Review

This study demonstrates a newly launched ITCZ-MIP. Its primary objective is to understand the width of the Intertropical Convergence Zone (ITCZ), its underlying control mechanisms, and the resulting impacts. Specifically, it provides pilot experiments to strengthen the scientific importance of the MIP. The pilot results begin with the narrowing ITCZ to the equatorial energy input, consistent with the theoretical framework and explains the relationship with the global-mean temperature. Furthermore, they report that the ITCZ width does not fully account for the proposed emergent constraint. Even though it is not totally surprising the failure as the emergent relationship would be coupled with various features behind the width of ITCZ, the proposed MIP would be valuable to dynamically assess the role of the width.

Overall, it is pleasure to read and review the manuscript. I have no doubt that this study will be published in this journal, and its scientific value will be substantial when the MIP provides various multi-model results. However, I believe that it has the potential to be further improved by revision or expansion of certain sections. Hence, I think minor revision is appropriate. As such, I have provided some comments below based on my reading of the manuscript. Please review the feedback provided for further consideration.

Major comments

1. Figure 1.

    1.1 I think Figure 1 should provide more information for the authors to understand both the experimental design and the basic state of the experiments. For example, the authors don't show the modified SST profile. Although they show it in Figure 4, it is not shown in Chapter 2. I think at least SST profile and q-flux profiles (both F and q_itcz) should be provided in the manuscript.

    1.2. In the current step, Figure 1 shows only q_itcz. But both caption and y_axis note it as a q-flux forcing.

2. chapter 4.1

    2.1. Why is the scatter plot not shown for Figure 7 (i.e., ITCZ width vs. Hadley cell edge)?

    2.2. CESM2 is used as a representative example for other figures; but why is Figure 8 showing GFDL-AM2?

    2.3. Chapter 4.2 explains the background hypothesis well, which helps to understand the pilot results. However, I carefully argue that it looks a bit rushed in chapter 4.1. Let's assume that the narrowing of the ITCZ is accompanied by a strengthening of the Hadley cell. The upper tropospheric momentum transport would be enhanced, resulting in the

acceleration of the subtropical jet. Previous studies consistently show that the enhanced subtropical jet provides a fertile region for baroclinicity, leading to an equatorward eddy-driven jet (e.g., Lee and Kim 2003; Brayshaw et al. 2008; Shin and Kang 2021). I think this mechanism would be a plausible explanation, or at least a possible hypothesis, in Section 4.1. Whatever the connection, the potential explanation for the effect should be mentioned in the manuscript.

Minor comments

1. “These results indicate that idealized model experiments have the potential to increase our understanding of ITCZ width.”(L8) would be too ambiguous and general implication. I think it would be better to represent meaningful insights already shown in pilot results and/or what we could learn from when the MIP is fully prepared.

2. Introduction. The authors well explain the global effects of the ITCZ (and its width) in the introduction. However, I find the regional aspects lacking. In particular, the width of the ITCZ is closely related to the hydrological cycle over tropical countries, given its sharp meridional structure. The regional aspects may be typical and natural, but they are still worth mentioning.

3. L17-29. I think these paragraphs should be reorganized. Particularly, L19-21 tells the potential impact of the ITCZ width, whereas the proposed mechanism present from L22. Accompanies with previous comments, intuitive importance could be easily shown in regional aspects.

4. L51. Is the lack of a seasonal cycle a requirement for the aquaplanet configuration? Personally, I don't agree with this. Of course, it is natural to exclude seasonality in idealized experiments. At the same time, there are a number of aquaplanet experiments that take advantage of the seasonal cycle (e.g., Kim et al. 2018; Feldl and Merlis 2021).

5. L57. I think there are more experiments which utilize multi-model aquaplanet experiments (e.g., Seo et al. 2017; Voigt et al. 2016)

6. L62. “Most simulations have a slab ocean with a 10-m mixed layer depth.” As the itcz-SST is only for the reproducing SST profile, I think this would make confusion on the experimental design.

