Response to reviews of "Sunlight-absorbing aerosol amplifies the seasonal cycle in low cloud fraction over the southeast Atlantic" by J. Zhang and P. Zuidema

We would like to thank Michael Jensen and another anonymous reviewer for their constructive comments and suggestions on our manuscript, which encouraged us to think more deeply about our analysis and helped us improve the original manuscript.

Specific responses to each comment are contained below, with the reviewer's comments provided in blue and our responses in black. Changes to the manuscript made in response to the reviewer are provided in red italics. We have also made unsolicited smaller changes to the manuscript to further polish the writing.

REVIEWER 1 (Michael Jensen):

This manuscript builds on the authors' work published in 2019 and presents results from the analysis 8 months (July-October 2016, 2017) of observations of aerosols, clouds and environmental conditions to investigate the interactions of frequently occurring smoke particles and low-level cloudiness in the Southeast Atlantic. An important component of the analysis is a breakdown to subseasonal (monthly) timescales which allow consideration of the variability in boundary layer structure due to large-scale meteorological influences. The important results of this work quantify the influences that smoke, transported form the African continent, has on the seasonal evolution of cloud properties. The manuscript represents an important contribution to the field and is well-written. I do believe that there are a few minor issues and edits that should be made for improved clarity before the manuscript is published in Atmospheric Chemistry and Physics.

We thank Dr. Jensen for these nice comments and helpful suggestions. The manuscript has been revised accordingly to clarify these issues and concerns. Direct responses to comments and changes to the original manuscript are listed below.

Minor Comments:

Lines 109-120 – I found this section, comparing the ACAOD, rBC mass and fine mode AOD, a little confusing to keep track. One source of confusion is he use of "These" in line 114. I think "These" refers to ACAOD and τ_{af} . I think then that the purpose is to use τ_{af} as a proxy for ACAOD such that one avoids the problems of ACAOD only being available when clouds are present. If that is the case, it would be helpful to also include joint histograms of τ_{af} and rBC. It might also be helpful to use τ_{af} on the label axis for Fig. 1 (right most panel) and in the legend for Fig. 2.

We have reconstructed this paragraph to improve its clarity. Variables are referred directly by their names. Joint histograms of τ_{af} and rBC are added to the revised Fig. 1, and "τ_{af}" is added to the axis labels in Fig. 1, new Fig. 2, and the legend of the new Fig. 3, (and used consistently throughout the rest of the revised manuscript). Below is the reconstructed paragraph in the revised manuscript. The last 2 sentences of the original paragraph have been moved to the subsequent paragraph.

"The basic approach is to construct composites of the more and less smoky conditions for each month, and to analyze the differences in cloud properties with an eye on the accompanying meteorology as well as aerosol. Composites can identify representative conditions more robustly than case studies, and are more

forgiving of anomalies as long as the anomalies do not dominate. A difficulty rests with what to call smoky in each month: surface-based measurements may not be indicative of the free-tropospheric aerosol loading and vice versa. Joint histograms of daily ACAOD and rBC mass concentrations over Ascension, by month, indicate that smoke is predominantly present in the boundary layer during July, equally frequent in the boundary layer and free-troposphere in August, and mostly in the free troposphere in September and October (Fig. 1a-d). This is consistent with aircraft in situ and lidar assessments, when available (Haywood et al., 2021; Redemann et al., 2021), and with the surface-based lidar assessments, although the latter can be obscured by lower cloud, and, as a point measurement, may not be representative of a larger region. Yet, ACAOD is only available when there is cloud underneath (Meyer et al., 2015), allowing free-tropospheric smoke in clear conditions to go undetected. The ACAOD measure may be most suspect in July, when there is less low cloud compared to other months (ZZ19). To better detect all smoke, the daily-mean clear-sky τaf is also examined. The July joint histogram of τaf and rBC mass concentrations in July confirms the free-troposphere is frequently clean, and that smoky periods primarily consist of high loading of near-surface rBC mass concentrations (Fig. 1e). Later in the season, in September and October, the level of agreement between the joint histograms of rBC-ACAOD and rBC-τaf further supports the interpretation of a shift in smoke vertical distribution towards the free-troposphere over time (Fig. 1g and h). That said, ACAOD and τaf are not entirely interchangeable, with a correlation of only ∼0.55 over a 3◦ by 3◦ domain-average in September and October and with ACAOD often exceeding τaf (Fig. 2). Comparisons to aircraft-derived above-cloud aerosol optical depths reveal a genuine high bias to the satellite ACAOD estimates (Chang et al., 2021), thought to reflect a too-high single-scattering albedo assumption within the retrieval (Peers et al., 2021).”

