

## ***Interactive comment on “Evaluation of a Unique Approach to High-Resolution Climate Modelling using the Model for Prediction Across Scales (MPAS) version 5.1” by Allison C. Michaelis et al.***

### **Anonymous Referee #2**

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Review “Evaluation of a unique approach to high-resolution climate modelling using the model for prediction across scales (MPAS) version 5.1” by Michaelis et al.

This manuscript describes a set of simulations and their basic results with the global high-resolution model MPAS 5.1. The set-up of the experiments is interesting and in some aspects new. The results presented are sound, well represented and illustrate the very good quality of the model. I have however a few comments.

1. The authors show the TC characteristics for the present climate, but not for the future climate, whereas they show in section 5 plots of the future climate. I was disappointed that nothing was said about TCs in the future climate. I assume that it will be analysed

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in a future paper, but for the reader it feels very disappointing. Why not postpone the climate change simulations altogether to a following paper? The added value of section 5 is very minor. Concentrating on the present climate and showing the quality of the model as in sections 3 and 4 should be fine for presenting the model and the experimental set-up. If you want to discuss climate change I would like it to be more than what is in the paper and include TC changes, or make it explicitly that they will be discussed in a future paper. 2. The experimental set-up is new in certain aspects, but I had the feeling that the authors over state a bit the uniqueness. They present it as global PGW simulations, whereas to me they are a clever way of time slice experiments with prescribed SSTs, with a spin-up to let the model come into equilibrium with the SST and CO<sub>2</sub> concentrations. I found the PGW term therefore confusing and suggest to remove it. 3. Ordering the simulations by El-Nino strength is clever, but anomalies outside El-Nino can occur, which can be substantial, and still have to be adjusted. I am not sure if one month spin-up is sufficient for these regions. 4. The use of high resolution SST data and computation of future SST by taking the delta of an ensemble of low resolution GCMs is a good approach. However, the delta SST can be influenced by the biases in SST. This can be particular large at the western boundary currents, that are incorrectly represented by low-resolution GCMs.

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