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Interactive comment on "Impacts of Long-range Transport of Aerosols on Marine Boundary Layer Clouds in the Eastern North Atlantic" *by* Yuan Wang et al.

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In their manuscript, Wang et al. compare observed vertical profiles of aerosols at the Azores from the July 2017 flight campaign around the ACE-ENA ground site with reanalysis data from CAMS and ERA5 and with new simulations from nested WRF-AAM simulations. The WRF simulations span from "regional model" (∼20 km) to "cloud resolving" (300 m horizontal) resolution. Observed and reanalysis aerosol distributions match qualitatively. It is shown that variations in both aerosol plume bottom and cloud top height matter for whether and how much aerosol is actually entrained into the marine boundary layer. The authors conclude that MBL aerosol variations influence cloud

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properties more than free troposphere variations. I agree with this conclusion overall but have some questions about their interpretation of the idealized experiments in Figure 10. That notwithstanding, the manuscript is well-organized, clearly written, and presents interesting new results. I recommend publication following minor revisions.

Major comments:

1. It would be helpful to keep in mind that the observations only show instantaneous contact between aerosol in the free troposphere (from long-range transport) and MBL. Although the MBL concentrations seemingly are assumed to be from local sources, it is also possible that there is a contribution from FT aerosol entrained earlier and transported with the MBL flow. Instantaneous snapshots of FT aerosol-MBL top contact (or lack thereof) cannot capture the effects of previously entrained aerosol.

2. Page 14, Lines 378-389: If the CCN perturbation is being averaged between 500 m and 3 km (as stated in the Figure 10 caption), then a lot of the CCN in the elevated plume case is irrelevant to the cloud properties. The lower susceptibility values are those an artifact of averaging in aerosol that isn't doing anything to affect the clouds. This is consistent with the interpretation of different above-cloud and below-cloud susceptibility values in Diamond et al. (2018). I'm not convinced this says anything in particular about aerosol source *once that aerosol is in the MBL*. (E.g., the difference between 500 particles/mg from a local source versus the entrainment of 500 particles/mg in the FT from long-range transport, assuming the same initial MBL background concentration.) The conclusions as written strike me as being overly broad for the evidence presented.

Specific comments:

1. Page 2, Line 16: No evidence in the text is provided about the accuracy of the instrumentation, so this descriptor probably doesn't belong in the abstract.

2. Page 2, Line 26: Why "preferably"? Aerosol near the MBL top is a necessary

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condition for entrainment and thus influence on indirect effects. Perhaps an argument can be made that absorbing aerosol well-separated from the MBL could have important semi-direct effects, but that's not addressed in the paper.

3. Page 4, Line 64: There is similar work to Diamond et al. (2018) looking at several aircraft campaigns based out of California (Mardi et al., 2019). This may be worth mentioning as the aerosol concentrations typical of the Azores are likely more similar to the northeast Pacific than to the southeast Atlantic during seasons with extensive biomass burning aerosol plumes.

Mardi, A. H., Dadashazar, H., MacDonald, A. B., Braun, R. A., Crosbie, E., Coggon, M. M., et al. (2019). Effects of Biomass Burning on Stratocumulus Droplet Characteristics, Drizzle Rate, and Composition. Journal of Geophysical Research: Atmospheres, 124.

4. Page 4, Lines 69-70: There are two distinct issues that may be getting blurred here: satellites missing thin aerosol layers (what is addressed explicitly) and satellites saturating and underestimating the extent of thick layers (e.g., Rajapakshe et al., 2017).

Rajapakshe, C., Zhang, Z., Yorks, J. E., Yu, H., Tan, Q., Meyer, K., et al. (2017). Seasonally transported aerosol layers over southeast Atlantic are closer to underlying clouds than previously reported. Geophysical Research Letters, 44(11), 5818-5825.

5. Page 5, Line 104: Is there any relevant literature you can cite for the accuracy of the measurements during the campaign? They are used as "truth" and not evaluated directly in the present paper.

6. Page 6, Line 141: There is no supporting information I could find. Did you mean to reference the map in Figure 5 here?

7. Page 11, Line 308: You may want to consider adding an in-text or supporting information figure here showcasing the improvement when using the outmost domain. This discussion seems very useful for others interested in performing similar modeling work and could probably be highlighted a bit more.

8. Page 12, Lines 331-333: I would urge some caution in the comparison with Painemal et al. (2014), as that paper's results may have been influenced by the low bias in CALIOP-derived plume base altitude discussed earlier (Rajapakshe et al., 2017) and the authors do discuss this issue as well.

9. Page 13, Line 347: Where, vertically, is the CCN that is being quantified here? I was interpreting this as an MBL average, but it would be helpful to be explicit here.

10. Page 15, Line 415: Isn't it the bottom of the FT plume, not its "center", that should matter for the discussion here? One can easily imagine a very thick plume (like in the southeast Atlantic) that interacts with the cloud top significant but is "centered" at much higher altitude.

11. Figure 9: The caption does not seem to describe the entire figure. It should more fully explain the differences between the two columns.

Technical comments:

1. Page 5, Line 124: "Were" instead of "are"? I believe you are referring to the previously published results of Wu et al. (2020) to justify the assertion here, but the current phrasing makes it sound like this work is performed in the present paper.

2. Page 8, Line 206: "However" instead of "either"? I don't understand what the "either" would be referring to.

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