Line 165 – It is not clear how the stratiform and cumuliform classifications shown in Fig. 3b are done. Are these based on the observer reports? A radar-based classification? Or something else? It would be helpful to clearly identify where these are coming from.

We really appreciate this comment, as we forgot to introduce the classification of the stratiform and cumuliform clouds in the original manuscript. The classification is based on the standard WMO codes. Surface observers at Ascension (from the UK Met office) observe the sky and record the cloud type according to these codes 8 times per day. Details in the classification of cloud type are added to the revised manuscript, as shown below (this is added to the Datasets and compositing approach Section where the UK Met office surface-observer-reported cloud type dataset is introduced, constituting the new lines #77-81):

“Surface observers from the United Kingdom’s Meteorological Office at Ascension Island, trained to look away from the island, report cloud types following the World Meteorological Organization’s (WMO) protocol (WMO, 1974) every 3 hours. These reports inform the frequency of occurrence of cumuliform and stratiform clouds over Ascension. Cumuliform clouds include low-cloud type 1 (CL=1, cumulus with little vertical extent) and CL=2 (cumulus with moderate or strong vertical extent). Stratiform clouds include CL=4 (stratocumulus formed by the spreading out of cumulus), CL=5 (stratocumulus), CL=6 (stratus but not of bad weather), and CL=8 (stratocumulus and cumulus with bases at different levels).”

Line 180 – Here, in the discussion of Fig. 4a and later in the discussion of Fig. 5a, the cloud boundaries being higher or lower are mentioned. But this information is somewhat buried in the profiles of cloud frequency of occurrence. To help clarify these discussions, it would be helpful to provide statistics of the cloud base and top heights themselves.

We agree with the reviewer that providing statistics would help us convey the description of these observations more effectively. We now added statistics of cloud top heights and cloud base heights in the new Fig. 6, 7 and 9 to facilitate the discussions on the island-based radar-derived cloud vertical structure under high versus low smoke loadings. Quantitative mention of the changes in cloud base/top heights as a function of the smoke loading is also added to the text in the revised manuscript.

Line 183 – Would suggest replacing “drizzles” with “precipitation.” The disdrometers are not very sensitive to drizzle sized droplets likely need larger precipitation drops to make a recording. If you decide to stick with drizzle, it should be “drizzle” rather than “drizzles.”

We replaced the word “drizzles” with “precipitation” and refrained ourselves from using “drizzles” in the rest of the revised manuscript.

Suggested minor edits:

Line 74 – Add “rain gauge” after “tipping bucket.”
Line 80 – Add “the” before “Terra and Aqua.”
Line 113 – Add “compared to other months” after “lower.”
Line 114 – Add “a” before “3o x 3o”

These are edited accordingly in the revised manuscript.

Line 122 – What threshold are you using to define “high ACAOD?”

Optical depth of 0.2. This information is added to the revised manuscript, as “*A few days exist with high ACAOD (above 0.2) with those in 2017 coinciding …*”

Line 125 – What is meant by “composite decisions?” Is this the determination of high or low smoke days? Would “composite classifications” be clearer?

Yes, and thanks for the suggestion. “composite decisions” is replaced by “composite classifications” in the revised manuscript.

Line 179 – It would help clarify this sentence to start with a phrase like, “For more smoky conditions...” It was not clear to me at first that this is what was being discussed.

Sentence reconstructed, now reads as “*For more smoky conditions in July, low-cloud is less frequent throughout the day (Fig. 6a), cloud bases are higher by 50-90 m, and cloud tops are typically lower, by up to 150 m, compared to less smoky conditions.*”

Line 189 – “absorbing” should be “absorption.”

Corrected.

Line 190 – “extending through the night” suggests that shortwave absorption occurs through during the night. I think you mean to say that the warmer sub-cloud layer persists through the night when no shortwave absorption is taking place.

Yes, and we rewrote the sentence, which now reads as "*Given that smokier conditions last for a few days (Fig. 3), the shortwave absorption can warm the sub-cloud layer over multiple days, with the warmer sub-cloud layer persisting through the night (shown for August in ZZ19), supporting a boundary-layer semi-direct effect.*"

Line 217-218 - The difference in the sharpness (difference from inversion base to inversion top) of the inversion is a combination of the sharpness of the individual cases, and the variability in inversion height (both base and top) across the composite population. Reporting the statistics of the inversion base, inversion top and thickness (in height, and change in temperature and water vapor) would be useful. Another way to separate the impact of the changing inversion base height is to normalize the height coordinate by the inversion base height.

