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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 #3

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This paper intends to provide a documentation for the latest version of the high-resolution Bejing Climate Center Climate System model (BCC-CSM2-HR). It consists of a succinct overview of what are new in each of the model components and an evaluation of the model performance using a broad set of climate metrics. The evaluation of the BCC-CSM2-HR is presented in parallel with that from a medium resolution model, BCC-CSM2-MR, which differs from the HR model not only in resolution, but also in core components. Improvements of the HR over MR are highlighted throughout the text. BCC-CSM2-MR is used to participate in CMIP6, while BCC-CSM2-HR participating in HiResMIP. The materials presented regarding the models and the simulation

skills are expected to be useful for the users of the models and/or the simulated data, and thus suitable for a publication on GMD as an essential reference.

The manuscript however has much room to be improved. The manuscript apparently has writing contributions from different coauthors. The quality of the writing for different parts of the manuscript are quite different. There are many incomplete, inaccurate, inconsistent or unclear statements throughout the text. Many of them are pointed out in the specific comments below, which are not in the order of severeness of concern, but in the order of their appearances in the originally submitted manuscript. Given the broad evaluation on various model features and climate phenomena, it is understandable coauthors contribute to different sections or subsections with their expertises, and the materials in general are appropriate for a model documentation paper like this. But lack of thorough review of the final writing seems evident.

In addition to addressing the comments below to refine the writing, it is also suggested to provide a separate section to describe the observational data used in the evaluation and if any regriddings (for grid normalization) are involved. In the current form, the description for the reference data are scattered and very limited.

Specific comments

Line 31, "participation to", use "participation in" instead.

Lines 45-45, "double ITCZ . . . is . . . disappeared", just use "disappears".

Linne 65, remove 'but'

Line 68, higher resolutions are not limited to 50; change "i.e." to "e.g.".

Line 76, just say "MJO" in the context should be sufficient to convey the same message concerning MJO. Do the authors have other emphasis implied for MJO in its representations in model?

Line 78, The meaning of "small-scale" here is very vague, and storms and TCs are not really small-scale. Suggest to change to “weather scale” or “small-scale processes associated with mid-latitude storms and tropical cyclones”.

Line 92, change ‘within’ to ‘with’ or ‘using’

Line 95, it is costly but certainly more than just a few research centers can perform it. Suggest to remove “and can be realized only at a few . . ..:., or as a way to transition to the follow-up description, emphasize it with “ . . . costly effort but a growing number of research centers can exercise it.’

Line 110, use consistent notation for the numbers in the same phrase, either with “2 weeks to 1 yea“ or “two weeks to one year”.

Line 111-112, I think “the medium resolution version” is more suitable here than “the previous version of medium resolution”. Both HR and MR model mentioned are for the same generation (BCC-CSM2) and the ‘current’ version does not seem to have an MR model that shares the same code base as the HR.

Line 123, to ‘a’ fine grid, just like describing the other grids.

Line 124, with an achievement to “deliver all of these model versions”.

Line 125, suggest to put it as “ four components – atmosphere, ocean, land and sea ice – interacting with each other”.

Acronyms should be defined when first appearedïijŇe.g., AVIM, BCC-AGCM3, BCC-AGCM, among others.

Figure 1, it should be useful to have a separate subplot for the thickness of the model layers in the lowest few kilometers; but it does not have to, given no emphasis on the description of layer thickness of the lower tropospheric levels.

Liness 145 and 187, replace ‘dynamic core’ with ‘dynamical core’.

Line 170, suggest to change to ‘at different spatiotemporal scales and interactions

between them’.

Line 189, to make it clear, please use ‘grid spacing dependence’.

Line 202, suggest to change to ‘... at the top and the kth layers of the model, respectively’.

Line 168 and more in the text, suggest to use ‘Spatially-varying’ in place of ‘spatially-variable’. Also change it in Table 1 for the HR column of the dynamical core.

Line 205, it should be useful to explain what the ‘polar instabilities’ here refer to and how it increases with damping coefficient near model top.

Line 209, The meaning of the sentence starting from “This is . . .” is not clear. Does this properly convey the meaning: “This is possibly due to much more damping of the meridional waves,”?

Line 231-232 on transporting entrained cloud water to its neighboring grids inside the model time step. Does it involve treatments other than what can be expected from typical process splitting? The description seems to imply that as it is said to be only in the HR model. Please elaborate, given that it appears to be a unique feature.

Line 236, use in favor of instead.

Line 248, BLs, in general, readers should be able to know what it refers to. But as always, please expand it in the first use.

Line 255, ‘svl’ does not have ‘vl’ as subscript.

Line 248, some description should be helpful on how to detect the presence of inversion layer for determining which eddy dffusivity formulation would be used for a column, especially if thd detection of inversion involves some special treatment.

Line 290, cite some references that use LTS=17.5K as a threshold criterion for BL stability to factually demonstrate the ‘robustness’ as written in the text. Furthermore, the Hack scheme is said to be applied to the whole atmosphere column previously. Please elaborate if it remains so after introducing this activation threshold, and if so, justify the use beyond boundary layers.

