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Comment on acp-2022-411

Anonymous Referee #3

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-RC3, 2022

Review of "Cloud adjustments from large-scale smoke-circulation interactions strongly modulate the southeast Atlantic stratocumulus-to-cumulus transition" by Diamond et al.

The manuscript presents a set of model simulations that investigate the role that absorbing biomass burning aerosols play in modulating cloud development in the stratocumulus to cumulus transition zone of the SE Atlantic. The authors nicely combine different modelling approaches to examine both changes to the free-tropospheric structure and circulation that result from diabatic heating within the aerosol plume, and the cloud-scale responses that result from both these synoptical scale and microphysical perturbations. The case-study under investigation is based on, and the model results compared to, data from several airborne and surface based field experiments in the region.

I enjoyed reading the paper and felt that the analysis performed was thorough. The paper length was rather long, but I expect necessary given that experiments from two different models and observations needed to be described.

I have some minor comments that I would like the authors to consider before publication.

Main comments

- Why do the authors use different meteorological datasets to run trajectories in the boundary layer and free-troposphere in figure 3? Also, for the free-tropospheric trajectories, why was 2 km altitude chosen as a starting point i.e. above the boundary layer/middle of the aerosol plume etc. Noting that the observed aerosol plume closer to Ascension Island is at a higher altitude (Fig 2).

- WRF has a high bias in the FT aerosol number concentration, but the OC and BC mass loadings are comparable to those observed (Fig S1). Can the authors confirm that the model data in Fig 4a/Fig S1b is the accumulation mode only that this mode does represent a comparable size range as the aircraft measurement? If yes, are there significant concentrations of aerosol species in addition to OC and BC in the model that might account for the difference?
- Figure 5 uses altitude (km) and Fig 6 uses pressure (hPa) on the y-axis. Is it possible to have consistency so that features at different altitudes can be compared more easily?
- Figure 7: Can the authors comment on why the trend in cloud drop concentration in the WRF model does not follow the MBL aerosol number concentration, particularly towards the end of the trajectory? This seems counterintuitive and is different to the results from the SAM model shown in figure 15.
- Figure 8 d: "$\Theta_{l,FireOn} < \Theta_{l,FireOff}$ at plume top" is only strictly true if one considers the plume top height being fixed at the height of the FireOn simulation. Θ_l at the plume top height of the FireOff/RadOff simulations is actually cooler than the value at the plume top height of the FireOn simulation.
- Figure 14: Can the authors change the colour bar for cloud water mixing ratio, given that it uses similar colours to the cloud droplet concentration shading on which it is overlaid.
- Line 878: Consider rephrasing "the effect of subsidence exceeds that of the indirect effect", given that this is only when comparing against the baseline aerosol case. Arguably, the most dramatic impact on the cloud field does result from microphysics i.e. in the simulation that reduces the FT aerosol concentration by a factor of 2.
- Section 5: Refer the reader to Barrett et al. (2022) for a more complete description of the joint flight.
- Line 910: Can the authors also comment on possible reasons for the cool and moist bias in the MBL in the SAM simulation, when compared to the observations.
- Line 913: Although the regional simulation is free-running, I would have thought that the aerosol plume takes several days to reach Ascension Island from the source region, as can be inferred from the trajectories in figure 3. Assuming that the model was initialised with a realistic aerosol location/amount near the source region, then does this point to model errors in the aerosol transport further offshore? Have the authors made any comparisons with satellite measurements of the plume location throughout the simulation period e.g with something like CALIOP/CATS/MODIS above cloud AOD/SEVIRI above cloud AOD etc, in order to examine this?
- Line 995: It may be worth re-stating here, that the simulations that followed the classical model of an entrainment driven transition had much higher aerosol loadings in both the free troposphere and in the boundary layer than were measured by the ground site and aircraft. Suggesting that they may not be representative of typical conditions in this part of the SE Atlantic e.g. Fig 20.
- Given the above point, and that the strongest cloud response from the baseline case arguably arises from the SAM simulation that is initialized with a factor of 2 reduction in the free-tropospheric aerosol concentrations (which is also perhaps in better agreement with observations – Fig S1), I do wonder if the title of the paper should be adjusted. As it appears from the results presented, that microphysical controls can be as important in modulating the SCT as the semi-direct effects (large-scale smoke-circulation interactions) that result from heating within the free-tropospheric aerosol plume.