We thank the reviewer for raising this important issue, and we totally agree that reporting statistics of features of the inversion can be very helpful to the discussion of the boundary layer thermodynamic structures. Therefore, we have added statistics of inversion bases/tops (shown as box-whiskers) and change in potential temperature and water vapor mixing ratio across the inversion (in mean values) for each composite on the new Fig. 6-9. Quantitative mention of the changes in inversion height and thickness, as well as changes in temperature and water vapor across the inversion, as a function of the smoke loading are also added to the text in the revised manuscript. The derivation method of these inversion statistics is added to Section 2 in the revised manuscript (new lines #70-72)

In addition, we also added a sets of radiosonde profile composites, corresponding to Fig. 6-9, with height normalized by inversion base heights, as a supplement (Fig. S1 and S3-S5).

Line 228 – Add "the" before "boundary layer".
Line 234/235 – Add "the" before "Ascension region."
Line 235 – "weaken" should be "weakened."
Line 237 – "is correlating" should be "correlate."
Line 249 – "an" should be "a."

These are edited accordingly in the revised manuscript.

Line 248 – Same comment as that for line #217-218.

Addressed the same way as in previous comment Line #217-218.

Line 274/275 – With the exception of the 12-18 LST cloud frequency profile, the plot does suggest greater cloud occurrence during the less smoky cases. Have you done a statistical test to determine that these profiles are not significantly different?

The reviewer is correct about this observation. A student's T-test does suggest significant difference (at 95% significance level) in the cloud frequency profiles between smokier and cleaner conditions, and this island-based radar observation is consistent with the satellite-derived cloud fraction difference over a larger area over Ascension for October, except in the afternoon (12-18 LST).

Description of the October KAZR cloud frequency profiles is rewritten in the revised manuscript (new line #298-302) as "*At Ascension, the radar-derived cloud frequency profiles (October 2016 only) emphasize a more persistent stratiform cloud structure, through the linear increase in cloud frequency with height,* *lasting throughout the diurnal cycle and invariant of the smoke loading (Fig. 9a). Cloud occurs more frequently when it is less smoky (Fig. 9a, confirmed through a Student's t-test), consistent with the satellite-derived low-cloud fraction covering a larger area (Fig. 5a), except in the afternoon (12-18 LST).*"

Line 289 – "Offshore" is used twice. Suggest removing the first instance.

The first "offshore" is removed.

Line 294 – It would be very helpful to the reader to note the latitude of Namibia here.

Latitude range of the coast of Namibia is added to the revised manuscript.

Line 326 – It would probably make sense to put square brackets around [2018] here.

We think our current in-text citation format is in agreement with the ACP guidance/requirement, as we prepare the manuscript using the LaTex .cls and .sty files provided by the ACP journal editorial office.

Line 329 – It is not clear how the profile is "bench-shaped." Is this a term used elsewhere? If so, please provide a reference.

We have removed this term from the manuscript and just mention the profile is sharply-defined.

Line 329 – It looks like the maximum in the longwave cooling profile is > 50K/day at cloud top near a height of 1.7 km? Is the 28K/day just for the smoke layer?

Yes, all discussions here were meant for this moist-smoky layer in the free-troposphere. Statements related to this are rewritten to avoid confusion, which now reads as *"These yield a sharply-defined longwave cooling profile, maximized at ~-28 K day^{-1} over a 50 m distance at the top of the free-tropospheric aerosol/moisture layer."*

Line 330-331 – It is very difficult to see this difference in height of the maximum of LW versus SW cooling/heating. Adding some grid lines, or some other horizontal line to Fig. 8 would help the reader see this.

An inset plot zooming into 4.2-4.5 km of the heating profiles is added to panel a), to facilitate reading, especially the location of the maximum LW and SW cooling/heating.

Line 336-340 – This last paragraph seems tertiary to the previous discussion and does not really add necessary support. I would suggest removing it.

We appreciate this comment. Instead of removing it, we tried to make it more relevant to the discussion of this section, by emphasizing the observed free-tropospheric mixed-ness between composites of September and between September and October regardless of smoke loading. Now the beginning of this section (new line #336) reads as "*The September thermodynamic profile is better-mixed, to a higher altitude, over the southeast Atlantic (Fig. 7a and Fig. 8a) when more absorbing aerosol is present, and in comparison to October. …. here we show that longwave cooling at the top of the humidity layers can help maintain their vertical structure through encouraging downward small-scale mixing.*"

Line 342 – Remove "the."
Line 343 – Suggest " We extend the work of ZZ19..." rather than "This extends Zhang and Zuidema (2019)..."
Line 351 – Suggest "compared to" rather than "than."
Line 360 and line 377 – Use acronym (LWP) for liquid water path.

These are edited accordingly in the revised manuscript.

