

Atmos. Chem. Phys. Discuss., author comment AC1
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Reply on RC1

Michael S. Diamond et al.

Author 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-AC1>, 2022

We would like to thank the reviewer, Dr. Che, for their constructive comments. Please find a detailed response that outlines our changes in response to the suggestions below.

"1. Line 124-127. Can the author explain more why precipitation scavenges can result in a lower number of larger droplets?"

This was originally phrased unclearly; clarified to read "leading to clouds with lower cloud droplet number concentrations and larger droplet sizes and thus greater precipitation".

"2. Line 141-145. The authors suggest that BB aerosol in the African region is mainly due to anthropogenic agriculture burning. However, satellite images show that most of the burning is in the Savannah region (Figure 8 in Che et al., 2022, <https://doi.org/10.5194/acp-2022-160>). Is this consistent with the authors' claim?"

Our understanding — based on the holiday effects (Earl et al., 2015; Pereira et al., 2015), daily cycle of burn intensity (Roberts et al., 2009), and long-term trends driven by land-use change (Andela et al., 2017) — is that much of the BB aerosol in the southern African region is anthropogenic in origin. We are not aware of any reason why the occurrence of fires primarily in savannah land types (as classified by the MODIS land cover product) would be inconsistent with a strong anthropogenic influence. However, as the exact breakdown between "natural" and anthropogenic fires is not directly related to our results or analysis, we've toned down some of the language that suggested anthropogenic influence was the majority of burning (which may be true, but would need to be quantified) to instead say that anthropogenic influence is large.

"3. Line 260-216. The authors used the high-resolution meteorological field from WRF-CAM to run HYSPLIT trajectories initialized from Ascension Island, but used the coarser GDAS data to run HYSPLIT trajectories at 2 km. Do different datasets have an impact on the results of backward trajectories? Why is a high-resolution weather field not chosen for 2km trajectories?"

We ran the 2 km trajectories using the system in place for in-field forecasting, whereas the back trajectory used to set up the LES case was run specially using the WRF-CAM5 wind fields. There may be slight discrepancies between the WRF-CAM5 and GDAS wind fields (although it is worth noting that WRF-CAM5 was initialized with NCEP reanalysis

fields that should be consistent with GDAS); however, as we are only using the 15 August 2017 data as a loose constraint with a relatively large tolerance for "matches" (within a degree of longitude on either side of 5° E from 07:00 to 18:00 UTC), we doubt that the greater precision from using the WRF-CAM5 wind fields would have any bearing on our conclusions.

"4. Line 636-638. The moist FT is considered to be due to ex-CBL. Is it possible that it is also due to BB aerosol heating of the FT, resulting in enhanced evaporation of the underlying cloud droplets?"

Because the "ex-CBL" air contains significant moisture in all cases (FireOn, FireOff, and RadOff), and two of those cases do not contain any FT heating due to smoke, we are confident that the moisture is not primarily a semi-direct effect. There is some apparent enhancement of moisture in FireOn above that of FireOff and RadOff, which could plausibly represent an absorption effect. Enhanced evaporation of underlying cloud droplets cannot explain this difference, however, both because we are using total water mixing ratio (so the partitioning between vapor and liquid water is irrelevant) and because FT air is entrained into the growing MBL but detrainment of MBL air into the FT is minimal, and certainly negligible at altitudes of ~2-3 km.

"5. Line 846-847. The TKE is similar for all cases, but why does Toff have a higher boundary layer height and Woff have a lower one than AllOn? Can the author explain more?"

We argue that the weaker inversion in TOff (greater subsidence in WOff) primarily explains the higher (lower) boundary layer heights as compared to AllOn even though TKE values are similar. We have rearranged portions of the text to clarify.

"TOff grows more rapidly than AllOn (Figure 16b), despite similar MBL turbulence (Figure 16h) and updraft speeds, due to the weaker inversion during most of the simulation, which also leads to enhanced entrainment of smoke and thus greater N_c (Figure 16c)."

"The greater subsidence in WOff (Figure 6e) suppresses MBL growth by several hundred meters compared to AllOn (Figure 16b) despite similar TKE values (Figure 16h), resulting in less smoke entrainment and lower N_c as well (Figure 16c-d)."