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Hernandez-Deckers et al., Updraft dynamics and microphysics: on the added value of the cumulus thermal reference frame in simulations of aerosol-deep convection interactions

This paper uses three hours from a borderline-resolution large-eddy simulation of convection over Houston to analyze differences between results from two approaches to sampling cloud elements. The first method, called the *cloudy-updraft* method, identifies the active portions of clouds as the regions where there is coincident cloud condensate and upward motion. The second method, referred to as the *tracked thermals* method, is more complicated and uses a tracking algorithm to look for peaks in the vertical velocity field, infer the associated thermal bubbles, and then track them over time. Each method is applied to a series of simulations that differ solely by the aerosol concentration used with the cloud microphysics. The results show that the overall story about aerosol-cloud processes is similar between the two approaches, but an apparent sampling bias leads to subtle differences. The use of a range of aerosol concentrations also highlights the difficulty of separating signal from noise in aerosol-cloud analyses.

Overall, the paper is well presented—it is both organized well and the English is very clean. The science is sound and the comparison between methods is an important investigation that informs how researchers can use and intercompare results from these methods in the future. This is where the primary value lies in this paper, as the findings for the aerosol-cloud processes essentially mirror what has been found in prior studies.

This paper will be suitable for publication after addressing a small number of questions and suggestions noted below.

Specific Comments

(1) Intro. The introduction builds from the ACPC MIP in terms of indicating that the differences between models are typically larger than the sensitivity to aerosol details within a model. This makes it hard to then infer true process rates and details for use in subsequent parameterization development, etc. The argument is then made that one should disentangle the dynamical details of the deep convection from the microphysical processes. The implication is that the bubble tracking procedure achieves this. However, this misses the points that the model-to-model differences still exist, and many of the microphysical factors within the thermals rely upon the dynamics, such as the supersaturation being dependent on the vertical velocity. More importantly, if the present study were repeated using a different model, e.g., that treats autoconversion and other processes differently, the results could substantially change, such as happened between models in the MIP. There is likely a better way to frame the introduction to be more useful for the study that is presented.

(2) Even though this set of simulations is based on a realistic setup, the presentation of the results essentially assumes this is an idealized setup. There is no reference to an observation anywhere in the paper in relation to the simulation or results. This

paper would be strengthened by putting the aerosol and meteorological state in the context of reality. Is the simulation anywhere close to the observed conditions for this day? Which aerosol concentration is closest to reality?

(3) L96 Please state the model top, as this is relevant in relation to both the number of levels and the height of the convection.

(4) L109–10 I have some concern about the use of a single domain with such high resolution driven only by the FNL. Assuming the 0.5° FNL data set was used, that is roughly a factor 200 jump in grid spacings along the lateral boundaries. How much more accurate would the results be if a more traditional nested approach were used to step down to Δx=250 m from the Δx=0.5° of the FNL?

(5) L109–10 Given the non-steady-state nature of the land-sea breeze driving the convection, what is the impact of the infrequent boundary updates on the convection? The lack of nesting would likely exacerbate this issue. Please state the frequency of the boundary condition updates (likely 6 h).

(6) L124–5 The phrasing about the thermal radius was not immediately clear without referring back to the Sherwood (2013) paper for clarification. Please reword this to add a little more detail. For example, my impression the first couple times I read this sentence was that there was some sort of scaling relationship being used, which is not the case (a misconception that stuck in my head from reading too fast the first time). It is clearer now that I understand what is being done, but it took me a little digging to make myself confident I understood how the radius is calculated. Adding a little more detail will help readers out.

(7) L139–41 This sentence infers that the thermal/bubble structures identified both in this study with 250 m grid spacing and in Hernandez-Deckers and Sherwood (2016) with 65 m grid spacing are a strong function of the model numerics and not a resolved feature. Both studies are just identifying features at WRF's effective resolution—dynamical structures finer than this would tend to not be coherent. Thus, the actual behavior of the thermals in nature could be somewhat different than what is being seen in the simulations. While I realize it is not feasible to use a model domain that has converged results for the identified cloud features, one should at least note this limitation to the reader.

(8) Figs. 2 & 3 Visually the presentation of the composited thermal characteristics in Figs. 2 & 3 is quite effective in conveying how the aerosol concentrations impact the cloud state within the thermals. Having a bit more description about how the composites were constructed would help interpretation. Most importantly, what portions of the thermal lifetimes are averaged together? Would it be more useful to look at certain times within the lifecycle, such as at maximum translation velocity or at a given altitude? Otherwise, conditions with low and high cloud water concentrations are averaged together and could hide important features.

(9) Figs. 4–6 The profiles in Figs. 4–6 are presumably averaged over the three analyzed hours. Is there any evolution of the clouds during this time? For example, how far inland has the land-sea breeze moved? Does this impact the results at all?

(10) The authors speculate that the differences identified between the cloudy-updraft and tracking methods is due to sampling bias. However, this is not confirmed beyond pointing out a physically consistent argument that sounds plausible. Is there a way to alter the cloudy-updraft method to reduce the selection bias and confirm the speculation? For example, can one add an additional criterion such that the vertical velocity must be within the values that are consistent with the velocities seen in the thermals identified via tracking? Finding a way to make the comparison fairer and seeing if the differences between methods go away would greatly strengthen the paper.