REVIEWER 2:

This manuscript seeks to explain the role of absorbing aerosol on cloud structure throughout the biomass burning season in the southeast Atlantic as opposed to focusing on a single month or the biomass burning season as a whole using observations collected during 2016 and 2017 at Ascension Island. The work is certainly a worthwhile contribution to the literature however there are some concerns that should be rectified first, as listed below. Many of the figures contain a lot of information, so much so, that it can become overwhelming.

We thank the reviewer for these nice comments and helpful suggestions. We recognize that some figures integrate quite a lot of information, which can be overwhelming to readers. One small change we have made is to split Figure 2 into two separate figures. But otherwise, we have kept the figures to be one figure per month, as we believe this best communicates the co-variance between the large-scale meteorology, thermodynamic structure, aerosol condition, and cloud properties between the composites – basically we are not sure how to do this better.

Direct responses to comments and changes to the original manuscript are listed below.

Line 120: Is there any concern about subgrid box variability and sharp gradients with such a large grid box? Could this explain why figure 3a shows roughly the same cloud fraction for more and less smoky cases in September, but Figure 5a indicates more clouds (or thicker) with smoky conditions.

This is an excellent point, and we thank the reviewer for raising it. We previously looked at domains averaged over 2°x2°, 3°x3°, and 4°x4° regions to eliminate the concern that we might have identified smoky/less smoky days with a sharp spatial gradient, so that we are only picking days with a rather homogenous aerosol field over Ascension.

However, we hadn't done the same for the SEVIRI low-cloud fraction composites, which are at a rather large domain size (4°x4°). The cloud fraction over the Ascension region can also possess a sharp spatial gradient with the equatorial region to the north. To address the reviewer's concern (which we agree with), we broke the 4°x4° domain into four 2°x2° smaller domains and plotted the areal-mean low-cloud fraction conditioned by high and low smoke loadings (Fig. R1, light- red and blue curves). A persistent spatial gradient in low-cloud fraction over Ascension area is observed throughout the season, with the northwest corner being the lowest and the southeast corner being the highest, as expected. However, the subseasonal evolutions in low-cloud fraction within these 2°x2° domains are aligned with each other and agree with that of the 4°x4° domain. We believe this can eliminate the concern about subgrid cloud fraction variability causing a biased or muted difference between the high and low smoky loading composites, especially for September.

The diurnal-mean low-cloud fractions of the high and low smoke composites of September are indeed similar, with the cloud fraction of the high smoke condition being only a tad smaller, which is mostly driven by the lower cloud fraction over the northern part of the 4°x4° domain (Fig. R1). To reconcile this satellite-observed subtle difference in low-cloud amount with the ground-observed (KAZR) change in cloud frequency (original Fig. 6a), when more smoke is present, we looked at the full diurnal cycle composites of the SEVIRI-derived low-cloud fraction adjustment (Fig. R2; also included in the newly added supplement material as Fig. S2) and found a similar increase, although weak, in cloud amount during the afternoon, as indicated by the KAZR composites. It's true the change is weak, but we now show support through two independent datasets, with the SEVIRI analysis encompassing a 400 by 400 km domain. We recognize the ground-based cloud radar (KAZR) derived vertically-resolved cloud frequency is impacted by i) it being a point measurement, ii) a potential systemic bias from orographically-produced cloud at the AMF1 site, and iii) point-measured cloud frequency and areal cloud fraction are more interchangeable/equivalent when data record is sufficiently long. We have added language to this effect.

New lines #191-193

"A compositing on smaller domains (2◦ x 2◦ within the 4◦ x 4◦ domain, not shown) does not affect this result. A spatial gradient exists, with more cloud to the southeast and less to the northwest, but the cloud fraction evolutions agree between the domains."

New lines #235-237

"The island-based cloud frequency profiles can be limited in interpreting cloud cover over a larger area, due to a systemic island orographic effect and subsampling by the relatively short time series of point measurements, but diurnal cycle composites of SEVIRI-derived low-cloud fraction also indicate an increase in afternoon cloud cover, if weak (Fig. S2)."

