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# *Interactive comment on* "Mitigation of the double ITCZ syndrome in BCC-CSM2-MR through improving parameterizations of boundary-layer turbulence and shallow convection" *by* Yixiong Lu et al.

**Anonymous Referee #2**

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Review of Manuscript gmd-2020-40

Title: Mitigation of the double ITCZ syndrome in BCC-CSM2-MR through improving parameterizations of boundary-layer turbulence and shallow convection Authors: Yixiong Lu et al. Recommendation: major revision

Summary

The authors examine how the Pacific double ITCZ bias responds to modifying the boundary layer turbulence and shallow convection schemes in the BCC-CSM2-MR



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GCM. They suggest than an improved representation of the stratocumulus-to-shallow-cumulus transition in the new parameterization leads to increased cloud cover and reduced SST in the southeastern tropical Pacific. This, they argue, alleviates the double ITCZ bias.

The paper is generally well written and concise. It is not clear, however, if the changes in the new model version objectively constitute an improvement. Rather, it seems that the modest improvement seen in the Pacific ITCZ is achieved at the expense of an unrealistically high cloud fraction and excessively cold SST in the southeastern tropical Pacific. This raises the question of the role of error compensation. I believe the results of the study are worth publishing but there needs to more objective/quantitative assessment of the bias reduction. There also needs to be more discussion regarding the aspects that deteriorate in the new model version, and discussion of the potential role of error compensation. Detailed comments follow below.

Major Comments

1) Figure 3 (longitude-height sections of cloud fraction) While REF_amip undeniably underestimates cloud fraction, NEW_amip certainly overestimates it, to the point where one wonders which version is better. Even qualitatively, the superiority of NEW_amip is not that obvious. In the Peruvian stratus region, e.g, there is a spurious offshore maximum at 95W, 850 hPa. Thus, it is important to have an objective measure of model performance. I suggest adding a table with pattern correlations and area-averaged root-mean-square errors (RMSEs) for all regions.

2) Figure 4 Again, it would be helpful to have an objective measure of improvements in the equatorial Pacific, like the RMSE. The unrealistically zonal orientation of the SPCZ seems to be pretty much the same in both experiments. It is true that the 3 mm/day contour does not extend to 90W anymore in NEW_amip, but that is just a very narrow protrusion whose elimination should have little impact on the area average. Interestingly, the improvements look more convincing in the equatorial Atlantic.





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3) Figure 7 No mention is made of the cold bias in the target region that is incurred by using the new parameterization. Visual inspection suggests that the area-averaged RMSE of SST may actually deteriorate in NEW_cmip. Please calculate those metrics and discuss them.

4) Figure 10 How does the simulated wind stress compare to observations/reanalysis? Please add a panel.

5) Figure 11 I suggest removing this figure or expanding the analysis. While zonal advection is certainly a plausible mechanism for the cooling, a detailed heat budget analysis would be needed to make a convincing argument. Other processes, such as upwelling and vertical mixing may play an important role as well.

6) Figure 13 If this figure is to be kept there needs to be an additional panel showing performance before the introduction of the new schemes. Otherwise it is impossible to evaluate the improvement.

Minor Comments

1) ll. 32-33: Please mention some references for the Atlantic ITCZ bias (e.g. Richter al. 2014, Siongco et al. 2015).

2) ll. 59-60: Please provide some references for the claim that stratocumulus biases contribute to the double ITCZ problem. Also, some studies have found that shortwave radiation biases in marine stratocumulus regions are overcompensated for by excessive latent heat flux (even in AGCM-only simulations with prescribed observed SST), which suggests a different origin of the warm SST biases (de Szoeke and Xie 2008, Toniazzo and Woonough 2014, Vanniere et al. 2014, Xu et al. 2014, Zheng et al. 2011). This should be discussed.

3) ll. 69-70: Please provide a reference for this claim.

4) ll. 91-92: The atmospheric component ultimately traces its origins to the NCAR Community Atmospheric Model (CAM). It is important to note this origin and to explain

to what extent the BCC version has diverged over the years. Does the BCC model feature similar biases as current incarnations of CESM?

5) l. 143: What does “roots in level k+1” mean?

6) section 2.3, last para: In the light of the substantial progress made in the field, the LTS criterion appears crude and outdated. There must be more sophisticated criteria.

7) l. 256: “Below will clarity” -> “Below we examine”

8) l. 267: “triangular-shaded” -> “triangular” or “triangle-shaped”

9) Figure 6: Given the relatively small improvement in precipitation seen in Fig. 4, the large improvement in this figure is somewhat surprising. I guess the improvement is diluted in the annual mean (Fig. 4)?

10) ll. 411-412: “cold tough bias” -> “cold tongue bias”

11) The authors should discuss the work of Hourdin et al. (2020) as those authors also stress the importance of the marine boundary layer in tropical biases.

References

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