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Interactive comment on "The roles of island size and orography on tropical convection and aerosol transport" *by* Stacey Kawecki and Susan van den Heever

Anonymous Referee #1

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Title: The roles of island size and orography on tropical convection and aerosol transport Authors: Kawecki and van den Heever RECOMMENDATION: Rejection (resubmission encouraged)

This paper deals with some numerical idealized simulations with the ultimate goal to study the aerosol transport near tropical islands. Using two environmental profiles typical of tropical maritime convective regimes, the role of island dimensions and mountain heights is systematically analyzed in a simplified setup, in order to identify the different circulations and their impact on convection development and aerosol transport. Although potentially interesting, the paper needs some relevant revisions, which I do not think can be performed in a short time. Thus, I recommend rejection, but I encourage resubmission after the suggested points are considered.

Major points: -The main problem I see in this paper is that the results are not related to the existing literature in the field; no attempt is made to understand the solutions within the actual theories of convectively unstable flows. Although the present study considers a 3D island (allowing the "flow around" solution), while the theory is mainly developed in a 2D setup, an effort should be made to interpret the results of this specific study in the general framework. Apart from the Froude number (whose interpretation is, however, incorrect, in my opinion; see later), no indication is provided to identify the values of the relevant control parameters (vertical wind shear, CAPE, . . .) used to initialize the experiments. Actually, the generality of the results shown here is very limited. You wrote "This finding also diverges from previous work on this topic (Wang and Sobel, 2017; Nugent et al., 2014). The differences between the results of the simulations conducted here and many of the previous findings is most likely due to the model setup, particularly the domain configuration and the island morphology", but this is difficult to evaluate this point if you do not provide information on the parameters you (and other studies) consider. Thus, I recommend to add a completely new Subsection, where the Authors analyze the existing literature and interpret their results in this framework. The study would be much more valuable after this part is included. A list of papers is provided hereafter: Flow over orography in conditionally unstable conditions: Chen, S.-H. and Y.-L. Lin, 2005a: Orographic effects on a conditionally unstable flow over an idealized three-dimensional mesoscale mountain. Meteor. Atmos. Phys., 88, 1-21. Chen, S.-H. and Y.-L. Lin, 2005b: Effects of moist Froude number and CAPE on a conditionally unstable flow over a mesoscale mountain ridge. J. Atmos. Sci., 62, 331-350. Chu, C. M., and Y.-L. Lin, 2000: Effects of orography on the generation and propagation of mesoscale convective systems in a two-dimensional conditionally unstable flow. J. Atmos. Sci., 57, 3817–3837. Miglietta, M. M., and R. Rotunno, 2009: Numerical simulations of conditionally unstable flows over a ridge. J. Atmos. Sci., 66, 1865–1885, doi:10.1175/2009JAS2902.1. Miglietta, M. M., and R. Rotunno, 2010: Numerical simulations of low-CAPE flows over a mountain ridge. J. Atmos. Sci., 67, 2391–2401, doi:10.1175/2010JAS3378.1. Miglietta, M. M., and Rotunno, R.: Numerical simulations of sheared conditionally unstable flows over a mountain ridge, J. Atmos. Sci., 71, 1747-1762, 2014; Orographic convection: diurnal heating versus mechanical forcing Banta, R.M. Daytime boundary-layer evolution over mountainous terrain. Part I: Observations of the dry circulations. Mon. Weather Rev. 1984, 112, 340–356. Demko, J.C.; Geerts, B. Boundary layer energy transport and cumulus development over a heated mountain: An observational study. Mon. Weather Rev. 2009, 137, 447–468. Yang, Y.; Chen, Y.L. Effects of terrain heights and sizes on island-scale circulations and rainfall for the island of Hawaii during HaRP. Mon. Weather Rev. 2008, 136, 120–146. Kirshbaum, D.J. On upstream blocking over heated mountain ridges. Q. J. R. Meteorol. Soc. 2017, 143, 53–68. Kirshbaum, D.J. On thermally forced circulations over heated terrain. J. Atmos. Sci. 2013, 70, 1690–1709. Kirshbaum, D. J., Adler B, Kalthoff N., Barthlott, C., and S. Serafin, 2018: Moist Orographic Convection: Physical Mechanisms and Links to Surface-Exchange Processes, Atmosphere 2018, 9, 80; doi:10.3390/atmos9030080 General theory and Froude number Smith, R.B. Hydrostatic flow over mountains. Adv. Geophys. 1989, 31, 1–41.

