Review of "Acceleration of the southern African easterly jet driven by radiative effect of biomass burning aerosols and its impact on transport during AEROCLO-sA" by Chaboureau et al.

This study uses a meso-scale model at convection-permitting scales to analyze the impact of aerosol-radiation interactions on simulated large-scale motions with a focus on African Easterly Jet-South (AEJ-S). The authors also use the same simulations to estimate the direct and semi-direct effects of aerosols over the southern African biomass burning and outflow region. Even though there is no lack of motivation for such a study and its usefulness for the modeling community in improving (or finding consensus) in estimating the impact of biomass burning aerosols (BBA) on radiation budget, clouds, and circulation/transport over this region. Especially since the vertical distribution of aerosols after long-range transport have been found to be lacking in many global models for this region.

However, there are serious concerns regarding the modeling approach and the use of over-simplistic assumptions regarding aerosol properties in the model, as mentioned below under my major comments. There is a need for some substantial modeling work to be redone/added before one can infer anything meaningful from the results and analysis presented in their Section 4 and beyond. I do not recommend the publication of the manuscript in its current form but can be reconsidered for review once additional modeling work is performed and analysis is redone.

Major comments:

When performing forecast runs or free-running simulations, it becomes necessary to perform ensemble runs (with larger number of ensemble members for shorter forecast periods) to understand the sensitivity of the model to the initial conditions in simulating/predicting the thermodynamic, wind and other meteorological fields. It is imperative to judge that how significant are the changes in wind speeds, temperature, cloud cover etc. (using a t-test for example) that the authors report between the two simulations versus the model internal variability.

Is the model assumption of SSA=0.85 at 532 nm? It would be worth demonstrating the assumed SSA curve for rest of the solar spectrum (or at least ~400-1000 nm, where absorption due to biomass burning and dust are most relevant). The model assumed SSA value is representative of strongly absorbing aerosols, possibly for the more aged smoke that was observed over the Ascension or during ORACLES. However, there is quite some variability of SSA over the continent (which is also part of the modeling domain), see Figure 11 of Piston et al. (2019). Therefore, I wonder if this strong heating (with SSA=0.85 everywhere) over the continent is exaggerating the changes in large-scale motions. The sensitivity of the model results to different SSA assumptions becomes critical here and should be discussed.

Specific comments:

Lines 27-33: There are several noteworthy references for modeling semi-direct effect over SE Atlantic in recent years, especially for the similar modeling scales authors are looking at. For example, Lu et al. (2018), Gordon et al. (2018) and Das et al. (2020). These are worth citing in this section along with their major findings in relation to direct and semi-direct effect and how that compares to current study.

Section 3.1, Figure 2: Why is the spin-up time for aerosols considered within the 16d forecast period? A better or more common approach is to spin-up the aerosols for at least a 7–10-day period before the forecast, while the meteorology can be reinitialized using the ECMWF forecast from the starting point of forecast. This way the complete 16-day period can be used for analysis, which is what would make more sense to be able to make anything of the direct and semi-direct effect estimates presented later.

Further, not only AOD, but it would also be worth comparing the model SSA with retrievals from AERONET over these sites, including Mongu and Namibe sites. One would have assumed to include the two sites at the first place since they are the long-term sites over this region, with Mongu being a prime BB source example and Namibe has some considerable dust influence, which would be good to see provided the model includes dust as well. The resulting SSA (of the aerosol mixture, BBA + Dust) is important to constrain here since not only loading, but absorption/scattering will control the aerosol radiative impact and its feedback on the circulation.

Fig 3. Difference plots here would be useful for each row. Also, quantitative difference in windspeeds would be useful to demonstrate rather than qualitative description since the focus is on changes in AEJ-S based on the manuscript title/objectives.
Section 4.1. How do the DRE and radiative heating compare to other studies in literature? Authors just mention comparison to Mallet et al. currently. Also, 3-4 times higher values of DRE and radiative heating could also be because of higher model AOD magnitudes compared to MODIS and strongly absorbing SSA assumption. The seasonal mean versus September mean reasoning alone cannot explain such large differences.

Fig. 8: This figure is trying to present too much information in single panel (especially, c-d and e-f). Also, why meridional cross-sections are shown for this analysis when the AEJ-S flow is zonal? It would make more sense to show contours/cross-sections in the direction of plume transport (i.e., along longitude), starting from land and extending up to the ocean rather than meridional cross-sections over two narrow strips.

Line 375 and 382: The use of words like “first semi-direct effect” and “another radiative semi-direct effect” is misleading or inaccurate in these sentences. There is a whole lot of literature and history on how direct, semi-direct and indirect effect of aerosols have been defined. These effects have traditionally been defined with respect to the impact of aerosols on clouds and radiation balance. Unless authors are redefining what encompasses “semi-direct effect”, they should refrain from using “semi-direct effect” arbitrarily or when implying the aerosol heating feedback on circulation rather.

Overall, it is hard to make anything of the changes discussed in 4.3 and beyond unless it is a comparison between model ensemble-means. It would benefit to show difference plots between your BBRAD and NORAD set of simulations for most of your variables (cloud fractions, wind speeds, temperature etc.) with significant changes depicted with hatching/shading. For example, change in surface temperature by 3K and average heating rates by 1K day^{-1} (line 371) are too large and one would wonder if it's because of coarse representation of BBA in the model or due to the very short period of analysis with a single member run during which most fields have not stabilized in the model.

Comment on Data availability: The modeling results, at least the ones used for demonstrating the analysis presented within the paper should be uploaded and published using an appropriate data repository. The sentence stating, "The Meso-NH-derived fields and back trajectories data can be obtained upon request to the corresponding author of the paper" does not suffice based on current ACP guidelines on data policy.

References:

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