

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2021-693-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2021-693

Anonymous Referee #2

Referee comment on "Weakening of tropical sea breeze convective systems through interactions of aerosol, radiation, and soil moisture" by J. Minnie Park and Susan C. van den Heever, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-693-RC2>, 2021

Overview

This study performs 2 large, idealized simulation ensembles of sea breeze convection evolution covering a range of atmospheric and surface parameters, 1 with low aerosol loading and the other with high aerosol loading, with a statistical emulator used to fill in sensitivities across a greater range of conditions. The authors find that increased aerosol loading via reduction of incoming shortwave radiation inhibits surface fluxes and the land-sea thermal contrast that drives the daytime sea breeze. This thus contributes to weaker convection along the sea breeze front, particularly for warm clouds and through suppression of deep convection initiation. Once deep convection initiates, aerosol effects on convective updrafts are modulated by other atmospheric conditions. Under all conditions, increased aerosol loading suppresses precipitation with a magnitude that is modulated by other atmospheric conditions. It is further found that soil saturation fraction is the most important modulator of updraft velocity variance in cumulus clouds ahead of the sea breeze of the parameters tested.

Overall, this is a well written study, although it could be more succinct in some places that I note in the detailed comments. Although the importance of aerosol direct effects on atmospheric thermodynamic structure and clouds is known, it has largely been ignored in recent studies focusing on microphysical impacts from aerosols. This study connects these two pieces over a wide range of low-level thermodynamic states, providing a nice addition to literature in this area. I have several "major" concerns regarding choices and interpretations of some analyses. In addition, analyses mix shallow and deep convection together, but they are quite different dynamically and microphysically (e.g., shallow clouds not necessarily being buoyancy driven and not containing ice) and thus have some differences in key environmental influences on them. Therefore, grouping them together is confusing and perhaps also misleading in terms of several interpretations of analyses. Lastly, more caveats should be discussed such that results can be more appropriately interpreted within the context of real-world convective clouds. More details are provided in the specific comments below. All should be mostly straightforward to address.

Major Comments

- More specific language can be used in places that avoids over-generalized conclusions from the analyses. In particular, the several parameters varied are limited to the surface through boundary layer top inversion layer. However, as mentioned in the introduction, there can be substantial sensitivities to vertical wind shear and free tropospheric RH, so the results cannot be generalized to sea breeze environments in general. In addition, there is no microscale and mesoscale variability in surface conditions, thermodynamics, or circulations that are environmental conditions affecting cloud evolution, but these not considered in the idealized setup. Of course, this is perfectly fine and not everything can be covered in a single study, but then the title and results would be more accurate by referring to low level thermodynamic impacts rather than environmental impacts in general.
- More caveats to better contextualize conclusions are needed. Here are some examples:
 - The horizontal grid spacing at 1 km is too coarse to resolve the primary convective updrafts in deep convection, let alone cumulus clouds.
 - RH is always relatively high in the boundary layer. Does this contribute to updrafts intensifying as SHF increases and LHF decreases? In other words, are there potentially conditions in which increasing LHF with equal decreases in SHF will increase updrafts?
 - Some past studies have shown highly non-linear effects of aerosols on clouds, but that cannot be assessed with only 2 different aerosol concentrations. How does that affect the robustness of conclusions?
- The discussion around the 3 ingredients for moist convection is confusing since they apply to deep convection rather than shallow convection and the plots are not actually of the 3 ingredients but of variables that are related in some way.
 - First, the changes in moisture, instability, and lift can be more clearly shown. For example, why not show boundary layer mixing ratio differences ahead of the sea breeze line?
 - For instability, Doswell (1987) explicitly refers to “conditional instability” in relation to deep convection. This is not a function of boundary layer depth as is implied but instead a function of free tropospheric lapse rates, as is stated in Doswell (1987). Therefore, a better measure of instability is CAPE than boundary layer depth. Granted, this also assumes that the cumulus clouds forming are buoyancy driven rather than mechanically driven (e.g., intrusion of a saturated rising boundary layer thermal into an inversion layer), which is likely not the case in most scenarios based on Figure 8. In addition, Doswell (1987) discusses deep convection and says nothing of ingredients for shallow convection. The mixture of these 2 types of clouds with many of the setups having no CAPE such that shallow clouds are simply an extension of boundary layer dry thermals rather than buoyant clouds like deep convection leads to some confusing messaging.
 - And then for lift, a strong sea breeze will push further inland, but this doesn’t say anything about the depth of the sea breeze, which may be more relevant to the lift at the cloud level such that the cloud can penetrate through a deeper layer. I wonder if this can be greater in weaker sea breeze conditions if the sea breeze convergence is deeper from the front being less sloped and better balanced with zonal winds. Would it be better to examine vertical motion along the front?
- Some figures can be improved.
 - It’s not clear what Figure 6a is showing. Is it the percentage of cloudy columns that have cloud tops < 4 km AGL? In other words, is this ignoring non-cloudy columns such that it is not a cloud fraction? Also, why not have the y-axis extend down to 0% so that the fraction in all simulations can be quantified? The statement on lines 317-318 that most simulations only have low clouds isn’t clear from Figure 6a-b in

which most simulations don't even appear to have a percentage value or difference between ensembles. In addition, Figure 6b isn't referred to in the text, so is it needed?

