ACP-2021-939: Reply to Reviewer 2

Smoke in the river: an AEROCLO-sA case study, Cyrille Flamant et al.

The authors would like to thank the reviewer for her/his valuable comments which helped improving the quality of the manuscript. Our point-by-point responses to the reviewer's comments appear in blue below. The text modified in the revised version of the MS and included in the response appears in red.

General comments:

The manuscript entitled "Smoke in the river: an AEROCLO-sA case study" written by Cyrille Flament presented the formation of a river of smoke over south Africa found in AEROCLO-sA campaign. Based on full dataset of reanalyses data, numerical modeling, ground-based, airborne, and space-borne measurements, this study suggested the interaction between temperate tropical trough (TTT) and cut-off low (CoL) to promote the transport of biomass burning aerosols. This kind of study is essential to interpret "smoke in the river" and I would like to consider the possible publication. However, I have fundamental questions on the numerical modeling used in this study. I would like to request to address my concerns listed below.

Major comments:

Description of Meso-NH: The current description of Meso-NH includes ambiguous statement. Please clarify following specific points.

Why high resolution simulation is needed in this study?

Using fine-scale Meso-NH simulation allows use to conduct direct comparisons with high resolution airborne observations at a commensurate scale (e.g. 1 km horizontal resolution and 30 m vertical resolution for the LNG lidar data). Furthermore, while the CAMS and ERA5 products are suitable for describing the large-scale circulation and aerosol distribution, high resolution modelling allows having a better description of the topography in the area and its influence on aerosol transport, especially in the presence of escarpments as is the case along the Namibian coastline. In section 2.1.2, we added:

"Running the model at a relatively fine resolution allows a comparison with high spatio-temporal resolution airborne observations at a commensurate scale, as well as having a better description of the topography in the area and its influence on aerosol transport."

As stated in the MS, there are 64 levels in the Meso-NH simulations, including 14 in the lower 1 km and 30 in the lower 6 km of the atmosphere, with bins sizes ranging from 60 m near the surface to 600 m above 7 km. In section 2.1.2, we added (line 149):

"…(14 of which in the lower 1 km and 30 in the lower 6 km)…"

