Reviewer's summary of the manuscript

In "On resolution sensitivity in the Community Atmosphere Model," Herrington and Reed seek to understand the physical mechanism(s) leading to a strong and long-standing horizontal-resolution-dependence in the Community Atmosphere Model (CAM). The bulk of the manuscript is focused on analysis of the output of a set of aquaplanet simulations with CAM6, run a a number of different horizontal resolutions. They show that CAM6 exhibits a comparable resolution-dependence to previous versions of the model, going back at least four model generations. They show that essentially all of the hydrometeorological resolution dependence originates from a strong scaling of vertical velocities with horizontal resolution: typical vertical velocity magnitudes scale in proportion to the inverse of the horizontal resolution. The increase in updraft velocities explains the increase in intensity of stratiform precipitation, and the decrease in frequency of low-to-moderate updraft velocities explains the decrease in frequency of low-intensity precipitation. The decrease in convective precipitation is caused primarily by a stabilizing of the atmosphere with increasing resolution, which results from the increase in subsidence-driven warming (and stabilization) that occurs with increasing subsidence rate.

The manuscript is generally well-written and of high scientific quality; it provides a novel and in-depth analysis of the causes of a long-standing model issue. It is publishable in principle, though I have two main concerns.

Summary of Review

The first concern is that I wonder if the manuscript in this form is suitable for QJRMS; it focuses entirely on understanding a convergence issue associated with single atmospheric modeling system (a US one, nonetheless), and it lacks discussion of how these results might generalize to other modeling systems. The authors should strongly consider expanding the discussion (and perhaps the meta-analysis shown in Figure 1) to include resolution dependence issues noted in other modeling systems. The second concern is that the authors' experimental design is not adequate to support one of their main theses: that the vertical velocity scaling exponent is definitively -1.

Given that both of these concerns will likely require substantial modification to the manuscript and/or running of new experiments, I suggest that the manuscript be returned to the authors for major revisions.

Aside from these issues, this is otherwise a fantastically thorough and detailed manuscript that warrants publication. The lead author should be proud to have this be the capstone manuscript for his Ph.D. dissertation.

General Comments

Generalizing results to other modeling systems

The authors should strongly consider expanding the discussion (and perhaps the meta-analysis shown in Figure 1) to include resolution dependence issues noted in other modeling systems. There are some other papers in the literature (e.g., Volosciuk et al., 2015) that describe resolution dependences in other modeling systems. Many of the results described in this manuscript seem like they should generalize to other hydrostatic models, and it would be worth adding some discussion on whether these results are consistent with what is shown in other models.

Relatedly, I would be curious to hear the authors' commentary on what these results imply for how a scale-aware model should be designed. It seems that now that the resolution dependence (of CAM at least) is understood, a solution might be possible, and the authors of this manuscript understand this resolution dependence better than just about anyone. They are therefore well-poised to comment on possible solutions: especially those that might generalize to other modeling systems.

Volosciuk, C., Maraun, D., Semenov, V. A. & Park, W. Extreme precipitation in an atmosphere general circulation model: Impact of horizontal and vertical model resolutions. J. Clim. 28, 1184–1205 (2015).

Inappropriate experimental design for a definitive statement on resolution exponent

Based on my reading, the manuscript states two key messages: (1) vertical velocity is inversely proportional to horizontal resolution (and therefore is inconsistent with other theories of resolution-dependence), and (2) dynamic/thermodynamic changes caused by intensification of vertical velocities can explain the resolution dependence of stratiform and convective precipitation. The bulk of the manuscript focuses on (2), and in my opinion, this aspect of the manuscript is on very strong footing.

However, the first key message seems to be based on interpretation of results from an inappropriate experimental design. The results of the authors' experiments indeed are consistent with a scaling exponent of -1, however the experimental design does not isolate horizontal resolution from physics timestep. The authors explicitly state that changing the physics timestep will "lead to greater resolution sensitivity." They later state that "the n=-2/3 slope proposed in Rauscher et al. (2016) greatly underestimates the increasing occurrence of large magnitude vertical velocities with resolution)" – in other words, the n=-2/3 slope has too weak of a resolution sensitivity. It is possible that the changes in the physics timestep explain why the resolution-dependence is stronger than predicted by Rauscher et al.

The authors state that they change the physics timestep in order to "avoid time-truncation errors". Based on my understanding of Herrington and Reed (2018), this seems like a valid consideration for the experimental design. But why does the physics timestep need to change: why couldn't it just be fixed at a value small enough to avoid time-truncation? This would avoid conflating timestep-dependence with resolution-dependence.

I recommend that the authors either remove their strong assertions that these results are inconsistent with the Rauscher et al. theory, or I recommend that the authors perform a set of tests with the physics timestep held fixed at a small value.

Issues associated with figures

Figure 5: what do the colors represent? The colorbars should have labels. Based on the caption, it seems like they should represent precipitation rates.

Relatedly, the associated text (pg 5, col 1, lines 56-57) state that "changes in M__s with resolution are subtle, while the changes in f__s with resolution are large (not shown)." Why show M__s if the f__s term dominates the resolution dependence? And if space is a concern, why not put the f__s figures in supplementary material?

Minor issues

- pg 1, col 1, line 55: 'that allows' -> 'that allow'
- pg 3, col 2, line 16: 'is ran for' -> 'is run for'
- pg 3, col 2, line 21: 'are ran for' -> 'are run for'
- pg 5, col 2, line 63: 'irregardless' -> 'regardless' or 'irrespective' (see https://www.businessinsider.com/irregardless-real-word-regardless-kory-stamper-education-dictionary-mean-girls-lexicon-merriam-webster-2017-6)
- pg 9, col 1, line 49: 'is also no significant changes' -> 'are also no signficant changes'
- pg 9, col 1, line 58: 'which often occurs with appreciable subsiding motion aloft'… is this statement justified? On my examination of Figure 11, it looks like just as many of the the ZM-active contours have large areas of green (omega ~ -0.1 Pa/s) directly above as have blue (positive omega) directly above. Take the region in Fig 11b, between 30W and 0, for example: there is very little subsidence above the ZM convection. In case my eyes are having a difficult time interpreting this graph, it might be useful to draw the contour of constant omega=0.
  - pg 9, col 2, line 26: 'the authors suggests' -> 'the authors suggest'