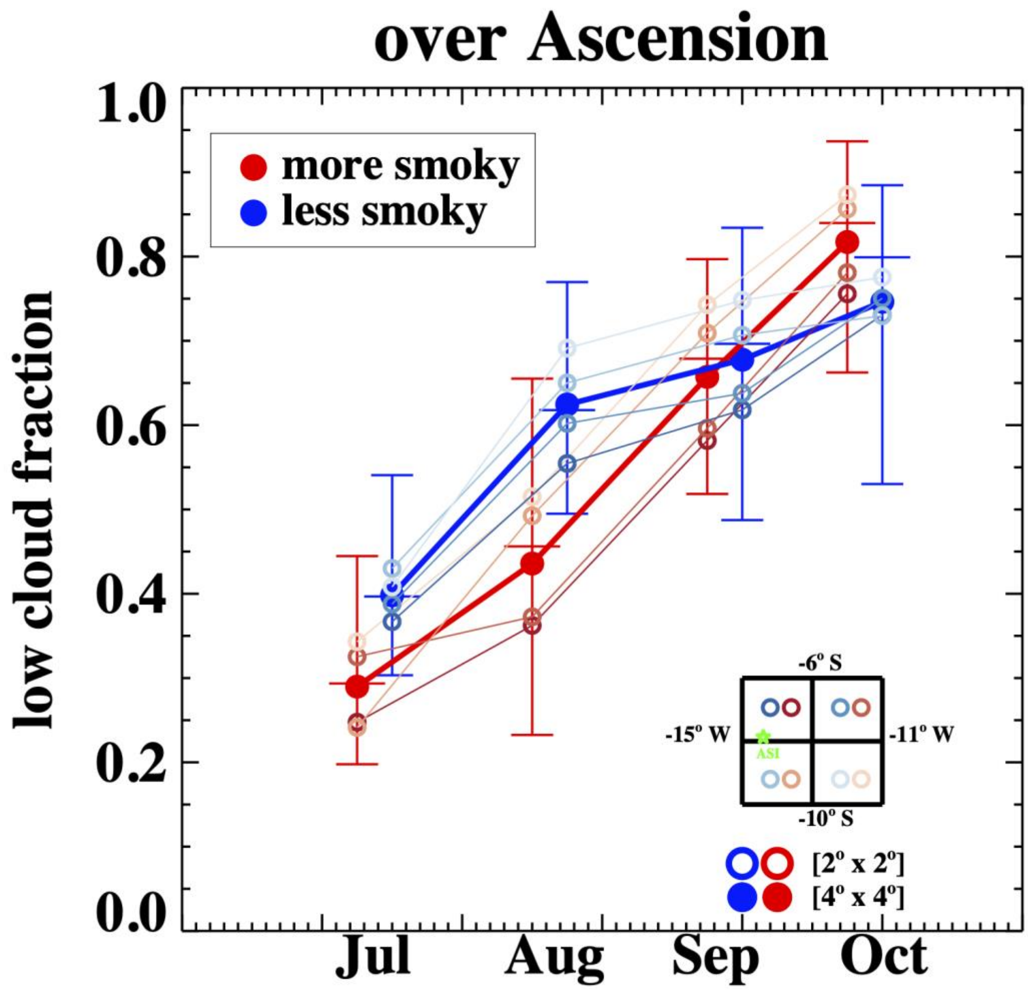

Figure R1. SEVIRI-derived areal-mean (4°x4°) low-cloud fraction (as in Fig. 3a). Added curves are areal-mean subgrid (2°x2°) low-cloud fraction within the 4°x4° domain.

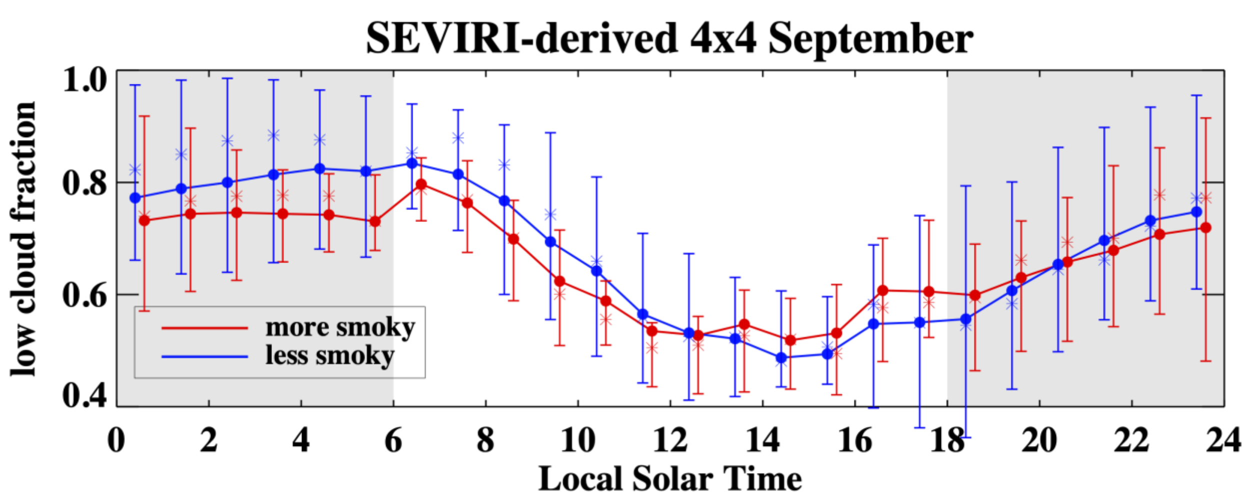

Figure R2. Diurnal cycle of SEVIRI-derived low-cloud fraction, composited by more and less smoky conditions. Means, medians, and inter-quartile range are indicated by filled-circles, asterisks, and error bars, respectively.