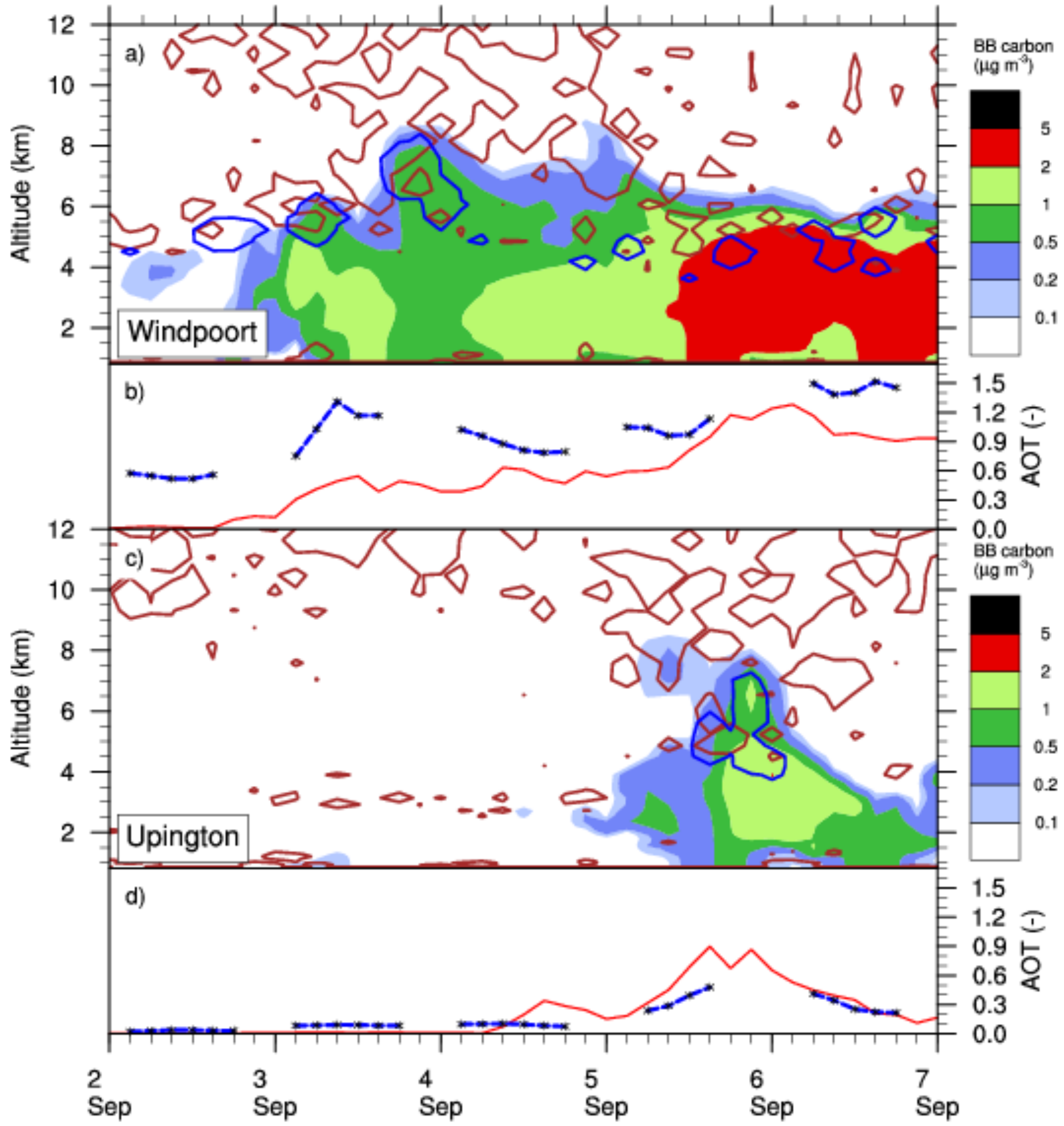
For the analysis from 2 September, there is only one day spin-up time. Is it appropriate to adequately remove the initial condition? (Even in Figure 14, 1 September is plotted but is it appropriate?) Related to this point, there is no description for the initial condition data. What kind of initial condition was assumed?

We do not use initial condition for aerosols, so basically the BB tracer concentrations are set to 0 at the beginning of the simulation and the BB tracer concentrations build-up with time. It can be hypothesized that, in source regions, BB tracers will fill the lower 6 km of the troposphere in about 12 h, i.e. approximately the time for the atmospheric boundary layer to develop during the day.

Therefore, the assessment of the BBA Simulation is meaningful only after a spin-up period that we estimate at 12 hours. This is now mentioned in the MS in Section 2.1.2:

"No initial conditions are assumed for BBA, so that BB trace concentrations builds up with time in the simulation domain. In source regions, BB tracers will fill the lower 6 km of the troposphere in about 12 h, i.e. approximately the time for the atmospheric boundary layer to develop during the day. Therefore, the assessment of the BBA Simulation is meaningful only after a spin-up period that we estimate at 12 hours."

We agree that showing BB tracer related AOT on 1 September can be deceptive. Hence, as suggested by the reviewer, we have excluded 1 September from the comparison with AERONET in Fig. 14 and with now are showing Fig 14 starting on 2 September.



The proxy for BBA are used by organic matter taken from CAMS whereas black carbon is analyzed from Meso-NH. I guess that black carbon is an important proxy to represent BBA, but the purpose of using black carbon is passive tracer?

Agreed. The expression 'black carbon passive tracer' is misleading. In fact, we use in Meso-NH a BB tracer that includes both organic carbon and black carbon. GFED emission maps are based on BB carbon fluxes, including both black and organic carbon. This is now corrected throughout the MS and in the figures.

