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Comment on acp-2022-411

Anonymous Referee #2

Referee comment on "Cloud adjustments from large-scale smoke–circulation interactions strongly modulate the southeastern Atlantic stratocumulus-to-cumulus transition" by Michael S. Diamond et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-411-RC2, 2022

This study provides a thorough investigation into the role of biomass burning smoke on the transition of marine stratocumulus-to-cumulus over the SE Atlantic. The authors use two modelling frameworks to provide information on both large-scale and the cloud-scale impacts; this is an excellent approach and has provided some unique insight into the primary pathways through which the smoke interacts with the clouds over the region. I enjoyed reading this study, and appreciate the thoroughness that the authors have applied to the analysis. I recommend this for publication but have some largely minor comments that I would like the authors to address first.

Major comments

Representativeness of simulated conditions. The SAM simulation AllOff_N/2 shows that if the FT aerosol concentrations are halved then the baseline cloud evolution is very different. This would imply that the response of the cloud to smoke is possibly sensitive to the baseline. The low cloud fraction comparison between SEVIRI and WRF over the domain (Figure S5) suggests to me that the observed SCT is better reproduced assuming a cleaner FT – is this true? If so, then this would suggest the bulk of results and conclusions of the paper are not representative of the region. To an extent the authors have acknowledged this on lines 993-995, but it would be interesting to hear how confident the authors are that the SAM simulations are representative of the mean state of the region.

Enhanced moisture above the inversion. I think the banding effect is a really interesting result and adds to the growing appreciation of moist layers over the region and their role in cloud evolution. The authors discuss the moisture effects etc in lines 1028 to 1041 but I feel there are remaining questions that could be answered here. As pointed out, several studies (e.g., Adebiyi et al 2015; Pistone et al 2021) have observed high moisture content in elevated plumes of smoke – do the authors believe that the WRF simulation accurately reproduces the degree of moisture enhancement in the plume? If not, what impact do the authors think this would have on the results? Would we expect to see a stronger or buffered cloud response?

Minor comments

Line 328. Does turning off the shallow convection scheme impact the ability to reproduce the SCT?

Line 396. SAM not defined.

Line 400. ‘The LES is only nudged in the FT…’ I suggest this is moved higher up in this or the previous paragraph.

Line 408. I suggest introducing the model before the LES forcing section.

Line 443. Is this assumption appropriate? Assuming the wind-dependent source is sea salt wouldn’t you expect a substantially higher hygroscopicity?

Line 480 (and table 1). The use of ‘aerosol-radiation interactions’ and ‘smoke-radiation interactions’ is confusing. By referring to ‘aerosol-radiation interactions’ are you essentially referring to any non-smoke aerosol? Please clarify. This would avoid the confusion on line 480 which sounds like semi-direct effects in the MBL are negligible.

Line 569 (and line 752). How do the WRF simulations compare to the SEVIRI Nc maps?

Line 808. Could this be a saturation effect? If the model had more representative Na values in the MBL (Figure 19) might there be more sensitivity?