Paragraph beginning on Line 121, Page 4: This assessment seems difficult to follow, and subjective. The terms “mostly tracks”, “track each other well”, “tracks fairly well” are used to characterize three distinct periods and three different variables, yet it is hard to distinguish between these. The second half of July 2016 and July 2017 are indeed in excellent agreement, but the character of the agreement between the surface and free troposphere is very different during the other months and this is the point that I believe is trying to be made. This paragraph could benefit from some refinement, with connections made to what was learned from the histograms.

We thank the reviewer for pointing out the need for a refinement on the descriptive statements in this paragraph. It has been reconstructed as below:

“Elevated smoke layers are rarely present over Ascension in July (Fig. 1a). A few days exist with high ACAOD (above 0.2) with those in 2017 coinciding with high near-surface rBC mass concentrations, but not so in early July 2016; Fig. 3, both confirmed by lidar observations (not shown). Variations in column τ_{af} track the near-surface rBC variations, except for early July 2016 (Fig. 3). This suggests that the surface-based rBC values can reasonably indicate when aerosol exists within the atmospheric column, most of the time, for July. In August, τ_{af} and ACAOD variations are more similar instead, when they are both available within a 3◦ by 3◦ domain. High ACAODs/τ_{af} s appear to anticipate high near-surface smoke loadings by up to a week (Fig. 3). As an elevated smoke layer is almost always present above Ascension during August (Fig. 1b and visual inspections of island-based lidar profiles), surface-based rBC variations are deemed to better represent the variability in total column aerosol loading than ACAOD. Therefore, for August, the composite classifications follow those of ZZ19, which are based on the near-surface smoke loading. Occasionally, an increase in the free-tropospheric smoke loadings is evident over the larger (3◦ by 3◦) domain before it is perceived by the island-based rBC measurement, for example during the third week in August, 2016. In September and October, the near-surface smoke loadings are much less than in July-August (Fig. 1), and τ_{af} and ACAOD vary more similarly instead (Fig. 3). The τ_{af} confirm that those days with missing ACAODs indeed correspond to days with little free-tropospheric aerosol (e.g., early September 2016, 2nd week of October 2016 and 2017).”

Line 140: A careful assessment should be conducted regarding the selected days for high and low smoke loadings with regard to the composites. A larger number of low smoke days, for example in July, could have a muted composite compared to the smaller number of high smoke days. Furthermore, the large scale circulation in the southeast Atlantic differed between the two 2016 and 2017 (as pointed out by the earlier shift in regime in 2016) so it should be established whether the difference in the large scale meteorology composites were due to more cases in one year over the other.

We agree with the reviewer on the concern of imbalanced composite size, and ideally, we would want to have perfectly balanced composite size for high and low smoke loading days. A tercile or quartile-based composite classification guarantees more balanced composite sizes, but the tercile or quartile thresholds are not necessarily representing a meaningful condition in terms of aerosol loading. Thus, in this study, we weighed the advantage of maximizing the number of samples in each composite (to increase the robustness of the statistics, particularly for spotty measurements such as rain), while maintaining a balance and similarity within each composite. As a result, the composite sizes are typically comparable between high and low smoke-loading conditions - except for the month of July, as the reviewer points out.

To address this concern, the robustness of our July analyses/results is tested by applying a quartile-based composite method that has matching composite-size. In this test, threshold values of 30 and 300 ng m^{-3}

are used for low and high smoke conditions, representing the first and last quartile values (33 and 290 ng m^{-3}) of the rBC mass concentration histogram of July. This yields 13 (14) less (more) smoky days, a major reduction in the number of cleaner days, compared to our original number of selected less smoky days for July. **The test obtained same results/conclusions/findings as shown in our original July analyses with less-balanced composite sizes, shown in Fig. R3.** For this reason, we continue to base the results we hope to publish on our original analyses with 25 cleaner days because it does make the less smoky composite more robust. Note we adopted the slightly relaxed high smoke-loading threshold of 300 ng m^{-3} in the revised manuscript, yielding 4 more smokier days than the original smokier composite while showing same results with more robustness. It must be a greater lack of synoptic variability in July that allows composites based on 13 and 25 days to appear similar. We do modify the manuscript to mention the lack of sensitivity to the composite size.