Only GFED emission is used to calculate Meso-NH chemistry? Even though the biomass burning is dominant emission sources to this analysis, available emissions of anthropogenic source and biogenic source are needed to be considered to represent the chemical field over modeling domain.

This study is not focused on the chemistry of smoke, and reactive chemistry is beyond the scope of the paper, which focuses on meteorology and atmospheric dynamics. For the period of interest, the CAMS simulations highlight that BBA dominate the atmospheric composition, and other sources (pollution, biogenic, traffic) do not significantly contribute to the aerosol load.

As stated, GFED emission's grid resolution is 0.25 degree and this is approximately five times greater than the grid resolution of Meso-NH. How to interpolate into 5 km horizontal spacing? Without the fine-scale representation of emission itself, what is the advantage to conduct fine-scale Meso-NH simulation?

Here, we use a standard interpolation on spatial grid. Regarding the resolution, see our answer at point #1

In addition, because the treatment of vertical allocation is an important aspect to describe biomass burning emission sources, the information of the vertical grid allocation is required to understand the modeling treatment.

In the case of pyroconvection, it is indeed correct that the vertical allocation of BBA could be an issue. However, this is not the case over the region considered here as already discussed in several studies (e.g. Labonne et al., 2017; Menut et al., 2018; Mallet et al., 2020, among others). On average, fires are not intense enough to inject BBA above atmospheric boundary layer.

In section 2.1.2, we added:

"The BB tracer is then mixed vertically by turbulence in the atmospheric boundary layer. Fires in the area of interest are not intense enough to inject BBA above the atmospheric boundary layer as discussed in several studies (e.g. Labonne et al., 2017; Menut et al., 2018; Mallet et al., 2020, among others)."

Labonne, M., Breon, F.-M., and Chevallier, F.: Injection height of biomass burning aerosols as seen from a spaceborne lidar, Geophys. Res. Lett., 34, L11806, https://doi.org/10.1029/2007GL029311, 2007.

M. Mallet, F. Solmon, P. Nabat, N. Elguindi, F. Waquet, D. Bouniol, A. Sayer, K. Mayer, R. Roehrig, M. Michou, P. Zuidema, C. Flamant, J. Redemann, and P. Formenti, 2020: Direct and semi-direct radiative forcing of biomass burning aerosols over the Southeast Atlantic (SEA) and its sensitivity to absorbing properties: a regional climate modeling study, *Atmos. Chem. Phys.*, **20,** 13191–13216.

L. Menut, C. Flamant, S. Turquety, A. Deroubaix, P. Chazette and R. Meynadier, 2018: Impact of biomass burning on pollutants surface concentrations in megacities of the Gulf of Guinea, *Atmos. Chem. Phys.* 18, 2687–2707.

