**Review**
**“The roles of island size and orography on tropical convection and aerosol transport”**
**Stacey Kawecki and Susan van den Heever**

The authors attempt to show how island height, island half-width, and a strong and weak wind regime affect the airflow around an isolated circular island, along with the cloud development, precipitation patterns, cold pool spreading, and aerosol distribution in idealized simulations. The RAMS model is used to simulate an island with soundings based off of soundings used in a prior study with weak and strongly sheared environments (Robinson et al. 2011). Analyses are presented of the diurnal evolution of cloud, wind, rain, cold pools, and tracers. While the authors point out shortcomings in prior studies and seek to improve on them in their own research, the study tries to cover too many things and the analyses falls flat.
Recommendation: Reject

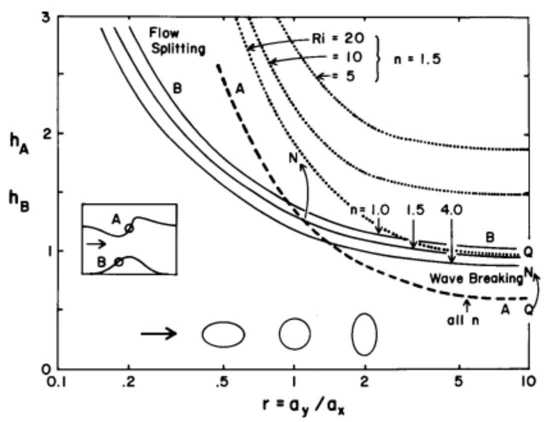
First, I recommend a deeper literature review. Multiple papers are cited incorrectly and/or in a sloppy way, and the authors are naive of prior studies highly relevant to their work. They point out shortcomings in prior studies (sometimes incorrectly), and then proceed to make their own highly simplified assumptions for their own idealized simulations without justification. The authors make little attempt to connect their work to prior literature, nor to reality. No observations are shown to justify their model setup nor their results, leaving it unclear if these simulations approach reality. This is especially important since they call their simulations realistic and typical of tropical orographic scenarios when they do not appear to be. This isn’t surprising since the soundings chosen were used in a prior study as extreme cases meant to bound conditions. The authors call their simulations weak and strong wind when both simulations are weak compared to other studies they briefly reference. In addition, the authors strongly motivate their ability to learn about aerosol transport in the coastal regions but their tracers are too simplistic, not interactive, and the results are not enlightening and not studied in depth. I recommend restructuring and narrowing the study to focus on the results that can be clearly shown, pointing out the limitations of the study earlier (for example the water vapor flux issue due to differences in low level moisture), and connecting the study motivation and results to prior literature and observations. Based on these issues, I do not feel it is appropriate in its current state for publication in ACP. However, after some significant restructuring, rewriting, and some additional research, this study could be resubmitted to hopefully add insight into orographic processes.

**Specific Points in order by Line (L) with major points in bold:**

L 48-52: Discussion of mountain wave drag seems off topic.
L 81: Wang and Sobel, 2017 were not the first to show that increasing terrain height increased precipitation amounts. This is well established in orographic precipitation literature dating back to the early 1900s. They are also not the first to show that increasing wind speed increased precipitation on the windward side.
L 85: The Smolarkiewicz 1989 paper cited should be 'Smolarkiewicz and Rotunno' not 'Smolarkiewicz et al.’
L 93-94: Nugent and Minder, 2013 does not exist as far as I can tell. If Nugent et al. 2013 is meant, I believe they used a 3-dimensional mountain.