Line 306-307, It appears that AVIM2.3 is mentioned abruptly. Is AVIM2.3 used in the HR model while AVIM2.2 in the MR version? Is the difference between v2.2 and v2.3, in sub-grid surface classification, to take advantage of the capability of the HR model? Last, provide a description for the grid configurations used by the land surface model – aware it is indicated in Table 1.

Table 1, please add references for CLM3, SISv1, SISv2.

Lines 320-323, the text for the neutral diffusion scheme with constant diffusivity here to describe the MOM5 that is used in the HR model appears inconsistent with that in Table 1, which says neutral diffusion scheme is not used.

Line 315, what does it mean by 'comforts of algorithm'?

Lines near 318, please add some reference for tracer advection scheme MDPPM.

Line 336-337, add a reference for the Semtner's scheme.

Line 342, use plural for 'simulations'.

Line 343, suggest to remove 'from 1971 to 2000', which would otherwise gives an incorrect initial impression that the simulations themselves are for that period only. It is clear enough at the beginning of the Results section and it is this period of the historical simulations that are analyzed for the paper.

Lines 371-372, Only see two models involved. Is 'three models' just a typo? Also, it is dubious to say making a 'right' intercomparison. A 'reasonable' intercomparison would sound more appropriate here. After all, the model components are quite different, so do the initialization procedure and the simulation period.

Line 375-376, the meaning of the sentence is ambiguous. suggest to change to "...

primary source of data for estimating Earth's energy balance."

Line 377 and related text: which version of CERES-EBAF data are being used?

Lines 385-386, the text says "TOA LW and SW components in HR are much closer to CERES-EBAF than the MR". The numbers in Table 2 clearly says the opposite for almost all quantities with the exception of clear sky fluxes.

Lines 395-397 and Table 2. It is not true both the MR and HR have stronger LW cloud radiative forcing than CERES-EBAF data. Only the HR model does. The sentence as is is also problematic. Rephrase to reflect that the HR model has near 2 W/m2 of additional warming effect (biases). SW cloud forcing for CERES-EBAF looks like from an earlier version of data. If using the latest version of CERES-EBAF (e.g., v4.1), the model-observation discrepancy could be much larger. Again, please indicate the version of CERES-EBAF data used, and try to use the latest version of data if not yet. Given the several inconsistencies between the text and the Table 2, I am not sure if the correct table is used in the manuscript.

Lines 406-408, suggest to change to " . . . new treatments for boundary layer processes" , because there is a new scheme introduced, as well as new treatment to go with a scheme (Hack scheme) that is also used by MR. Also suggest to remove the 2nd part of the sentence, it could be perceived like the confinement of water vapor only occurs to the HR model over the eastern ocean basins. Moreover, simply attributing to the parameterization scheme could be an understatement because differences in both horizontal and vertical resolutions could also have an impact.

Though CIRA86 appears to indicate the data period is for 1986, it is still necessary to explicitly state the time span of the data, just like for the other data products.

Lines 416-421. It is unclear whether the first sentence is to describe the observed vertical structure or the model biases. It is more likely the former but then the wordings are not appropriate. The vertical structure is the well-known Earth's atmospheric stratification, from surface up, troposphere, stratosphere and part of mesosphere in the HR model. The cool layers span the transition from troposphere to stratosphere layer. The description in the text appears like casually picking a pair of cold/warm features without reference to the actual atmosphere. The 'cool layers' as used in the text, the center of which over broader latitudes reflect the location of tropopause, and it is not centered near 300 hPa. The layer centered at 1hPa cannot be 'too warm' by itself.. It marks the top of the stratosphere and the transition to mesosphere. I think the current description is oversimplified and not acceptable, other than that the HR model is capable of capturing the structure of upper stratosphere and the transition to mesosphere while the MR model cannot, and the reversal of polar stratosphere structure from DJF to JJA. The description for Figure 4 needs to be totally rewritten.

Line 429, The description does not appear to separate well this tropical region of warm biases and the thicker layer of warm biases in broader lower stratosphere over the tropics and mid-latitude. Note that the warm biases are still situated in lower stratosphere, not 'upper stratosphere'.

Line 442-443, the reference to QBO here is purely speculative and without basis. These are biases in mean climatology, as a long term mean, even if the models have skill in simulating QBO, the signal would have been largely averaged out in long term mean. Suggest to remove this statement.

Fig 7, right panel for zonal mean precipitation over land? No words about it? HR should resolve better the precipitation features influenced by orography. If there is no intent to provide a description to highlight the improvements over land in HR, might as well not to include the Figure.

Huffman et al. 2019 for IMERG data and algorithm, please provide a URL for the reference. Also on IMERG data at line 475, while what is stated in Huffman et al. is that their algorithm can also be used to intercalibrate 'potentially other precipitation estimators', it sounds very odd that the available IMERG data product has combined 'potentially

other precipitation estimators' – which would mean the source data for IMERG are not fully clear. The 'potential' apparently is for different context, Re-examine the IMERG document if such 'other' data are included and if so make it explicit. The description in the current form is not appropriate.