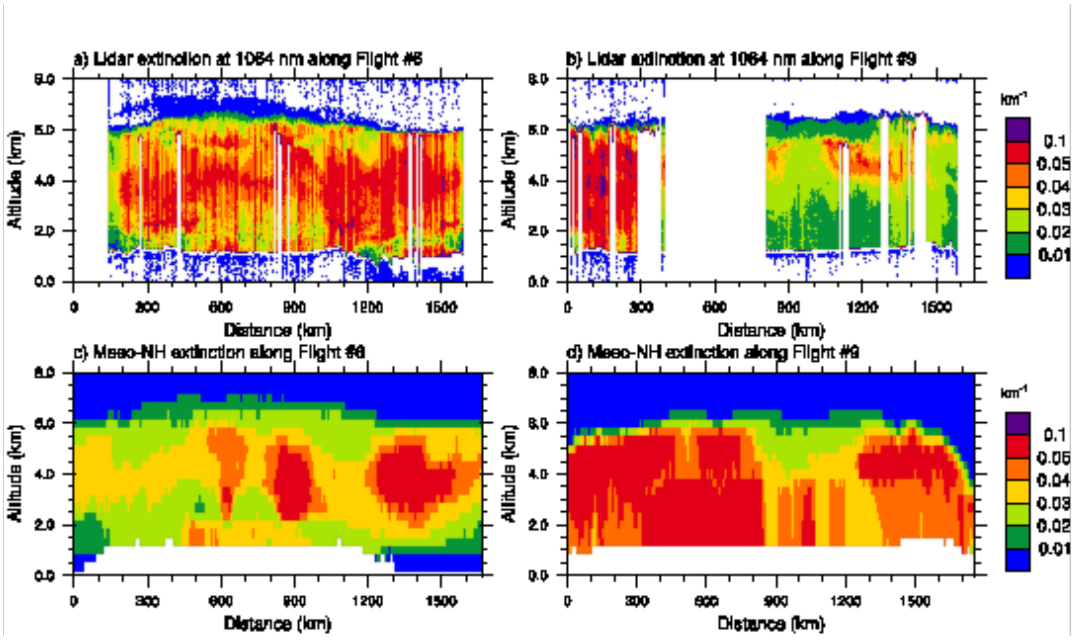
After the clarification of these questions on model configuration, I am wondering the performance of Meso-NH itself. There is no direct comparison for modeled black carbon. In Figure 9, model were indirectly compared to measured attenuated backscatter coefficient. Despite the discussion in line 396-404, I simply impressed that model posed much mixing of black carbon from the surface to 6 km.

Observed high coefficients were only found in 3-6 km, and I am suspicious the modeling skill to capture the measurement. Without the appropriate modeling performance, results drawn from Meso-NH model could be also suspicious. I would like to request to include more discussion to reinforce the modeling performance by Meso-NNH to represent the behavior of black carbon.

In Figure 9, we are now showing comparison between extinction derived from the airborne LNG lidar and extinction derived from Meso-NH at 1604 nm. This allows a more direct comparison between observations and model outputs. We have added a description of the lidar-derived extinction products in Section 2.2.3:

"Extinction coefficients at 1064 nm are retrieved from ABC profiles using a standard lidar inversion method that employs a lidar ratio of 40 sr, characteristic of BBA. The retrievals have an estimated uncertainty of 15 %."

The new Figure 9 showing extinction coefficients instead of ABC is shown below.



Specific comments:

Line 24: This is not consistent to line 81. Please confirm.

Agreed. For the sake of consistency and accounting for the referee's comment, we have modified the sentence line 81 as:

"Tropical temperate troughs (TTTs) typically form when a tropical disturbance in the lower atmosphere is coupled with a mid-latitude trough in the upper atmosphere (Lyons, 1991)."

Line 94: The campaign period was August-September, and the analyzed event was early September. In my understanding, this analyzed period could be regarded as late winter to early spring and not to fit winter. Is this contradict to the statement in Line 91-92?

Agreed. The period of the campaign corresponds to late winter *stricto sensus*. We have modified the sentence line 91-92 to reflect this as:

"However, the role of TTTs in the transport of BBA during the **late** winter has never been investigated until now."

Line 108: There is no definition for “TTL”. Please clarify.

Corrected, should read TTT.

Line 150: If authors use four-digit as HHHH, “000 UTC” should be “0000 UTC”.

Corrected.

Line 169: Please confirm the wavelength of AERONET dataset. If 500 nm, this is slightly different to calculated AOT by Meso-NH. How can we understand the difference of wavelength in the comparison between calculation and observation?