The methodology part is re-written/reconstructed to make the descriptions clearer to readers, and some modifications are highlighted below:

New lines #109-112

"The basic approach is to construct composites of the more and less smoky conditions for each month, and to analyze the differences in cloud properties with an eye on the accompanying meteorology as well as aerosol. Composites can identify representative conditions more robustly than case studies, and are more forgiving of anomalies as long as the anomalies do not dominate."

New lines #143-144

"We use approximate daily-mean thresholds to establish the more/less smoky composites for each month, relying primarily on the rBC values for July and August, and τ_{af} (primarily) and ACAOD (secondarily) for September and October."

New lines #155-156

"The larger clean composite size in July of 25 days, produces results similar to those for the cleanest 13 days (which would be more balanced with the number of high smoke loading days) but are more robust."

Regarding the interannual variability on large-scale meteorological regime shifting, we don't think the difference in the large-scale meteorology composites were due to more cases in one year over the other. The season is that, as established by previous studies (e.g., Adebiyi and Zuidema, 2018), the aerosol conditions over the remote southeast Atlantic correlate well with the zonal wind strength (meteorological regime) in September/October, and thereby composites based on aerosol conditions come with corresponding meteorology, regardless of coming from 2016 or 2017 and time of a month (early or late).

We have added some language to that effect (new lines #156-162).

"The number of high and low smoke loading days is not necessarily evenly distributed between the two years, for example more smoky days are identified in October 2016 than in 2017, owing to interannual variabilities in biomass burning activities over the southern African continent and in the seasonally shifting large-scale circulation pattern (Redemann et al., 2021; Ryoo et al., 2021). Prior work establishes that the variability in aerosol loading is primarily governed by the zonal wind strength in September and October (Adebiyi and Zuidema, 2018). A compositing based on aerosol conditions will also select for the representative synoptic regime, independent of differences between the two years."

