Second review of 'Impact of scale-aware deep convection on the cloud liquid and ice water paths and precipitation using the Model for Prediction Across Scales (MPAS-v5.2)' by Fowler et al.

I appreciate the authors taking time to address my questions regarding the initial manuscript. I think some of the additional analysis adds some strength and credibility to the results.

A couple smaller overall things to note. One, the manuscript increased in length to >800 lines. At this point that length is likely "baked in," but my experience in this is will be an onerous read for most. Two, I am a bit disappointed in the lack of contextualization with regards to other work that was referenced in the initial reviews. For example, the timestep sensitivity has been something that has been covered by many authors. While I agree that further assessing this particular aspect with MPAS and the parameterizations discussed here is beyond the scope of this paper, citing other references where readers could turn for more information would be useful.

I have a few other comments and clarifications that should be added to the text below given the response. Primarily, outside of the additional text regarding the timestep sensitivity, I feel the authors could do a better job underscoring the additional analysis they've undertaken here and how it impacts their originally-submitted results. If anything, I believe this further increases the potential impact of the paper by acknowledge the full scope of research the authors have conducted (both before and during the review process).

In general, the paper falls somewhere between major and minor revisions – I'll err on the side of 'major' revisions with that caveat in mind. However, I leave the final decision to the editor and their judgement (as well as context added by additional reviewer(s)).

Major concern 1: 30-day simulation length

I appreciate the authors analyzing the shorter simulations. I do think this provides additional confidence that the changes seen in the 30-day runs are robust. Obviously, the gold standard would be multi-year runs with MPAS – however, it is understood this is computationally difficult.

I would *strongly* recommend adding a notation regarding the 10-day analysis to text. If the authors went through the effort to calculate these, it would seem prudent to say something like "analyzing shorter 10-day averages produced qualitatively similar results to those contained here (not shown)." While I understand that reviews in GMD are made public, the majority of readers only read the accepted version of the manuscript. Adding language such as the above highlights that the authors considered the issue, investigated it, and concluded that their analysis is sound. Other readers may also find increased confidence that such an analysis was undertaken before publication.

In this vein, I would recommend citing prior work by Ma and Klein (CAPT), or others, which also confirms that fast-evolving biases in initialized runs can approximate longer climate runs. Essentially, that is what the authors lean on here (i.e., that you do not need very long climate runs to ascertain information regarding biases in parameterizations). One such example is below, although there are certainly others.

- Ma, H.-Y., Chuang, C. C., Klein, S. A., Lo, M.-H., Zhang, Y., Xie, S., Zheng, X., Ma, P.-L., Zhang, Y., and Phillips, T. J. (2015), An improved hindcast approach for evaluation and diagnosis of physical processes in global climate models, J. Adv. Model. Earth Syst., 7, 1810–1827

Major concern 2: Viscosity and timestep sensitivity

I appreciate the clarification regarding the viscosity. It appears that the regional viscosity does vary, but this value is scaled such that it is constant when grid cells are identical in size. Since this is part of the dynamical core, it is inherent in the model numerics and therefore should be considered 'required' even in a pure resolution sensitivity study.

It is unsurprising that there is a timestep sensitivity in the moist physics. I certainly think the following manuscripts should be cited as they have aggressively investigated the dependence of convective parameterization on physics timestep in highly constrained simulations. They also may serve as references for readers to follow who would like additional information on the topic of timestep sensitivity.

- Williamson, D. L. (2012). The effect of time steps and time-scales on parametrization suites. Quarterly Journal of the Royal Meteorological Society, 139(671), 548–560.
- Herrington, A. R., and Reed, K. A. (2017). An Explanation for the Sensitivity of the Mean State of the Community Atmosphere Model to Horizontal Resolution on Aquaplanets. Journal of Climate, 30(13), 4781–4797.

I do think this sensitivity calls into question the confidence in "upscaling" effects noted in the manuscript. Significant biases in the transition region or immediately adjacent (on the downwind – or, in this case, west – side) are potentially upscale effects, but large biases between the U and V simulations either upstream or in the far field seem far more likely to be related to the differing timesteps in the "default" U and V runs. It is not clear to me that Table 3 emphasizes significant upscale effects. Has the same behavior is also seen with the identical timestep runs?

As such, I would recommend removing much of the upscale discussion and focusing explicitly on the resolution sensitivity with the refined domain (which is the vast majority of the analysis). I would argue that the confirmation of upscaling effects beyond the small geographic region analyzed in this paper requires the use of more constrained, global analyses as in the following manuscript (interestingly, the authors found more minimal upscale effects, albeit with an older and lower resolution version of MPAS).

- Sakaguchi, K., Leung, L. R., Zhao, C., Yang, Q., Lu, J., Hagos, S., et al. (2015). Exploring a Multiresolution Approach Using AMIP Simulations. Journal of Climate, 28(14), 5549–5574.

Minor comments

- Response to Line 194. If this authors do not wish to add a more detailed rationale for this tuning choice, could they rephrase as something like "sigma is chosen to be 0.7 based on the results in Fowler et al. (2016) who applied the same value because (reason)" where (reason) gives a brief reasoning why 0.7 is used there?
- Response to Line 412: A sentence stating this should be added to the manuscript for clarity in addition to the reviewer response.
- Response to Lines 513-515: Left blank?
- While some have been cleaned up since the first submission, the manuscript retains some awkward phrasings here and there – primarily issues with tense and number. I would recommend a slow read-through by all authors before resubmission to iron out many of these wrinkles before any proofing stage.