-Another critical point in the present study is the grid spacing of 1 km. Bryan et al. (2003) have addressed the horizontal resolution appropriate for the simulation of deep moist convection with cloud-resolving models from a turbulence perspective. A grid spacing of O(1 km) is often sufficient to simulate the basic features of deep moist convection; however, the precipitation distribution and amount show a strong sensitivity to horizontal resolution as the grid spacing is reduced to less than 1 km. Only by going down to a grid spacing of O(100m) in-cloud turbulence is allowed to occur (Petch et al., 2002) and the inertial subrange of turbulence can be resolved. The Authors admit this limitation in the Conclusions, but they should demonstrate, at least for 2 representative cases (e.g., 1 km height, 100 km island diameter, for STRONG and WEAK wind), that their resolution is sufficient to resolve these details. They must repeat the experiments with a horizontal spacing of a few hundred meters and demonstrate that their results do not change significantly using a coarser grid; REFERENCES: Bryan, G. H., J. C. Wyngaard, and J. M. Fritsch, 2003: Resolution requirements for the simulation of deep moist convection. Mon. Wea. Rev., 131, 2394–2415. Petch, J. C., A. R. Brown, and M. E. B. Gray, 2002: The impact of horizontal resolution on the simulations of convective development over land. Quart. J. Roy. Meteor. Soc., 128, 2031-2044.

- The interpretation in terms of Froude number is confusing; you wrote at Lines 227-228 that "Because of the wind shear in the simulations, only the first 30 model levels (lowest 3 km) are used in the calculation." Really, you consider only the lowest levels because these are the ones where the flow is significantly affected by the orography. Also, at Lines 228-230, you wrote "To attain one value for each simulation, the Froude number values for each model grid cell were averaged over both the lowest 30 model levels and the entire horizontal extent of the domain". However: 1) the Froude number is generally used to evaluate the interaction of the flow with the orography, so you should evaluate it on the upstream side of the mountain, and not over the whole domain; 2) if I understood correctly, you initialized the domain uniformly and the Froude number was evaluated at the initialization time, thus it should be the same everywhere in the domain; so, why averaging? 3) it is not clear to me if you calculate the Froude number level by level or in the whole 0-3 km layer (the latter is the way used in the literature). Finally, the value of 0.04 seems not consistent with the profiles you show;

-The paper appears too long and repetitive; in particular, the last session should be completely changed (e.g, Lines 615-620 and 643-665 can be easily removed). I suggest to write a short Discussion Section on the application of these results to the aerosol studies, and limit to conclusions to a few bullet points (one page would be enough).

Minor points: - Lines 147-148: in your setup, the airflow covers a longer distance over the sea in case of wider mountains; thus, your statement "each mountain has similar amounts of water vapor available for convective processes" is questionable; - Lines 163-164: how have the two profiles been chosen? Are they the result of some kind of averaging? As you address in the conclusions, using only these two profiles limits the exploration of the parameter space to a very small sub-region, but you should stress this point more in the paper; - Section 2.0: what is the frequency of the model outputs? - Line 216: you introduced the Froude number, but this is defined only later; - Lines 203-204: you wrote that the background flow is westerly and northwesterly, respectively, but you show the meridional wind in Fig. 1; I suppose that the absence of Coriolis does not affect this change, but you should be clearer on this point, as it may generate confusion; - Lines 205-219: I found that this paragraph can be strongly reduced, as the flow characteristics you describe are well known in the literature; - Lines 215, 218: Smolarkiewicz not Smolakareic!; - Lines 238-239: "Under such conditions diurnal heating can more easily influence local circulations than with stronger, more turbulent flows . . .": please, provide a reference for this; - Line 247: Emanuel instead of Emmanuel; - Lines 282-283: I think the abscissa should contain the longitudinal value, not the averaged latitudinal; - Lines 324-332 and 414-425: I suggest to summarize these results in a conceptual model, to illustrate in a new Figure, and to reduce the text; - Lines 378-379: you suggest that the convergence is the result of sea breeze; however, looking at Fig. 6, it seems that the coastline (whose contours -I suppose- are denoted with the red line) is not crossed by the wind, as it should be the case for a sea-breeze circulation; - Line 405: eastern instead of western; - Lines 433-439: it took me some time to understand the sentence, please organize it better, e.g. "When compared with the flat, 100 km, STRONG-EXP simulation, a mountain height of 2 km causes increases . . ."; - Line 448: mixing ratio is more relevant than relative humidity in order to provide more moisture; - Line 531: layer instead of level; - Line 532: in keeping . . .: what do you mean? - Figure 2: arrowheads are too small; - I suggest to superimpose Fig. 3 with Fig.2. Also, is the pattern of moist convergence different from that of (wind) convergence? It seems it is not the case. Please, comment on this; - Figure 4: what does the disturbance at around 10.30 in the lower panels represent? Noise? - Figure 6: how is the integrated condensate mixing ratio defined? - Figure 11: it may seem strange that high values can be reached in the same area at two different levels: is it a consequence of advection?

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