the temporal standard deviation of the seasonal mean layer top heights"

According to the authors the uncertainties come also from temporal averaging but this is not what is seen in figure 2. The error bars are mostly <100 m wide. However, from week-to-week the uppermost aerosol layer top can change be 2 km or sometimes even more. This contradicts the very small weekly variability. Is the temporal standard deviation really deployed?

Thank you for showing us that this paragraph was unclear! The sentence only refers to the numbers given in the plot (seasonal means and their uncertainty). The error bars of the single data points in Fig. 2 (Fig. B1 in the revised version) arise only from the variation of the threshold value in the Hofer algorithm. At the request of reviewer 1, we show only the manually-defined aerosol layer top heights in the revised version, so the error bars (not anymore shown in Fig. 3, please see figure caption) are constant with a value of 50 m as we manually defined the aerosol layer boundaries as full hundred meters (LL 201 – 202). The uncertainty of the seasonal mean values given as numbers is the temporal standard deviation for the 3-month-averages. We clarified it in the text.

Figure 3 and lines 252-266 There are some cases where the AOD retrieved by the lidar is higher than the sunphotometer AOD. Can the authors comment a bit on that? Is is due to diurnal cycle differences?

This is an important point which you are mentioning here! Such kind of differences in the AERONET and lidar AOD can be caused by temporal inhomogeneities in the aerosol conditions because the AERONET measurements were obtained during daytime while the lidar measurements originate from the night and the aerosol conditions can change in the meanwhile. Another possible reason could be an overestimation of the AOD of the PBL due to the interpolation in the region of incomplete overlap where we assume a constant extinction but the real one might be smaller than that value.

Lines 267-281 I would suggest that the Polyphon part goes first here so that the readers understand where the dust-related properties come from. In addition, the authors mentioned that a PDR of 0.31 was used. Was the 0.25-0.25 PDR range used for calculating the errobars of Figure 3c?

Thanks for the suggestion! We restructured the paragraph. For the calculation of the error bars, we used the analytic Gaussian error propagation with the uncertainty of the total particle backscatter coefficient, i.e., relative error of 15%, and with the uncertainty of the dust backscatter coefficient, also 15% relative error, as the dust fraction was calculated as the ratio of the dust backscatter and the total particle backscatter. We also added this explanation to the revised manuscript (LL 133 – 136).

Lines 285-289 The seasonal pattern that the authors describe here it is not so easy to see, at least not for the lidar ratio and for the angstrom exponent that have high uncertainties. Indeed the PDR seems to show some difference between Nov and April for both 2021-2022 and for 2022-2023 in the lofted layer. But for the other 2 parameters I find it really hard to discern a repeating pattern. Indeed the LR difference at 355 and 532 is higher during 2022-2023 but for 2021-2022 this is not so clear. The authors could refer to the monthly averages that show more clear patterns

You are right, it is quite hard to see in the plots with the single cases and easier to identify in in the monthly mean time series! We revised the paragraph according to your suggestion.

Line 307-309 This sentence is not so clear and needs to be refined. Which are these eight data points?

Thank you for pointing to this lack of clarity! It means that the monthly mean data points for the lofted layers are averages of at most eight single values, because the sublayers of the lofted aerosol layers were considered separately, i.e., four weeks (four Fri/Sat cases) per month and year and two sublayers per case gives the number of eight. We rephrased the sentence and hope that it is clearer now.

Line 359 The Angstrom exponent of the lidar ratio is not a very common lidar product. It should be introduced at some point in the paper before being used to define a threshold. The authors should also shortly describe what high/low values represent.

Thanks for the comment! We have removed it from the analysis as it is not very common and also not really necessary for the definition of the seasons, because the aerosol features of the mixing season are already sufficiently described by the dust fraction in the lofted layers and the PBL.

Line 395-428 In this part, the results between different past and modern campaigns are discussed. In order to really compare the values and draw firm conclusions one has to consider also the corresponding uncertainty. The authors should keep in mind that differences can only be significant if the uncertainty is sufficiently small.

This is absolutely correct! We have it in mind and indicate to the general agreement but some differences within the uncertainty range at the beginning of the discussion between past and modern campaigns (LL 467 – 470).

Technical comments:

Line 85 Typo here: “Aeolus was a equipped…”

Done.

Line 94-95 “one for far-range (fr) and one for near-range (nr) measurements”: I would suggest to define those 2 acronyms (fr and nr) with capital letters (FR and NR) to be more easily recognizable.

Done.

Line 102 “a newly calibrated instrument”: Please replace with: “a newly calibrated sunphotomer” (to avoid confusion with the lidar)

Done.

Line 112 “0.7 × 105 MHz m”: Should the units here be MHz m^-1 ?

You are right. We corrected it.

Line 122-123 “The resulting smoothing length is 382.5 m for the nr-measurements and 742.5 m for the fr-measurements, which are the standard values used in the processing chain.” Please provide here the 2 window sizes also in range bins.

The smoothing lengths correspond to 51 and 99 bins. We added it to LL 136 – 137 of the revised manuscript.

Line 125 “The basis for this study were data” -- > “The basis for this study is data”

We changed it to “The basis for this study are data”, because the ACP language guidelines demand the word “data” to be considered as a countable noun.

Line 142 “and any badly calibrated profiles” -- > “and any profiles with calibration issues”: Here, the sources of the calibration issues that the authors have experienced can also be mentioned in parenethesis (e.g. polarization calibration, Rayleigh calibration etc)

Thanks for the suggestion! We added it.

Lines 188-190 Are these results presented somewhere in more detail? Please point to the corresponding section

The results of the error estimation you are referring to are used as asymmetric error bars for the automatically-retrieved layer top heights. On the request of reviewer 1 we shifted the results of the Hofer et al. algorithm to the appendix. So, you will find them in Fig. B1. We also added a reference to this figure when describing this error estimation in the methodology part.

Lines 206 “On only few days,” -- > “Very rarely” (or similar expression)

Done.

General comment I would suggest using an abbreviation for north hemispheric (e.g. NH) because it is repeated often and becomes distracting.

Done.

Line 256-257 “caused only small values of the AOD” -- > “contributed to only a small fraction of the AOD”

Done.

Line 270 “up to 0.5 was found” -- > “up to 0.5 were found”

The verb refers to “a higher dust fraction”, so we think it has to be in singular here.

Figure 3 and table 1 Please rename “Dust Fraction” to “Dust Integrated Backscatter Fraction” (or similar)

We renamed it to “Integrated dust backscatter fraction“

Line 304 “the particle linear higher depolarization ratio” -- > “the higher particle linear depolarization ratio”

Done.