Review of

Mitigation of the double ITCZ syndrome in BCC-CSM2-MR through improving parameterizations of boundary-layer turbulence and shallow convection

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General

The authors report the improvement in the known 'double ITCZ bias', common in most coupled climate models, in the BCC-CSM2-MR model. The authors aptly report the improvement caused by the implementation of new boundary layer and shallow cloud parameterisations. I am not an expert in the associated parameterizations, and so not in a position to critically assess the technical and physical aspects related to their implementation. I do have some comments with regards to the reported improvement in the double ITCZ bias and on the general presentation of the analysis. I find the work to be generally well presented and within the scope of GMD. With regards to the mitigation of the double ITCZ bias, I encourage the authors to take into consideration the comments provided below.

Specific comments

1. Adding the values of critical parameters used in the revised parameterization would help the readers and enable the reproducibility of the reported results (e.g., A in Eq. 5).
2. Line 27: the double ITCZ bias is seen year-round in the central and western Pacific, but only during the SH rainy season over the eastern Pacific (and Atlantic). (See Adam et al. 2018 and Li & Xie 2014)
3. Lines 228-230: increased cloud fraction in the subtropical eastern Pacific cools surface waters which subduct and eventually end up in the cold tongue. It is worth mentioning this coupling mechanism here, which was shown to have an important effect by Burls et al. (2017).
4. Lines 255-256: both the surface temperature and surface temperature gradient have an important effect on boundary layer and deep convection, as shown by Back and Bretherton (2009). The effect of BL convergence by SST gradients is not accounted for in the analysis.
5. Lines 272-274: Indeed, the biases in the eastern Pacific and Atlantic are reduced. However, a negative bias in the Equatorial Indian Ocean seems to get worse. Are

the biases in the Indian Ocean, as well as the changes in the revised model, also related to the BL parameterization?

6. Fig. 4: The cold tongue bias seems to persist in the revised model. (This is mentioned lated in the analysis.) Since the cold tongue bias is known to be closely linked to the double ITCZ bias, it is interesting and worth highlighting that an improvement is achieved only in one aspect of the bias.
7. Lines 286-293: Both the anti-symmetric and symmetric components of the precipitation bias are significant (e.g., Adam et al. 2016). I suspect that the equatorial precipitation index (Adam et al. 2016, 2018), which was found to be strongly correlated with other phenomena (Popp and Lutsko 2017), will be quite different from observations in the revised model, in particular since equatorial precipitation in Fig. 5 is lower than observed.
8. Section 4.4 and Fig. 11: This is an odd Figure. According to classic theory, wouldn't it be the curl of the wind stress that affects the zonal ocean currents, rather than the intensity of the Walker circulation? In any case, Fig. 11 and the short treatment of this potentially important aspect seem perfunctory. I would suggest either omitting this section from the paper or providing a more detailed and complete analysis.
9. 5.2 Indeed the HR model seems to dramatically improve the representation of tropical precipitation. However the reader is left curious and confused. The authors claim that it is the UWMT that accounts fo the major improvement in the HR model but provide virtually no support for this claim.
10. Lines 410-411: is this true also for the HR model?

Technical/editorial comments

14 promotes —> ameliorates
32 fake —> spurious
37 impediment to what?
49 convection and cloud radiative effects
62 Previous attempts
76 accounts for —> alleviates

References

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