Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-284-RC2, 2021

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "BCC-CSM2-HR: A High-Resolution Version of the Beijing Climate Center Climate System Model" *by* Tongwen Wu et al.

Anonymous Referee #2

Received and published: 3 March 2021

Overall, this is a straightforward and easy-to-read paper that documents the high resolution BCC-CSM2-HR model within the context of a previous documented medium resolution model (BCC-CSM2-MR). The paper concludes that there are noticeable improvements in the mean state and phenomena of interest at high resolution, but biases still exist. This paper is appropriate for eventual publication in Geoscientific Model Development, but I have outlined some major comments below that need to be addressed before the manuscript can be accepted. In addition, I have provided numerous specific comments for consideration by the authors.

Major Comments:

Printer-friendly version

Discussion paper

1. Throughout the manuscript it is often unclear what grid(s) is being used for the analysis. That is, what grid are the observations and model results plotted on? Are they re-gridded to a common grid? Are they on the native grid? The interpretation of the results (or, in some cases, even the results themselves) can be influenced by this, I think it is important that the authors provide more detail on this topic.

2. In Section 4 details of the observational datasets, including spatial resolution, temporal frequency, and time period, is not always clear. I would recommend that the authors reorganize the manuscript and include a Section that introduces all of the datasets and their details before Section 4.

3. For all figures consistent line colors for observations, BCC-CSM2-MR and BCC-CSM2-HR should be used. In particular, Figures 1, 2, 7, 8, 12, 14 and 18 use multiple different colors (red, blue, black, green, purple, etc.) to denote the simulations and observations. They should be the same colors for all figures and datasets.

4. Finally, I think the usefulness of this manuscript would be enhanced if the Conclusions (Section 5) included some discussion of how these main results compare to other work that has explored the effect of increases in resolution in climate models on the mean state, circulation, and phenomena of interest. In particular, the authors could put this work in the context of HighResMIP results, as well as some of the papers references in the Introduction of the manuscript.

Specific Comments:

L29-31: Provide detail of the horizontal grid spacing here if possible.

L34: Consider changing “dynamic core” to “dynamical core” throughout the manuscript.

L57-58: Extend this sentence to put in context of the more recent CMIP6 (in addition to CMIP5).

L66: Remove “but.”

L76: Change to “the QBO.”

L78: Replace “and” with “,”.

L94: Be specific that the authors are referring to atmosphere and ocean grids here.

L98: HighResMIP is not “the primary activity” of CMIP6, as only a subset of models has completed it. Please reword.

Table 1: There seems to be a miss formatted Wu (2012) reference in the “Deep convection” row.

L371-L372: Three models? Only two models are introduced in Section 2.

Figure 2: See major comment about regridding above.

L403-408: Figure 3: It would be easier to see the biases if the models were plotted as a difference from the observations. Consider adding additional panels to the Figure.

L446: Important variables in what way?

Figure 6: See major comment about regridding above.

L456-457: The authors should discuss the degradation in the simulation quality of precipitation east of the Philippines near the Pacific warm pool during JJA. I believe this was also seen in the Bacmeister et al. 2014 paper cited in the Introduction (see their Fig. 8). Some discussion of this degradation is needed here.

L465-468: This is an example where the underlying grid could be impacting the analysis (if the models and observations are not compared on a common grid – which is not obvious here).

Figure 8: See major comment about regridding above. The differences in grid could have implications here.

L491&L498-499: The authors could discuss how common this high-resolution cold bias is among other modeling groups, such as those that participated in HighResMIP.

Figure 10: The authors should include a panel of the observational (CRU) data at the top of this plot, similar to what was done for Figure 9.

L503-507: Is this somewhat to be expected since there are no wholesale (besides resolution) changes to the land modeling component?

L511-512: What is the resolution of the HadISST product? Please provide that information.

L544: What impact does this threshold have on the results if the same value is used for both resolutions?

L542-551: Provide more information on the temporal frequency of the storm tracking? Is it daily for all tracking steps? Is intensity a mean or instantaneous?

Figure 14: This analysis is doing for daily storm intensities? Please provide more detail. How is the daily value (mean) calculated when storm intensities are typically represented instantaneously (as in IBTrACS). Are storms tracked daily or 6-hourly similar to IBTrACS? See comment above.

Figure 14: I find it difficult to believe that a model with $\sim$45 km grid spacing is replicating these high intensities (particularly, surface wind speeds) so well. But, it is also hard to interpret what a daily maximum intensity is. The authors should put this result into the context of the HighResMIP results, as well as Davis 2018 (https://doi.org/10.1002/2017GL076966).

L595: How is skillfully defined here?

L603: Provide detail of what observational dataset is used for OLR here.

L1310: Is it 3 hourly for BCC-CSM2-MR and BCC-CSM2-HR? To make this clear consider removing the "," after "2019."

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-284,

2020.