We indeed use AERONET AOT data at 500 m. There are countless examples in the literature of AOT comparison at different, but close wavelength, as is the case here. Nevertheless we can make a back of the envelop estimate of the error/uncertainty associated with the wavelength difference, based on the fact that the extinction (and hence AOT) varies with wavelength according to a power law, Ext_550/Ext_500=(550/500)^a, with a~1 between 450 and 660 nm as assessed by Denjean et al. (2020) for elevated BBA coming from Central Africa. As a result, the error/uncertainty induced by the difference of wavelength is on the order of 10%. Moreover, Gaetani et al. (2021, see Figure 3) compared the daily AOT at AERONET stations with the CAMS product at 550 nm on a several year span in southern Africa (2003-2017) and found a robust linear relationship between the ln(AOT) at 500 and 550 nm, indicating that the AOT at 550 nm can be used a reliable proxy for the AOT at 500 nm. This is now discussed in the caption of Figure 8, where the first comparison between the AERONET AOT at 500 nm and CAMS and MODIS products at 550 nm is made:

“A robust linear relationship is observed between the natural logarithm of the AOT at 500 and 550 nm (see Denjean et al., 2020; and Gaetani et al., 2021), therefore CAMS and MODIS products at 550 nm can be used as reliable proxies for the AOT at 500 nm at the AERONET stations”.

Denjean, C., Brito, J., Libois, Q., Mallet, M., Bourrianne, T., Burnet, F., et al. (2020). Unexpected biomass burning aerosol absorption enhancement explained by black carbon mixing state. Geophysical Research Letters, 47, e2020GL089055. https://doi.org/10.1029/2020GL089055

Gaetani, M., Pohl, B., Alvarez Castro, M. C., Flamant, C., and Formenti, P.: A weather regime characterisation of winter biomass aerosol transport from southern Africa, Atmos. Chem. Phys., 21**,** 16575-16591, https://doi.org/10.5194/acp-21-16575-2021, 2021.

Line 389: Does “morning” mean small distance because this flight started from 0736 UTC? Sometimes it is ambiguous to use time using this Figure 9, so it would be better to add another x-axis represented by time.

Agreed, this is ambiguous indeed as the whole flight takes place from 0736 to 1014 UTC. We have modified the sentence as: “On 5 September, the BBA layer is observed…”

Line 571-572: I understand that Figure 15 summarizes and illustrated the finding in this study. However, this should be fully discussed before the conclusion section. Please move these discussions related to Figure 15 into Section 5.

Thanks for the suggestion. However, we have decided to keep the Figure where it was, as Section 5 is not a summary section, unlike Section 6. Nevertheless, to comply with the reviewer’s comment we have renamed Section 6 as ‘Summary and conclusions”.

Figure 2: The characters for mean sea pressure level is small. Please enlarge, or use color-scale to distinguish them.

Readability of Figure 2 has been improved.

Figures 3 and 4: The expression of data stated in the caption should be unified through manuscript. “1 September” is used in Figure 2, but “01/09” is used in Figure 3. These are hard to read.

OK, agreed. Caption have been homogenized through the manuscript as xx September, and hence modified in the caption of Figures 3 and 4. The captions now read:

Figure 6: There is no indexes to represent (a) to (f) within this figure. In the caption, MODIS is repeated. Please rephrase.

Figure corrected. The caption is now corrected

Figure 8: Please enhance the black color. This seems as gray color and hard to read.

The contrast is fine in the ps and pdf format of the figure.

Figures 10 and 12: Because four panels are not unified as altitude levels, it is hard to follow the meaning of this figure. It is much straightforward to align as 1-2, 2-3, 4-5, 6-7 km (or vice versa). Please reconsider the order of panel and also rearrange the discussion main text.

The logic behind the ordering of the sub-figures was more to have the upper level trajectories on the top panels and the lower level trajectories in the bottom panels.

Figure 13: There is no indexes to represent (a) to (d) within this figure. Please move the panel of vertical velocity because only this panel is slightly differently positioned.

Labels have been added and panel positions corrected.

Figure 14: What is the contours level for liquid water (blue) and potential vorticity (brown)?

We have added information in the revised MS. The caption of Figure 14 now includes the information as: "Blue contours represent liquid water every 0.1 g kg^{-1} while brown contours represent potential vorticity every -1.5 PVU (potential vorticity unit, PVU = 10^{-6} K kg^{-1} m^2 s^{-1})."