L 97-99: Cite?
L 102-103: When “often" is used, more than one example should be provided.
L 105: Many studies do not use “circular” islands because circular islands are rather rare. If trying for realism, this isn’t the best argument.
**Following on from this, what islands are these simulations meant to mimic?**
What latitude is the island simulated at? (this matters for sun strength and angle).
**Intro: The introduction is weak. What are the open questions? How will this work contribute to the ONR work? This study appears to be significantly focused on aerosol transport in the introduction, but there is no description of past research related to this topic. Please give examples of how/why this matters, or how it is poorly forecasted currently (or better yet, leave this to a different study where this can be a more singular focus).**
Table 1: Units needed for domain size
L 148: The amount of water vapor is similar in each simulation – has this been checked?
L 154: A witch of Agnesi mountain is not recommended for idealized simulations because it takes a very long time for the mountain height to go to 0. With interactive surface conditions, this means that the result is a large flat land area surrounding the mountain in the simulations.
Table 2: Add half width to this table
**L 162: The paragraph starting at line 162 needs additional detail to fully describe the soundings chosen. In Robinson et al. (2011) they create 6 soundings, 2 low shear (over different atmospheric levels) and 2 high shear soundings along with a low RH and a high RH. They say "Each of the six soundings was formulated to have a very low or high value of some chosen index expected to affect convection.” Based on this description, these do not seem like “typical tropical maritime convective regimes”. This paper should also say where these soundings come from (Melville Island, latitude), and which soundings are being used. The soundings are labelled as strong and weak zonal wind, but it is impossible to connect these soundings to the ones used in Robinson et al. (2011). Finally, this study should actually describe the sounding up front. Do they have an inversion? Do the moisture conditions differ? The simulations are described as Strong and Weak, but it isn’t clear until Figure 1 that there are significant changes in the amount of water vapor as well. Please describe wind speed, stability, water vapor etc. early on in the manuscript.**
L 171: Much of the introduction is spent saying how unrealistic others' simulations are, but then very simplistic choices like evergreen shrub for a land surface vegetation is chosen for a tropical island. What is the justification for this choice, and what is its roughness length?
**L 172: To follow on from the above point, a 20 deg C SST for the tropics based on soundings for an island in the western Pacific warm pool is a surprising choice. A low SST will enhance the sea-breeze strength by making the land-ocean contrast greater, thereby affecting the development of convection in the simulations. I recommend using a more realistic SST, or justifying this choice.**
L 193: The term “convective organization” is used often, but is not defined nor described. I recommend using a term like “cloud development” instead.
L 195: What are “these factors"
**L 204: I was very surprised by the westerly and northwesterly flow for simulations of a tropical island. In my experience, much of the tropics experiences trade winds which are easterly. This could be resolved by being more specific about what islands are being simulated. The soundings from Robinson et al. (2011) were made from Melville Island, an island known for its deep convection and sea-breeze convergence. If this is the target island, I wouldn’t call this a “typical” island for the tropics.**

L 215: Smith et al. 1993 should be Smith and Grubisic, 1993, and there is a spelling error in Smolarkiewicz
L 222: Is Nm or Nd calculated (the moist or dry Brunt Vaisalla frequency)? In a moist atmosphere, Nm should be used.
**L 225: In the mountain meteorology literature, most papers choose to use the non-dimensional mountain height, Nh/U, not the Froude number. Using the non-dimensional mountain height will make it easier to compare these simulations to the results of others.**
**L 228: I've never seen a study use one value averaged over the lowest model levels in a simulation. The reason is because, with the presence of an island, the Froude number should vary greatly, especially upstream and downstream of the island. Also, one would expect the stability to vary with height, so averaging over the lowest 3 km may be smearing out the results. I recommend limiting the area of the Froude number calculation to the area just upstream that will be lifted by the mountain, and looking at a vertical profile of the value to see what altitude makes the most sense for the Froude number calculation.**
**In addition, it would be helpful to compare the Froude numbers to prior work to see how one would expect the flow regime to change. For example, see Smith, 1989 Figure 1 (below). My hunch is that both of the simulations are in the same flow regime.**




Ronald B. Smith (1989) Mountain-induced stagnation points in hydrostatic flow, Tellus A: Dynamic Meteorology and Oceanography, 41:3, 270-274, DOI: 10.3402/ tellusa.v41i3.11839

