Review of "The radiative impact of biomass burning aerosols on dust emissions over Namibia and the long-range transport of smoke observed during AEROCLO-sA" by Cyrille Flamant et al.

This study investigated the radiative impact of biomass burning aerosols on local and regional atmospheric circulation patterns using a mesoscale non-hydrostatic model. The authors first concluded that their Meso-NH simulations can reasonably reproduce key features of dynamics, thermodynamics and atmospheric composition fields as seen in in-situ and remote sensing observations during a dust emission episode on Sept. 5, 2017. By comparing simulations with and without BBA radiative effects, the authors found that omitting the radiative effects of BBA results in weaker AEJ-S, weaker LLJ, deeper convective PBL, and an eastward shift of the smoke plume.

I found the methodology and the scientific conclusions of this study sound and solid. This study is no doubt publishable with profound implications for air quality and dust cycle in the southern hemisphere. The manuscript is in general well written and easy to follow, however, I do find places where lengthy sentences demand several readthroughs to understand. I believe the readers would appreciate some extra work on these lengthy sentences.

I recommend publish after minor revision.

General comments:

First, I am impressed by the capability of Meso-NH in reproducing BBA spatiotemporal distributions and atmospheric circulation patterns during the 5-day time period. I have a few questions regarding the setup of the simulations:

> 1) is there any nudging or re-initialization of the simulation based on ERA5 during the course of the simulation? Is the model free-running for the whole time period since initialization?
>
> 2) what is the rationale to create ensemble members by shifting initialization time? have you considered perturbing parameters related to BBA, e.g., SSA?
>
> 3) what exactly is the vertical resolution? And where is the top of your domain (i.e., how do you define "*600m in the free troposphere*")?
>
> 4) how is "NORAD" achieved? by turning off SW calculations related to BBA in RRTM? How is the semi-direct effect (those related to cloud adjustments) excluded?

I think these details could be added to the main text to help readers better understand the setup of the modeling framework.

Second, I strongly recommend adding a schematic diagram to the manuscript, illustrating the complex pathways through which radiative effects of BBA modulate local and regional atmospheric circulation patterns and the distribution of smoke and dust emissions in southern Africa. When I read through section 6, I had to constantly flip through figures to build such a

mechanistic diagram in my mind, I believe such a diagram in the main text would benefit the manuscript in profound ways.

Other than these two points, I only have a few minor comments and suggestions that I encourage the authors to consider while revising the manuscript.

Minor comments/suggestions:

Line 34-39, additional reference for the importance of BBA direct and semi-direct effects: Diamond et al. (2022) pointed out the critical role of accounting for smoke diabatic heating that reduces the free-tropospheric subsidence in reproducing the observed low-cloud cover over the SE Atlantic with a regional model.

Section 4.1, Zhang & Zuidema (2021) also documented the important role of synoptic patterns in governing smoke transport over the oceanic region during the month of September, such that a mid-latitude intrusion pattern (their Fig. 8c, very similar to Fig. 2b in this manuscript) is often associated with less smoky conditions in the remote SE Atlantic, consistent with reduced transport of smoke by a weaker AEJ-S. They show that this synoptic pattern also strongly affects the extent of the marine stratocumulus deck.

Fig. 2, please indicate what "e" and "w" represent in the caption, please check the orientation of a)-d) in this figure.

Fig. 10, there is no blue dotted line on the figure, you meant blue Xs, right?

Line 296-299, this sentence is lengthy and hard to understand, I recommend rewording.

The section titles in general seem a bit long to me, I think more concise and shorter section titles would read and look better.

Line 311, not sure if "*realistic*" is the right word here, I am convinced that the model captured the key features, but I feel "*realistic*" is perhaps too strong here.

Line 330-331, not clear which simulation set you're referring to. "…*is enhanced*" in which simulation? "*... associated low-level dynamics*" could you be more specific? Weaker or stronger LLJ?

Line 355-358, I do not see the temporal shift in Fig. 10f and h, to me, the orange and green curves pretty much track each other at Upington.

Line 371-374, not clear, please indicate "enhanced upward motion and drier mid-troposphere" in which simulation set? The cloud fraction increase is also not clear to me, I see both +ve and -ve cloud fraction changes, are you referring to the signal around 15 E or 25 E?

Line 375-376, my interpretation of the above sentence and Fig. 12f is that upward motion in BBRAD is enhanced due to differential warming, but here, you indicate NORAD has enhanced

vertical motion. I am confused. Also, could you be more specific about how the strength of updraft (vertical motion), LLJ and surface wind is dynamically related? (again, a schematic would help a lot).

References

Diamond, M. S., Saide, P. E., Zuidema, P., Ackerman, A. S., Doherty, S. J., Fridlind, A. M., Gordon, H., Howes, C., Kazil, J., Yamaguchi, T., Zhang, J., Feingold, G., and Wood, R.: Cloud adjustments from large-scale smoke–circulation interactions strongly modulate the southeastern Atlantic stratocumulus-to-cumulus transition, Atmos. Chem. Phys., 22, 12113–12151, https://doi.org/10.5194/acp-22-12113-2022, 2022.

Zhang, J. and Zuidema, P.: Sunlight-absorbing aerosol amplifies the seasonal cycle in low-cloud fraction over the southeast Atlantic, Atmos. Chem. Phys., 21, 11179–11199, https://doi.org/10.5194/acp-21-11179-2021, 2021.