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Geoscientific Model Development Discussions

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Interactive comment on "Modeling extreme precipitation over East China with a global variable-resolution modeling framework (MPASv5.2): Impacts of resolution and physics" *by* Chun Zhao et al.

Anonymous Referee #2

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Reviewer's Summary:

Zhao et al. in "Modeling extreme precipitation over East China with a global variable-resolution modeling framework (MPASv5.2) Impacts of resolution and physics" recreate an extreme weather event that occurred over East Asia between 25-27 June 2012. This study presents a comprehensive look at the performance of the Model for Prediction Across Scales (MPAS) across both hydrostatic and non-hydrostatic scales (e.g., 60km to 4km), uniform and variable-resolution grid-spacing, and three different microphysics schemes (one of which is "scale aware" for convective/resolved precipitation). The

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authors assess MPAS skill compared with CMA observations in East Asia and tersely compare results to GFS forecasts over a single-member, sub-weekly simulation period.

Overall, I think the paper is well written and fits within the scope of GMD and could be, given a bit more work, a valuable contribution to the scientific community, particularly due to its emphasis on evaluating the use new variable-resolution global climate models for extreme event recreation and sub-weekly weather forecasting. However, I think there are still several major revisions that need to happen prior to this paper being accepted. I would suggest that the editor assign major revisions to this manuscript.

Major Comments:

1) Given that this paper centers around the recreation of one weather event, why did the authors not perform an ensemble of simulations with slightly perturbed initial conditions to highlight internal variability impacts on precipitation intensity and spatial distribution? Was the computational demand too high to do so? If so, as mentioned below, it would benefit the reader to know this type of information explicitly. If not, why not perform, at least, a small ensemble of simulations (as the authors state is needed for GFS in Section 4 – line 529).

a) Line 213-215 – This might be a good time to bring up real-time computational demand (e.g., nodes used, simulated years per actual day, etc.) and physics/dynamics timesteps across “U” and “V” cases.

2) The authors should highlight other variable-resolution modeling efforts to give readers a sense that there are a community of models now available.

a) Line 106 – Given that the authors examine both hydrostatic and non-hydrostatic configurations of MPAS, the authors should also point to the literature of hydrostatic variable-resolution global climate models such as variable-resolution CESM (etc.) as these variable-resolution options have been used extensively for various applications (e.g., Rauscher et al., 2013; Zarzycki et al., 2014, 2015; Rhoades et al., 2016; Huang et al., 2016; Wu et al., 2017; Gettelman, et al., 2018; Wang et al., 2018; Burakowski et al., 2019).

3) This manuscript would also benefit from the discussion of previous studies that have shown that solely refining horizontal resolution alone has led to differing results in simulated precipitation bias across various models (including variable-resolution approaches).

a) Line 83-87 – There also have been studies showing that solely refining horizontal resolution alone can lead to unexpected “oscillations” between positive/negative simulated bias in daily-to-seasonal average precipitation too. For example, refining horizontal resolution from 55km to 28km has been shown to improve various assessments of simulated bias (e.g., orographic precipitation, hurricanes, atmospheric rivers, etc.), however refining resolution from 28km to 14km has shown an enhancement of bias (Rhoades et al., 2018; Xu et al., 2018). These differences have been shown to be bounded in theory and model structure decisions (Jeevanjee et al., 2016; O'Brien et al., 2016; Herrington et al., 2017, 2018; Gross et al., 2018). The authors should discuss these studies as well to give the readers clear perspective that resolution alone will not be the sole solution to better representations of extreme precipitation.

b) Figure 4 and 8 – These two plots somewhat prove the point made above that if CMA is used as the simulation skill benchmark and by eye, another reason Figure 4 and 8 should be difference plots, V16km seems to get the timing of the two locations of precipitation maxima and magnitudes the most correct over the storm track (i.e., 30 N +/- 2 deg), whereas at V4km the precipitation magnitudes seem too positively biased over a greater area and longer time. I think these plots also highlight the potential need for an ensemble of simulations given that the time-space structure of precipitation in each of the simulations is quite different and could simply be due to using one realization of the atmospheric internal variability.

4) Given the emphasis on microphysics choice (e.g., three different ones used in this

manuscript), a more in-depth discussion of each of the microphysics schemes should be presented in Section 2. In my opinion, the reader should be able to glean some of the tradeoffs of each of the microphysics schemes within the text and not just be referred to other publications.

a) Line 186 – Highlight a bit more detail about the microphysics schemes used as they can impact the spatial distribution of extreme precipitation that you discuss later on. For example, one-moment vs two-moment schemes, diagnostic vs prognostic, which hydrometeor species are represented in each scheme (i.e., rain, snow, graupel, etc.), what are their assumptions in drop velocity, horizontal advection, etc. A new book chapter has been published that could be a good lead on this as well (Gettelman et al., 2019).