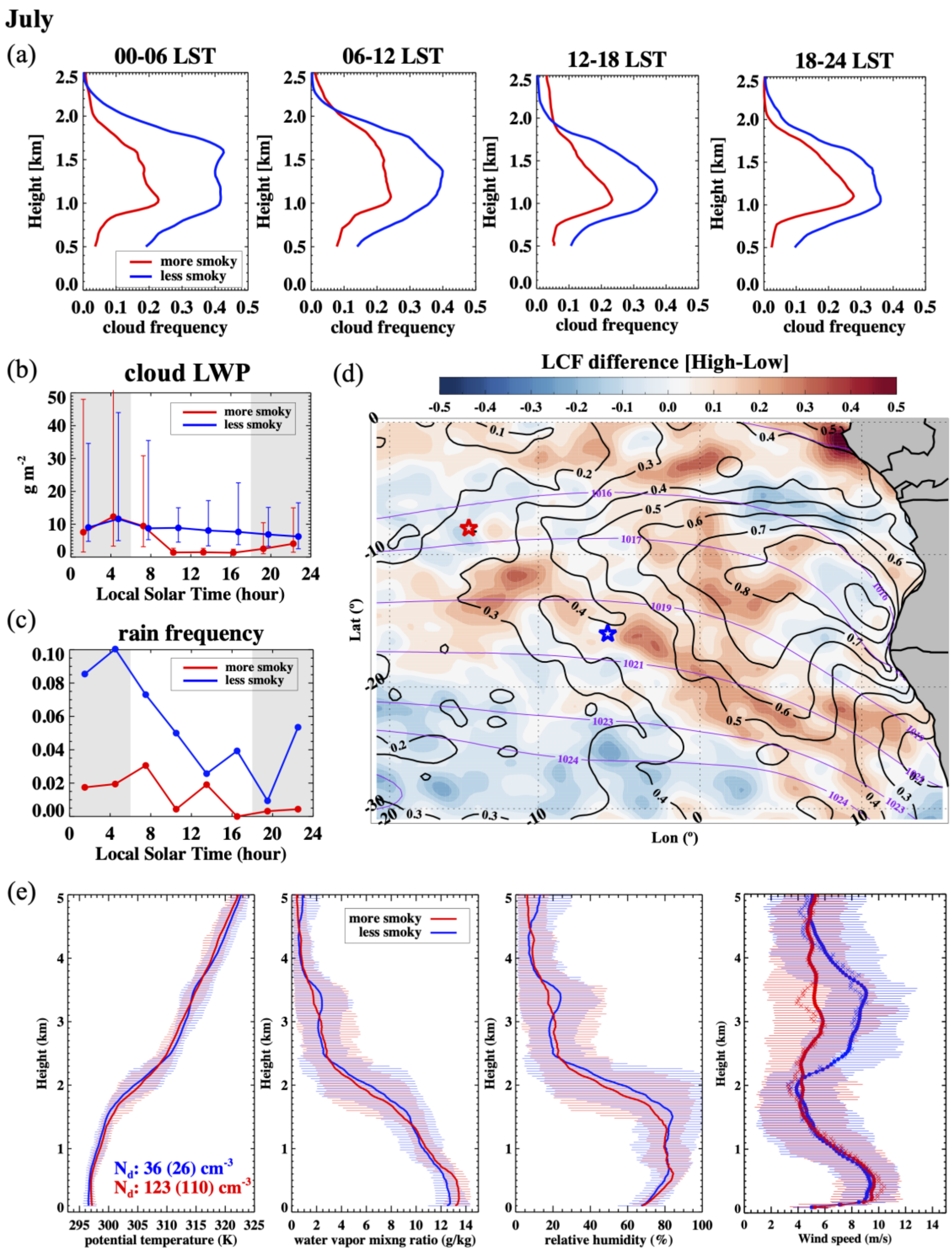

Figure R3. As in Fig. 5 of the original manuscript but using a quartile-based composite method with balanced composite-size (see text for details).

Lines 160-162: This sentence seems out of place.

Sentence removed.

Line 165: How is stratiform vs cumuliform determined? The surface observers?

We thank the reviewer for noticing the missing description of cloud type classifications, as we forgot to introduce them in the original manuscript. Please see the responses to reviewer1's second minor comment above.

Line 181: typo (should be bases, not based)

Corrected.

Line 223: typo (days WITH little smoke)

Corrected.

Section 7: Important details are missing regarding the RRTMG simulation. What exactly is being fed into RRTMG? Clouds must be represented somehow due to the peak in SW warming and LW cooling at the top of the PBL, and the kink in LW at cloud base around 750 m. Is an angstrom exponent specified? Does SSA not vary by wavelength or humidity for simplicity? I also feel that more can be done with this type of simulation as the section seems somewhat incomplete. How does the change in height of the aerosol layer from July/August to September/October alter the heating structure and stability of the atmosphere?

We thank the reviewer for pointing out the missing details in the RT calculations. Cloud water content is calculated from the radiosonde profiles based on the adiabatic assumption, and cloud optical properties are calculated by assuming a droplet number concentration of 40 cm^{-3}. SSA is spectrally dependent but not humidity-dependent, and the choices of SSA, AE, and AAE are all based on LASIC reported values (Zuidema et al. 2018 GRL). Added details in the revised manuscript is shown below (new lines #352-358):

"*Instantaneous radiative transfer calculations are based on a noon solar zenith angle, a spectrally-dependent single scattering albedo (SSA) of 0.8 at 529 nm based on Zuidema et al. (2018), and an asymmetry parameter of 0.67 loosely based on Cochrane et al. (2021). The spectral dependence of SSA relies on an absorption angstrom exponent of 1 and a mean angstrom exponent of 1.9 (Zuidema et al., 2018), with no humidity dependence. A cloud layer consists of cloud water content calculated from the radiosonde profiles using the adiabatic assumption, with cloud optical properties calculated assuming a cloud droplet number concentration of 40 cm^{-3} following Painemal and Zuidema (2011)*"

Although more could be done, all we aim to do with this section is to indicate that even though the LW cooling by the vapor is less than the aerosol SW heating, the offset between the two heating profiles has ramifications for the aerosol vertical structure that may be showing up in the monthly-mean composite profile for September. We do modify the language a bit in this section to clarify this.

Figure 1: Please add panel labels

This figure is updated to include joint histogram of fine-mode AOD and rBC mass concentration, as suggested by reviewer 1. All subplots are plotted on the same color scale, and unified axis range is used, except for August, for an easier comparison among the months.

Panel labels are added.

Figure 2: 1) fine mode AOD should be in the legend box; 2) \tau$_{AC}$ is used in the caption, but ACAOD is used everywhere else; 3) The amount of information in this figure is almost overwhelming. Consider making panel b its own figure and rearrange so that the winds are in separate panels from the other variables.

This figure is split into 2 separate figures, as suggested. Legend box and captions are revised accordingly.

Figure 8: This figure should be split into 2 panels, one with observations, the other with the radiation output. Qv and theta should also be displayed with different colors or line styles. Variable names should be consistent with previous figures which use potential temperature

Suggestions taken, and the figure is revised accordingly, in coordination with comments from reviewer #1.

We thank again the reviewers and the editor for their time and energy towards improving this manuscript.