

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-939

Anonymous Referee #2

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Referee comment on "Smoke in the river: an Aerosols, Radiation and Clouds in southern Africa (AEROCLO-sA) case study" by Cyrille Flamant et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-939-RC2>, 2022

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General comment:

The manuscript entitled "Smoke in the river: an AEROCLO-sA case study" written by Cyrille Flamant 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?
- 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?
- 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?

- 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.
- 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?
- 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.

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-NH to represent the behavior of black carbon.

Specific comments:

- Line 24: This is not consistent to line 81. Please confirm.
- 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?
- Line 108: There is no definition for "TTL". Please clarify.
- Line 150: If authors use four-digit as HHHH, "000 UTC" should be "0000 UTC".
- 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?
- 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.
- 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.
- Figure 2: The characters for mean sea pressure level is small. Please enlarge, or use color-scale to distinguish them.
- 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.

- Figure 6: There is no indexes to represent (a) to (f) within this figure. In the caption, MODIS is repeated. Please rephrase.
- Figure 8: Please enhance the black color. This seems as gray color and hard to read.
- 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.
- 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.
- Figure 14: What is the contours level for liquid water (blue) and potential vorticity (brown)?