Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2022-111-RC1, 2022

Comment on gmd-2022-111

Anonymous Referee #1

Referee comment on "Advancing precipitation prediction using a new-generation storm-resolving model framework – SIMA-MPAS (V1.0): a case study over the western United States" by Xingying Huang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-111-RC1, 2022

This paper presents some case studies using the SIMA-MPAS GCM, which is a combination of the atmosphere and land component of the Community Earth System Model (CESM) and the nonhydrostatic MPAS dynamical core using the SIMA framework. The main simulations are performed at variable resolution with a 3km grid spacing covering the western US and a 60km grid spacing for the remaining globe. When compared to observations, SIMA-MPAS shows more realistic precipitation intensities, snowpack cover, and a smaller 2m temperature bias than simulations with the regional climate model WRF at a similar resolution (4km grid spacing).

The study provides confidence in the ability of SIMA-MPAS to produce realistic global climate simulations with variable grid spacing and the use of storm-resolving scales in regions of interest. This is a nice achievement considering the non-trivial coupling between the CAM physics package and the MPAS dynamical core (as described in the paper) and it encourages further research using this model framework. Therefore, I think the paper would be a useful contribution for the atmospheric modeling community.

However, there are some aspects of the paper that should be improved before publication.

Major points:

- A significant part of this work is the comparison of the model output with observational data, which is used as justification for the fitness of the model. However, the description of the different observational datasets is very brief (or non-existent). Without going through the references and performing their own literature research, many questions are unanswered to the audience. What are the respective resolutions of the datasets? How is the data obtained (i.e., what kind of product is it)? Are there any known biases (here, one gets the impression that they are the ground truth)? How do they compare to other observational products? Since the observations play such a significant role in this paper, I would expect a more detailed description of the respective datasets.

- I find Fig. 3 confusing. For E) "Land 2m Temperature", the legend says that EBAF 4.1 was used. However, in Sect. 2.2 you state that CERES EBAF was only used for cloud and radiation fluxes properties, whereas GHCN (which is mentioned nowhere except in Sect. 2.2) and/or PRISM was used for 2m temperature. So either the legend in the figure is wrong or the description in Sect. 2.2 is wrong. Please adjust and clarify. Also, if you have 2m temperature from PRISM and GHCN, why would you not include both into the analysis? Furthermore, I would suggest to use a clearly different color for the observations to make it stand out more.
- I'm missing any information on the performed regridding for Fig. 5 and Fig. 6. Has the model and observational data been regridded for the analysis? If yes, to what grid and how? If no, how do you account for different grid spacings? This information is crucial for such an analysis (and the reproduction of the results!) and it should be clearly mentioned in the paper.
- SIMAS-MPAS (3km) with MG2 microphysics seems to perform very well in spatial representation of precipitation (Fig. 4) and daily precipitation frequency (Fig. 5) when compared to observations over 5 seasons. However, when only looking at one season for the comparison with MG3, the MG2 version underestimates heavy precipitation frequency. So there seems to be quite a bit of variability and this does not exactly provide confidence in the robustness of the results, especially for Fig. 6. Is one season really enough to conclude that MG3 performs better? Maybe MG3 would overestimate heavy precipitation frequency over all 5 seasons? You also state in the paper that this issue requires more investigation. Therefore, I'm not sure whether it's wise to include these results that prominently in the paper. Maybe these results would be better suited for the appendix.
- Is the SST and ice sheet for Set A constant? Or are the forcings from different years or only from year 2000? How is the model initialized? From the paper it is not entirely clear to me how the mean climatology is obtained. Also, the last sentence at Section 3.1 does not make much sense to me (lines 224-225). Please clarify.

Minor points / typos:

- Lines 14-16 in the abstract read like SIMA is the atmospheric component of CESM, whereas from reading the introduction and the website, SIMA is just a framework that allows for the coupling of different components. Please clarify or reformulate.
- Section 3.4 & Fig. 9: I would at least expect a sentence about the use of gravity wave drag parameterization for the different simulations (I assume the 60km uses one, whereas it's not really necessary for 3km), as this will likely have an effect on the strength of the jet.
- I would reverse the color bar for Fig. 4 b) and c). In Fig. 4 a), red means more precipitation and blue means less. For the differences it is the other way around. I believe it would make the plots easier to read to reverse it for b) and c) (as you have done it in Fig. 8).
- The terms "non-hydrostatic" and "nonhydrostatic" are both used in this paper.
- The term SST is used without definition.
- Lines 122, 143, 149, 181, ….: "We would" instead of "We'd"
- Line 421: "vertical wind patterns" sounds like you have analyzed vertical winds. Maybe

use “cross sections of zonal and meridional winds” or something similar.