- For Figure 7, are updrafts any grid points > 1 m/s or is a condensate constraint applied to ensure that they are cloudy updrafts? It would make sense to confine these to clouds given the focus of the paper. It's also not necessary to show some panels like the 25th, 75th and 95th percentiles that don't provide any additional information beyond what can be concluded in the other panels.
- Figure 11: I suggest brainstorming a way to reduce the panels in this figure in some summarizing way that supports the primary conclusions summarized in the text. A reader cannot be expected to comprehend the takeaway messages from 36 panels with 8 different lines and 2 different symbols nor will they likely try.
- Some conclusions are questionable, and at the very least, should be softened.
 - Lines 474-475: I'm not sure it can be concluded that the environment modulates the cold rain response to aerosol loading with so few ensemble members producing ice. If you were to impose some white noise thermodynamic perturbations, would similar effects in magnitude be seen? In other words, are differences for any 1 ensemble member robust? That isn't shown, so I think the most that can be claimed is that the responses of deep convection to aerosol concentration across different low level thermodynamic conditions are uncertain rather than claiming that they are robustly modulated by environment.
 - Comparing tests 75 and 110 is really comparing apples to oranges, so to speak, and is an example of confusing messaging due to mixing deep convection with shallow convection. Test 75 is a deep convective case with a lot of cold phase precipitation. Because cold phase precipitation will form in this case regardless of aerosol concentration, the aerosol effect on precipitation may be reduced relative to a case with only shallow, warm clouds like test 110 because the CCN concentration directly affects liquid hydrometeors but indirectly affects ice hydrometeors. With conditions being suitable for deep convection in one test and not in the other, the sensitivities of precipitation to surface or boundary layer thermodynamic conditions cannot be expected to be robust or interpretable. Because of this, deep convection with ice and shallow convection without ice should probably be separated in analyses for easier interpretation (which may help with connections to the 3 ingredients discussion as well that becomes confusing with shallow and deep clouds combined). In addition, it seems clear that increase boundary layer temperature decreases precipitation differences in Figure 12, which is likely related to liquid water path increases for the shallow convection, but the sensitivity to RH and saturation fraction are lost on me in this figure such that they don't seem robust, and I'm not sure the discussion of select cases in the text matters if there isn't a robust signal that can be seen.

Minor Comments

- The manuscript could benefit by cutting out extraneous text that is repetitious and/or distracting from the primary messages. Consider shortening the abstract and conclusions to better highlight the primary messages. In addition, consider removing the summaries in the results sub-sections.
- Lines 79-81: There are also observation (e.g., Varble 2018) and modeling (Grabowski and Morrison 2016, 2020) that show opposing conclusions and should be cited somewhere (e.g., on line 90).
- Line 86-87: Recent studies by Grabowski and Morrison (2016, 2020) that preceded Marinescu et al. (2021) and Igel and van den Heever (2021) should be cited here as

suggesting that warm phase invigoration is well founded but cold phase invigoration is not.

- Is inversion layer strength the lower tropospheric stability, the estimated inversion strength, or some other metric? Can that be clarified?
- Line 269: What “instability” is being referred to here? If it is CAPE, then it can be influenced by mixed layer depth but isn’t necessarily and certainly isn’t monotonically related.
- Line 272: Is the implication here that there is a separate lifting mechanism beyond convergence along the sea breeze front? If so, please clarify.
- Lines 351-352: What is the difference between “convective instability” for sea breeze initiated convection and “thermal buoyancy” for convection ahead of the sea breeze? Also, are you sure that convective clouds ahead of the sea breeze are in fact always buoyant and instead not at times just negatively buoyant saturated tops of boundary layer dry thermals decelerating in a stable layer? Figure 8 shows that most shallow cloud situations have no mixed layer CAPE.
- Line 522: Missing “conditions” at end of sentence.

References

Grabowski, W. W., and Morrison, H. (2016). Untangling Microphysical Impacts on Deep Convection Applying a Novel Modeling Methodology. Part II: Double-Moment Microphysics. *Journal of the Atmospheric Sciences* 73, 9, 3749-3770, <https://doi.org/10.1175/JAS-D-15-0367.1>.

Grabowski, W. W., and Morrison, H. (2020). Do Ultrafine Cloud Condensation Nuclei Invigorate Deep Convection?. *Journal of the Atmospheric Sciences* 77, 7, 2567-2583, <https://doi.org/10.1175/JAS-D-20-0012.1>.

Varble, A. (2018). Erroneous Attribution of Deep Convective Invigoration to Aerosol Concentration. *Journal of the Atmospheric Sciences* 75, 4, 1351-1368, <https://doi.org/10.1175/JAS-D-17-0217.1>.