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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Urbanization-induced land and aerosol impacts on sea breeze circulation and convective precipitation" *by* Jiwen Fan et al.

Anonymous Referee #2

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The authors have performed a matrix of simulations to investigate the relative roles of urban heat island and aerosol forcing on the behavior of an observed storm in Houston. They find that both mechanisms lead to increased precipitation for a single cell on their study day. This is a valuable approach and a necessary investigation of multiple forcing mechanisms.

The authors use a single microphysical scheme in a single model, which leads to the ability to directly compare the results in the simulation matrix. The authors show that the simulations with full aerosol and land cover effects are better in some respects than those without. The aerosol effect contributes the most toward bringing the simulations closer to the observations of surface precipitation in these simulations ... and the effect is quite significant as noted on lines 345-6. The aerosol dramatically accelerates

the growth of the cloud through the mixed phase zone, consistent with prior studies investigating the aerosol invigoration hypothesis.

More generally, the single model used in this study is one among many state of the art models, and as with any parameterization must build in a set of claims about microphysical processes. The study demonstrates that the aerosol invigoration mechanism *can* be large *if* these claims are correct. I would like to see this latter point emphasized more fully. The authors should review of what may or may not be correct about the microphysical process claims embodied in the model.

The most relevant portions of the study that could use clarification are the additional aerosol process models described on lines 133-134. These are relatively new and probably are driving much of the aerosol effects in this study. What are the likely errors in process rates in these parameterizations? What are the likely errors in thermodynamic characterization? Are they as large as the run-to-run difference the authors observe?

The point is not to relitigate prior work in full, but to the extent the authors can clarify the precision of their forecasts the more value the study has in advancing our shared understanding of how to properly model mixed phase microphysics and how to accurately partition the relative roles of aerosol and thermodynamic influences on convection.

Below, I also request clarification of the meteorological setting and terminology used by the authors.

Major concerns

The authors claim SBM_anth in Zhang et al. (2020, preprint) is the same as LandAero in this study, but Figs. 7 and 5, respectively, do not match. Likewise, the associated CFADs of reflectivity do not match, though this may be due to a different analysis box size.

Compared to the observations, the CFAD shows significantly enhanced probabilities of moderate reflectivity at high altitudes (line 241). It also significantly underestimates the prevalence of 40-50 dBZ values at and above the homogeneous freezing level. What is the origin of this overestimate? One interpretation of this is that the SBM is too aggressive in the production of smaller ice crystals that grow into snow at high altitudes, at the expense of graupel. Given the authors' interest in an aerosol invigoration pathway, it seems that matching the ice population in glaciated clouds is more important than matching higher reflectivity values at low altitudes, which can result from many intermediate microphysical steps.

As the authors note on line 250, rain rates > 2 mm/hr last about 25% longer in the simulation. This is in common among all simulations, and so would seem to indicate a general over-production of precipitation duration by the SBM scheme overall. This would be consistent with an overactive snow/ice crystal microphysics scheme instead of a shorter-lived cell tied to faster-precipitating graupel. Why is the standard for "better" maximum reflectivity and areal coverage, but not duration of precipitation?

Line 88: while Chen et al. (2011) also use the term "prevailing wind," that term is misleading, especially here in a review of how land-atmosphere interactions *generally* work in Houston. The climatological meaning of prevailing wind (c.f. the AMS Glossary) is the expected value of the wind in a given location over a climatological period. For Houston in the summer, the prevailing wind is onshore, from the southeast (https://mesonet.agron.iastate.edu/sites/windrose.phtml?station=IAH&network=TX_ASOS), and that pattern persists for stations further inland. Chen's study is of an anomalous day that may be representative of bad air quality days in Houston, but it is probably not typical of summer wind patterns or sea breeze interactions in Houston. It seems the authors are concerned in this paragraph with describing the general role of Houston's urban environment in influencing the sea breeze, and so this paragraph should be revised to more accurately reflect Houston's environment.

Line 112: Neither this study nor Zhang et al. (2020) give a synoptic or mesoscale meteorological overview of this case. Where was the trailing front? How did the meteorol-

ogy, including the sea breeze, develop during the day in comparison to the expected behavior of the land and sea breeze diurnal cycle in Houston? Rosenfeld et al. (2014) is not listed in the references, and the ACPC website is not a suitable reference for long-term archival purposes.

Line 227: What is the "sea breeze wind intensity"? How was the sea breeze perturbation wind identified? This is especially important as the authors discuss Fig. 12 (lines 358 and following), where the sea breeze intensity is said to cause the transition to mixed phase cloud.

Minor concerns

Line 64: CG lightning enhancements can also be caused by the presence of tall towers (e.g., Kingfield et al., 10.1002/2017gl073449).

Line 157: not using met from domain 1? Not clear to me. Revisit after reading rest of paper. Maybe condense into a chart or truth table. I see the point that all domain 2 sims use same meteorology to control for the fact that different aerosols will affect the meteorology on domain two? But if the meteorology is processing the aerosols, how much is the spatiotemporal distribution of aerosols conditioned by the meteorology?

Line 258: The reduction is not 45% from the larger LandAero value. Perhaps the authors used the wrong denominator? Reading approximate values from the graph to confirm the visual impression,

(6.75-4.8)/6.75 = 0.29

(6.75-4.8)/4.8 = 0.41

The claim is repeated on line 449.

Line 327 and Fig. 10: Does this total water content include the vapor phase?

Line 331 and Fig. 10: The line marking the onset of the mixed phase cloud (first appearance of blue above the 0°C level) is marked too late in panel b, and perhaps a bit in panel d, making the duration of mixed phase growth artificially short.

Line 393-4: the phrasing here makes for a confusing physical explanation; “the condensate loading effect is small as a result of enhanced condensational heating” but the condensational heating is directly related to the mass condensed, which should result in more condensate loading.

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