**Following on to this point, the low level wind speed should be more clearly described, especially since this is an important control parameter. In Figure 1, it looks like the low level wind speed is 0 and 5 m/s and yet these cases are called "Weak" and "Strong" wind. According to most studies, these would both fall under the weak wind category.**
Figure 1: The Island shape looks ovular. Please set the aspect ratio of the figure to show it is circular. x and y-axis labels are needed on the Island Diameter figure. An x-axis label is needed on the Initial Tracers figure. Why are the tracer regions labelled 0-500, 500-1000, 1000-1500, and 1500-2000 if those upper levels are not shown nor discussed?
Figure 4: At 10:30am there is some odd activity in the figure. This should be mentioned.
L 281: Why is a latitudinal average chosen? An average perpendicular to the flow would be a better choice.
L 301: Please describe why the cloud top heights >13 km are smaller in the higher mountain simulation.
L 306: I'm surprised by the comment that the convective behavior in the weak simulations doesn't appear to be as extensive as in the strong simulations - I found the changes in the weak simulation to be more interesting!
L 348-352: I noticed this too - very interesting. Please describe this in more detail in the discussion/conclusion as I found this to be counter-intuitive.

L 366: Perhaps “inland focus” is meant instead of “inland expansion”?
General Comment: I recommend changing the order of the discussion to talk about precipitation before cold pools. It seems odd on L 387 to talk about cold pools developing from precipitation before describing the precipitation first.
Figure 5: Are these CFADs for the entire simulation duration, or a specific time period?
Figure 7: Caption should read: “Same as Figure 6 except for the STRONG-EXP 100-km diameter island.” A wind vector for scale is also needed on the image (Figure 6 needs a wind vector scale too)
L 405: “eastern” slope?
Figure 6: I expected to see clouds everywhere at around 10:30 like the Hovmoller figure.
L419: Throughout the prior discussion, I’ve been confused about the discussion of lateral and leeward expansion of cloudiness. I recommend that for descriptions like: “expansion of clouds toward the shore” that the text distinguishes what is downward propagation, and what is lateral shoreline movement. Here on L 420 the text says that it is “clearly tied” to the development of cold pools, but it isn’t so clear to me. A simulation turning the cold pools off would help to make this distinction if this is important to the study.
L 437: I find this interesting too. I wonder if it has something to do with a larger change in slope.
**L 448-450: This is a huge issue, and should be brought to light much much earlier. The water vapor flux will be largely different along with the vertical stability and structure, so one really can’t compare these two soundings since it won’t be a fair comparison. I recommend sticking with the same sounding and changing the wind speed to keep these more comparable.**
L 463, 487: Please translate to standard units of mm/hr
**L 498: This should be described more in methods. I recommend adding the tracers after an equilibrium has been reached, because the initial flow may not be representative of the actual atmospheric flow. Another way to get around this would be to continuously add tracers throughout the simulation to get around this issue of the oddities at the initial time.**
Figure 10: Add Weak and Strong labels on the left side to help the reader distinguish between the top 6 and bottom 6 panels. Also, I recommend showing the winds at the level shown by the plot rather than just the same surface winds in (ace) and (bdf). Vertical wind contours would also be helpful.
L 511: I find it surprising that there is more vertical mixing in the weaker simulation rather than the strong. I would’ve thought that stronger wind speeds at low levels would lead to stronger vertical mixing.
Following on from this point, I wonder if there is any evidence of downdrafts, and downward mixing of “cleaner” air. I would’ve thought that high accumulation would occur over the island, and downdrafts on the island flanks would bring down depleted air.
L 557-558: I wonder how much of the decreasing land-averaged precipitation with increasing island size has to do with averaging over a larger land surface? Have these been normalized by island area?
L 562: The comparison is not apples to apples here with prior literature. The high wind regime of 5 m/s is similar to the low wind regime of others’ studies and the environment is very different since this is an unstable environment with deep tropical convection compared to the shallow convection with strong trade wind inversion of others’.
L 615: Is there a hydraulic jump in these simulations? I suspect not, and I suspect this is because the “strong” flow simulation is not actually all that strong. This is another thing that can be connected to the Smith diagram above.
L 640 Rotunno 2983 citation
L 641-642: This is a key point. The simulations show two rather extreme situations (as defined by Robinson) and they are not representative of the tropics in general.