Review of Mallet et al, 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.

Two atmospheric model simulations of direct and semi-direct effects of biomass burning aerosols over the south-east Atlantic ocean are compared. Simulations are performed over long enough periods to be climatically interesting. The main differences between models are that ALADIN uses prescribed SST and 12km horizontal resolution, RegCM does not and has 80km resolution. Both models use single-moment aerosol microphysics and neither represents indirect effects of aerosols on clouds. The model evaluation is excellent, with maximum use being made of the latest satellite products and detailed insights noted on the differences between various observational products.

Biomass burning aerosol causes significant shortwave heating, consistently with other studies. The models significantly underestimate cloud fraction. This underestimate means they will be interesting to compare with other models that overestimate cloud fraction (eg Unified Model, Gordon *et al* 2018), but they get LWP about right – suggesting the clouds must also be too optically thick.

I think the topic is important, the analysis is sound and the paper is very well-written. It is well suited for publication in ACP and should be highly cited. My suggestions and comments, overall, are minor.

Scientific comments

Can you speculate further about why ALADIN-Climat underestimates the cloud fraction? Currently saying 'cloudiness, precipitation or boundary layer scheme' is a bit vague, though I appreciate the simulations are expensive and it may not be possible to do diagnostic sensitivity studies.

L340 can you dig a bit deeper into the huge difference between MODIS and MISR AOD? Could it be due to cloud masking? Maybe reference papers where the two retrievals have been compared to AERONET?

Line 549- what about changes in inversion/cloud top height with smoke aerosol? Are there any such changes? Lines 568-580 say subsidence is reduced by smoke and tropospheric stability is decreased, but perhaps you can spell this out a bit more? Dipole patterns in Figures 12 (bottom-right) and S4 suggest cloud height changes might be occurring.

One other thing missing from the analysis is an evaluation of free-tropospheric (and boundary layer) relative humidity. Do the models replicate the observed increases in RH associated with, or coincident with, smoke layers?

Finally, I was expecting to see a discussion of the net semi-direct radiative effect of the BBA in Wm^{-2}. If the DRE is $(F\text{-}F_{clean})_{SMK}\text{-}(F\text{-}F_{clean})_{CTL}$, as per Ghan (2013), can the SDRE be calculated in these simulations as $F_{clean,CTL}\text{-}F_{clean,SMK}$? (not sure I got the sign right, but you get the idea). We would presumably expect RegCM to show a negative SDRE as Figure 11 shows an increase in cloud fraction (albeit perhaps not statistically significant) and Figure 12 increases in LWC, while ALADIN will have a positive SDRE in some regions.

Text comments and suggestions

Line 300: missing "such" in "such as"?

The text says both models are "driven" by ERA-interim reanalysis – does that mean the boundary conditions are derived from ERA-interim or is there nudging of horizontal winds?

How do the two models handle sub-grid cloud?

What does MACv2 do for the cloud fraction/water path? Or what is used for the cloud fraction in the calculation to get the DRE from MACv2 on the right of Figure 8?

Models assume external mixing of aerosols. Both represent fresh and aged smoke, but there is not a separation between BC and OC. This is interesting and complementary to other models. Both use scaled GFED emissions (could additionally cite https://www.atmos-chem-phys.net/20/969/2020/ to justify this). Which version of GFED?

Line 460 When reading this, at first I got slightly confused between decreases in cloud fraction in SMK compared to CTL (which are irrelevant here) and decreases in cloud fraction from south to north, which is what you are talking about. Maybe rephrase to "decrease in low cloud fraction with latitude as one moves northwards from 5S" or similar.

Line 495 CTRL->CTL.

Line 668 would be good to put some numbers on the TOA DRE as you do for the surface DRE.

Figure 1 would be helpful to reference Klein and Hartmann in the caption, as in Figure 2.
Figure 2 can yellow be replaced by orange for RegCM_CTL, so it doesn't fade into the background?
Figure 2b do you show grid-box-average or in-cloud LWP?
Figure 3 specify that (if I am correct) this is MODIS standard AOD, not MODIS ACAOD.
Figure 4 can MISR be added? Again, specify which MODIS retrieval is used in the caption.
Figure 11e should there be some dashes, or are the cloud fraction changes nowhere statistically significant? If no changes are statistically significant it would be good to clarify that in the caption.
Figure 13 please specify what the contours are.

Can the two figures in S2 be put on the same color scale?

Caption of figure S3 – is it extinction, or change in extinction between SMK and CTL? Please spell this out