7. L73-77. It would be great if the authors could give a more detailed reason for a single ITCZ. This is also related with the ITCZ metric. Because the author’s ITCZ metric could be appliable for double-ITCZ, more detail reason would be helpful.

8. L85-89. I think this paragraph should be shown in earlier (e.g. L53-54 where explains the slab ocean). For me, the paragraph hesitates to understand the overall experimental design, especially the role of itcz-SST experiments.

9. Figure 3. Please include definition of the line in the caption.

10. Figure 4. I think the relationship would be related to the Qflux structure that could accompany with the non-linear SWCRE. Of course, this is somewhat in-line with second potential reason. It is supported by not only the downward TOA flux is dominated by the SW, but also the GFDL-AM2 shows the largest sensitivity of global-mean temperature (the model is already known for non-linear SWCRE; e.g., Shaw et al. 2015; Shin et al. 2017). I think this possibility is also worth noting.

11. L262. "And why does the multi-model mean response (Byrne et al., 2018) differ?" I think it would be easier to explain more how multi-model mean response here.

12. Momentum Perspective. I am just wondering what the authors think about the use of momentum (e.g. the rotation of the planet) compared to the imposing Qflux. As presented by the authors, the imposing Qflux has the potential to disrupt the climatology without adding additional heat. Of course, if the planet's rotation is changed, not only the general circulation but also the thermal structure would be consequently changed. But still, in terms of additional (or artificial) heating, it has advantages. I'm not saying that the MIP should be reconsidered/organized with the momentum perspective, but it's still worth noting the expert opinion in the discussion section.

Reference

Brayshaw, D. J., B. Hoskins, and M. Blackburn, 2008: The Storm-Track Response to Idealized SST Perturbations in an Aquaplanet GCM. *J. Atmospheric Sci.*, **65**, 2842–2860, https://doi.org/10.1175/2008JAS2657.1.

Feldl, N., and T. M. Merlis, 2021: Polar Amplification in Idealized Climates: The Role of Ice, Moisture, and Seasons. *Geophys. Res. Lett.*, **48**, e2021GL094130, https://doi.org/10.1029/2021GL094130.

Kim, D., S. M. Kang, Y. Shin, and N. Feldl, 2018: Sensitivity of Polar Amplification to Varying Insolation Conditions. *J. Clim.*, **31**, 4933–4947, https://doi.org/10.1175/JCLI-D-17-0627.1.

Lee, S., and H. Kim, 2003: The Dynamical Relationship between Subtropical and Eddy-Driven Jets. *J. Atmospheric Sci.*, **60**, 1490–1503, https://doi.org/10.1175/1520-0469(2003)060<1490:TDRBSA>2.0.CO;2.

Seo, J., S. M. Kang, and T. M. Merlis, 2017: A model intercomparison of the tropical precipitation response to a CO2 doubling in aquaplanet simulations. *Geophys. Res. Lett.*, **44**, 993–1000, https://doi.org/10.1002/2016GL072347.

Shaw, T. A., A. Voigt, S. M. Kang, and J. Seo, 2015: Response of the intertropical convergence zone to zonally asymmetric subtropical surface forcings. *Geophys. Res. Lett.*, **42**, 9961–9969, https://doi.org/10.1002/2015GL066027.

Shin, Y., and S. M. Kang, 2021: How Does the High-Latitude Thermal Forcing in One Hemisphere Affect the Other Hemisphere? *Geophys. Res. Lett.*, **48**, e2021GL095870, https://doi.org/10.1029/2021GL095870.

——, ——, and M. Watanabe, 2017: Dependence of Arctic climate on the latitudinal position of stationary waves and to high-latitudes surface warming. *Clim. Dyn.*, **49**, 3753–3763, https://doi.org/10.1007/s00382-017-3543-y.

Voigt, A., and Coauthors, 2016: The tropical rain belts with an annual cycle and a continent model intercomparison project: TRACMIP. *J. Adv. Model. Earth Syst.*, **8**, 1868–1891, https://doi.org/10.1002/2016MS000748.