Again on IMERG and Figure 8, though it can be assumed in such comparison, it should be explicitly stated that all data are rearranged to have the same grid resolution and time averaging interval. Don't see it in the text. Also, the Figure caption says the data are three hourly, but line 476 says hourly precipitation. Make them consistent. Furthermore, the description for Figure 8 is less than accurate and over simplified, which could be misleading by following the text alone. The 1mm/hr and 10/hr cutoff may be about fine for the MR model, but substantially off for the HR model. The authors may start with just describing the comparison of MR with IMERG, then highlight the improvement of HR, instead of describing them in the same sentence with the same cutoff value. The last sentence in the paragraph is well suited.

Line 486-488, can the authors provide an explanation why selecting EN4 and CRU data as references? There could be many other choices. Some description on the benefit of selecting these reference data should add credentials to the evaluation process.

Line 495, it should help by explicitly referring to Fig. 3b in addition to the text regarding strong SW cloud radiative forcing.

Figure 11, The plot should be for spatial distribution of sea ice concentration rather than overall sea ice extent. The description uses the correct term.

Line 527, "mostly smaller"? It does not read well with "almost smaller", the meaning of which would also be ambiguous.

Line 531, add 'by' to have "overestimated sea ice by about . . .".

Paragraph starting line 542, TC criteria, please explain why using a relative vorticity criterion differing by 15 times for the HR and MR models, how sensitive are the results to this criterion and how it impacts the interpretation of the derived results.

Line 542, Citing a single work of Murakami (2014) does not appear consistent with a plural form of 'previous studies'. Suggest to cite the works that first used the criteria that are listed in the paragraph.

Line 546, please elaborate how the air temperature deviation is defined or cite references that provide clear description for the calculations. The threshold of 0.8 K as the sum of the deviations at three levels also appears to be small, compared to thresholds used in the definitions of TC warm core in other TC trackers (e.g., the warm core criterion in GFDL TSTORMS tracker is at least 1K warmer than the surrounding local mean (Zhao et al., doi: 10.1175/2009JCLI3049.1) for an averaged temperature over a depth, not even a sum).

Lines 548-549: 'within the vortex center 3 deg x 3 deg grid box', suggest to change to " within the vortex center for an area of 3 deg x 3 deg'. Please also indicate which level of wind speed is concerned here?

Line 556-559, the description of global annual mean TC numbers read like it is also higher in the MR model than IBTrACS, which is not true given the numbers in the text and Figure 13. Furthermore on the description for Figure 13, while the speculation of the factors that are possibly relevant for missing Atlantic TC are reasonable, would the authors offer some discussion if the different criteria used play a role?

Line 575 about models cannot capture weak storms with maximum wind speeds less than 10 m/s. The statement seems inconsistent. Even IBTrACS does not have it, while actually the MR model has some with max wind speed at or below 10m/s. The description should also indicate that max wind speeds in MR are consistently weaker which is understandable given coarser resolution.

Line 580, the whole paragraph is to describe Figure 14 which is indicated at the beginning. Remove the redundant mentioning of Figure 14 at the end.

Line 588, should be lag-latitude evolution for the right panels of Figure 15.

Figure 16a, which observational/reanalysis data are used to create the observed MJO life cycle, or if it is adapted from a figure in another publication?

Line 619, use instead 'A good simulation of QBO . . . '

Line 630, suggest to remove 'in amplitude'. The meaning of the sentence would remain intact. Amplitude typically would account for the extent of the oscillation between both phases.

Line 637-638, the description sounds like the authors have looked at the comparison of the parameterized convective gravity wave forcing, but the results are less than conclusive, is that so? Otherwise, without any concrete evidence or explanation, how to draw a statement that " . . . seemed enhanced . . .". A statement of the forcing could potentially be enhanced would be a better statement if all by speculation, but that should also need to be justified.

Line 658 at the beginning of the 2nd sentence, suggest to change to "The phase locking (i.e., the peak variance) . . ." to inform even less familiar readers what the term refers to.

Line 666-667, suggest to change to "despite over extension into the Western Pacific".

Line 667-671, after describing the observed expansion of influence to extra-tropics, it should be followed by the description of more equatorially/tropically confined in the models. The focus is not on observational analysis, after all. The descriptions for the models are clearly over-simplified, with a single statement that HR improves over MR. Moreover, for a plot like this and the description to compare models vs obs on both negative and positive correlation, it should be useful to denote in the plot where the correlations are statistically significant, otherwise some of the discussions could be simply irrelevant.

Line 681, use participating in.

Itemization in the paragraph starting line 683, use 'First,.... Second, ..., Third ....' instead.

Lines 702-703, the description would give an impression that the simulations are just for 30 years. Suggest to change to "historical simulations with fully coupled BCC-CSM2-MR and BCC-CSM2-HR are analyzed over a 30 year period from 1971 to 2000.".

Line 715, lower troposphere temperature biases are relatively small in both models, right? Indicate so.

Lines 719-720 "do not change at higher resolution" and "insensitive to atmospheric resolution" are apparently redundant.

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