5) The authors could give a more nuanced assessment of MPAS skill. Based on how the text has been written, MPAS never seems to perform poorly, yet correlation coefficients and spatial structures of the storm events in the figures clearly show important differences compared with observations. In addition, the authors should be very clear over which area the correlation coefficients are being computed given that they are used throughout the text. I presume these correlations are computed over the entire domain. Given that this study is evaluating performance over a single weather event, shouldn't the correlation coefficients be computed over the "mean" track of the event (i.e., using CMA as reference, 30 N +/- 2 deg lat)1

a) Shouldn't ERA5 rather than GFS be used for forecast comparison skill? ERA5 resolution is much more closely aligned with MPAS horizontal resolutions used in this study.

b) Line 289-290 – A correlation coefficient of 0.48 and 0.42 for the GF scheme simulations doesn't indicate to me that these simulations reproduce the observed precipitation propagation. I agree with your later assessment that the differences between "U" and "V" simulations are small, especially for NTD, (which is an interesting result), but to say this compares well with observed is a bit misleading. I think this statement (and others like it) must be a bit more caveated and highlight the negatives/positives of the simulations more clearly.

c) Figure 8 – This plot (as indicated below) should be remade into a difference plot. If one uses CMA as reference, it appears that the precipitation maxima during this event occurs too soon (i.e., one-day) across all of the simulations.

Minor Comments:

Line 217-221 – The authors may want to include this analysis of minimal precipitation difference in the supplemental for reader clarification.

Line 238 – In MPAS, could reanalysis data also be used to replace the coarse resolution portion of the simulation outside of the refinement regions (i.e., akin to a conventional regional climate model)? This could be an interesting next step for a future publication (unless it has already been done) to limit model drift due to the large-scale boundary conditions.

Line 263 – Given that you point to the Meiyu front a few times in this text, you may want to spend a bit of time in the introduction to discuss the importance of the Meiyu front for shaping East Asian precipitation and cite some studies for further reading.

Line 283-284 – Again, the authors may want to offer this analysis in the supplemental material to allow for readers to determine how "negligible" the results were between microphysics schemes.

Line 324 – I think the authors should guide the readers intuition on GFS skill at 1 deg and 0.5 deg (seems pretty poor compared with CMA) as a comparison to MPAS. This seems to be a central point of the study that MPAS can offer enhanced skill (in some measures) for extreme precipitation forecasts, but seems to be a bit muted in the text.

Line 341 – Again, the authors may want to put this analysis that is not shown in the supplemental.

Line 355 – What does “fairly well” indicate? This is an example of the larger major comment that MPAS skill is not evaluated in a nuanced fashion.

Line 366-368 – If I’m reading the space-time plots correctly, it appears that the peak precipitation of the weather event at 30 N +/- 2 deg consistently occurs a day earlier than expected compared with CMA (save for V4km.Thompson which seems to overly precipitate over several days). Therefore, if this is true, the use of the word “roughly similar” is a bit misleading.

Line 489-490 – Another example of a slightly misleading statement about the MPAS 4km simulation skill. The heavy precipitation is captured much better than other coarser resolution MPAS simulations, but precipitation magnitudes, especially maxima, are high biased and precipitation durations are biased over a much longer time than other resolutions (e.g., Figure 8). In my opinion, this warrants a Table that explicitly states the summary statistics for each of the MPAS simulations (and CMA as well).

Figure 2 – The authors may want to make the color labels from 0-1 mm/day white instead of blue. This might make it easier to see the vector winds.

Figure 4 and 8 – Given that these plots are meant to just show the spatial distribution of precipitation (and not include vector winds) compared with the observed CMA product, the authors may want to provide these plots as difference plots to help readers locate mismatch. This would also highlight how GFS is suboptimal for forecasting extreme precipitation events in East Asia at the moment. Also, in Figure 8, each title should be consistent with the explicit use of WSM6 or Thompson microphysics.

Figure 5 – Given that this plot is purely meant to show the partitioning of resolved and parameterized precipitation across resolutions spanning hydrostatic/non-hydrostatic scales, could the authors change the units from mm/day and instead use a % of total precipitation? Again, I would suggest that a white/transparent color be used at the lower end of the color bar as well.

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Figure 6 – I would suggest that a white/transparent color be used at the lower end of the color bar to focus reader attention and more clearly present vector winds.

Table 1 – The table should be standalone; therefore, “U” and “V” should be defined in the caption (i.e., Uniform and Variable Resolution) and WSM6, NTD, GF, etc. should be as well.

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