Response to Reviewer 3 comments

[Review comments in Heading 6 style

Our responses in Normal text style]

This manuscript outlines the protocol for a proposed model intercomparison project (MIP) to study the Intertropical Convergence Zone (ITCZ). The proposed ITCZ-MIP has a series of experiments (separated into two tiers) as well as a concrete list of requested output from each simulation. The other portion of the manuscript is a pilot study using four models that run the ITCZ-MIP experiments. The authors demonstrate their methodology achieves the desired result of predictably altering the ITCZ width in their simulations. Their methodology does, however, create a significant change in global mean temperature, despite imposing zero global surface flux, which they show to result from changes in outgoing longwave radiation. The final portion of the manuscript covers the role of the ITCZ in a changing climate. Here, the models are far less consistent, and the authors use this inconsistency to motivate a need for more models to contribute to ITCZ-MIP.

My primary concern has to do with how this study relates to the previous tropical rain belts with an annual cycle and a continent model intercomparison project (TRACMIP). The experiment design of TRACMIP was built on the premise that the seasonality is important to understanding the ITCZ. Specifically, TRACMIP was meant to address the finding that sensitivity of the ITCZ to model changes in equinoctial aquaplanet simulations can be very different from simulations with a seasonal cycle (see Figure 1 of Voigt et al., 2016). I worry that by using equinoctial conditions for this study, the methods and conclusions drawn from ITCZ-MIP might have limited applicability to the ITCZ biases commonly found in fully coupled earth system models. Similarly, Zhou et al. (2020) showed that changes in the annual mean ITCZ width can be complicated by its seasonal nature. They found the ITCZ changes both its position and width within each season in such a way that the annual mean ITCZ appears to narrow, when the ITCZ actually widens at any time within the seasonal cycle. While there are opportunities for scientific understanding with simpler experiment designs, I would have liked to have seen more discussion about the potential limitations of the experimental design considering the lack of seasonality.

An important motivation we had for not including the seasonal cycle, which this comment made us realize we had not stated in the submitted manuscript, were observational findings of the author team. In the zonal mean over ocean, the seasonal cycle of the distribution of daily rain amount (as a function of daily intensity) is largely driven by the frequency of precipitation within the rain belts, rather than meridional migration of the rain belts, which dominates over land

(Pendergrass and Deser 2017). We will also include this motivation and citation in the introduction.

Looking back at Fig 1 of Voigt et al., (2016), this figure isn't emphasizing the seasonality in isolation or alone; but rather, seasonality is added along with a slab ocean that allows SST to freely evolve. We fully agree that prescribed SSTs are too simple for understanding ITCZ width; that's why our protocol focuses on slab ocean simulations (the one prescribed-SST simulation is just a necessary technical step to be able to run the slab ocean). We also agree that for studying ITCZ position and continental rainfall (the stated purposes of TRACMIP, in the abstract of Voigt et al., 2016), a seasonal cycle is appropriate.

Part of the knowledge gap we were responding to at the time was something we thought TRACMIP was not designed to look at, and few others (again, in 2017) were looking at, was ITCZ width. For a focus specific to ITCZ width, we thought that including the seasonal cycle could introduce a rectification problem, while excluding the seasonal cycle would help narrow the focus to the physical rain belt itself, separating the width of the rain belt from its movement over the course of the year - perhaps obscuring elements of the ITCZ width that might be important.

Zhou et al. (2020) certainly emphasize that averaging over multiple seasons complicates the picture of the ITCZ. They address this by analyzing individual seasons and basin separately, which is really all that can be done in a comprehensive configuration with continents and a seasonal cycle. Since we are not looking at the rectification of the seasonal cycle to get the annual mean, but rather simply removing the seasonal migration altogether, we think their findings actually provide motivation for removing the seasonal cycle - to get at the essence of the problem of what a band of precipitation does. Our configuration is better thought of as a single perpetual season, rather than an annual mean.

Furthermore, their complex and comprehensive configuration means that in the simulations Zhou et al., (2020) analyze, there is a potential for interaction between the location and the width of the ITCZ within individual seasons. In contrast, our equatorially-symmetric design, with no seasonal migration, removes the potential for this interactions between ITCZ width and location.

Overall, our goal with the ITCZ-MIP experimental design was to remove as many complexities as possible while still retaining the minimum necessary for ITCZ width to vary. The inclusion of a seasonal cycle would add complexity. Given the relative lack of research on ITCZ width and its role in climate, it seems sensible to start this MIP with a very idealised setup (i.e. no seasonal cycle) and understand that before adding complexity down the line (as other MIPs have done, RCEMIP being a recent example; Wing et al. 2018).

In the text, will add a paragraph about the exclusion of the seasonal cycle citing Zhou et al., (2020) and Pendergrass and Deser (2017), as well as Voigt et al., (2016; though this last paper was already cited in the original submission).

While the above concern is one I believe requires a major revision to address fully, the rest of the manuscript is well written and suitable for publication in GMD. I have a few additional minor comments below.

Minor comments

L82 "This narrower peak in SST…" should this reference Figure 4?

No, we didn't include comparisons between the Control SST and Qobs profiles in the manuscript, since that's already documented in the literature (cited in the sentence). To prevent readers from making this incorrect connection, we'll explicitly state "not shown here".

L298 "…its response is nonetheless similar to CESM1." Did you mean CESM2?

No, we meant CESM1. I think your confusion might have been that Isca and CESM2 are extremely similar in Fig. 9a (which shows the change in ITCZ width for 4xCO2). What we were trying to refer to was the slope of the lines in Fig. 9b, in which Isca has the same sign as CESM1, but the opposite sign from CESM2. Visually they don't look as similar, because of the offset in control temperature change, which we presume is due to Isca's idealized radiation.

To try and make this clearer for readers, we'll add " (in terms of the slopes in Fig.~9b)" to the end of this sentence.

L301 "This is consistent with the their relationship…" Typo

Thanks for pointing out this typo. We will remove "the" in the updated draft.

Additional References

Pendergrass, A. G., and C. Deser, 2017: Climatological characteristics of typical daily precipitation. *Journal of Climate*, **30**, 5985–6003, https://doi.org/10.1175/JCLI-D-16-0684.1.

Wing, A. A., K. A. Reed, M. Satoh, B. Stevens, S. Bony, and T. Ohno, 2018: Radiative–convective equilibrium model intercomparison project. *Geoscientific Model Development*, **11**, 793–813, https://doi.org/10.5194/gmd-11-793-2018.

Zhou, W., L. R. Leung, J. Lu, D. Yang, and F. Song, 2020: Contrasting Recent and Future ITCZ Changes From Distinct Tropical Warming Patterns. *Geophysical Research Letters*, **47**, e2020GL089846, https://doi.org/10.1029/2020GL089846.