Response to comments from Anonymous Reviewer 2

[Review comments in gray italic style

Our responses in plain text style]

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.

The novel component of the experimental design documented in this manuscript is the intervention of additional heat fluxes that changes the ITCZ width. We chose Fig. 1 because of this, and we think adding base state information to this particular figure would dilute that purpose.

The baseline heat fluxes needed to drive a slab ocean are standard elements of any slab ocean configuration; they are not new elements we are introducing here. Furthermore, they are specific to each model - someone running simulations following the protocol would need to generate these themselves.

That said, we understand that they may be useful in interpreting and understanding the changes in ITCZ width in the simulations, though we haven't gotten to that level of depth in this manuscript (whose scope is limited to documenting the experimental design). To address this, we will add a supplementary figure documenting the baseline heat fluxes for each pilot model's itcz-slab experiment and the resulting SST climatology.

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.

Thanks for pointing this out. In terms of the calculation it is an *additional* heat flux, added to the model-specific standard heat flux (F in equations 2 and 5). We will change the y-axis and caption. Additionally, we will change it in the title of subsection 2.3.

2. chapter 4.1

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

We tried hard to keep the original manuscript focused on the problem at hand; perhaps our focus was too narrow on this point. We will add the scatterplot of ITCZ width versus Hadley cell extent in the updated manuscript.

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

In the cases where we show just one model's output rather than all 4, we chose to do this in order to focus the reader's attention on the experimental design and its effects by showing an

example of model output; including plots for all four models would distract readers with the differences among models, which in these cases were not the reason we were showing the plots. The sections of the paper that author Pendergrass led use CESM2 as the example model, because she ran the CESM2 simulations. The sections of the paper that author Watt-Meyer wrote use GFDL-AM2, because he ran the GFDL-AM2 simulations. Author contributions in terms of model simulations (who ran which model) and sections of the paper led are documented in the Author Contribution statement in the manuscript.

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 eddydriven 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.

Thanks for the comment. We generally agree with this interpretation and added the following text to this section: “These circulation responses, which coincide with a strengthening of the subtropical jet (Fig. 9 [Fig. 8 of original draft]) could be explained as follows. A narrower ITCZ implies greater poleward transport of zonal angular momentum in the upper troposphere, and hence a stronger subtropical jet. The stronger subtropical jet in turn leads to increased baroclinicity at lower latitudes and therefore an equatorward shifted Hadley cell edge and eddy-driven jet (Lee and Kim, 2003; Brayshaw et al., 2008; Watt-Meyer and Frierson, 2019).”

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.

As suggested by the reviewer, we have replaced the final sentence of the abstract with two new sentences that: (i) articulate the insights into ITCZ width gained from the pilot study; and (ii) highlight the advances expected from a comprehensive MIP focused on ITCZ width.

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.

Thanks for pointing this out. We certainly appreciate wanting to do work that’s relevant. Since our main goal with this project is to strip the ITCZ back to its essence, and we thought that the knowledge gap that might be able to be filled here was the radically idealized end of the spectrum where zonal asymmetries and land are removed from the experimental design, we are wary of misdirecting readers by indicating that we might address specifics of precipitation over tropical land. But as a long-term outcome downstream from the project, we would of course expect that better understanding of the essence of the ITCZ width would eventually also be useful in realistic settings.

We’ve added a new sentence to the first paragraph of the introduction: “The ITCZ dominates regional hydroclimate in tropical regions, which are vulnerable to even modest changes in its width, strength, or position.”

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.

The first two paragraphs of the introduction have been re-organised to more clearly separate discussions of: (i) the potential impacts of ITCZ width on broader climate; and (ii) the processes controlling ITCZ width.

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).

Thanks for pointing out this oversight. Yes it's possible for aquaplanets to include the seasonal cycle; we should have stated that we chose not to include it. We will revise this statement accordingly.

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

We cited Stevens and Bony (2013) here as a reference for the statement, "tropical clouds and precipitation have dramatically different responses to increasing greenhouse gases in different climate models, even in simulations with an aquaplanet"; this point was the main thesis of this particular paper (which was a perspective rather than a standard research article). There are plenty of MIPs using aquaplanets, but it's not obvious to us how adding such a list would advance the line of reasoning we're trying to bring the reader through.

Since this was the first use of the term "aquaplanet," we also defined the term in this sentence, and it ended up in between the statement the citation was for and the citation itself; perhaps this led to confusion. We've moved the citation closer to the relevant statement to address this.

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.

We have added text to the referenced sentence to flag the prescribed-SST simulation and that it is described in the subsequent subsection.

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.

In response to this comment we have modified the sentences referenced by the reviewer. In particular, in the modifications our aim is to highlight that although the double-ITCZ problem is an interesting one, the processes controlling the width of a single ITCZ remain debated as does the influence of a single ITCZ on broader climate. So, in our view, it is important to tackle the “simpler” single-ITCZ problem in its own right. There is a considerable body of research on the double-ITCZ problem, which we think our study will neatly complement.

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.

We will make this change in the updated manuscript.

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

Thanks for pointing out this oversight. We will add it in the revised version.

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.

Thank you for pointing this out. We will include a new paragraph noting this in the updated manuscript.

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.

The first paragraph of this section (3.3) describes the response of the ITCZ width to global warming in CMIP5 models. The goal of placing this question here was rhetorical: to exit the section at the same level as we started. We don't want to over-emphasize, though, what we think there is to learn from analyzing the multi-model mean in this case, since the variations in ITCZ strength across models are very large (in both directions and in sign; see Byrne et al. 2018 Fig 4b).

Instead, we will omit this sentence from the end of the section, since the important points about the (CMIP) climate model relationship between changes in ITCZ strength and width are already stated in the first paragraph of this section.

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.

Interesting idea! We're glad if our manuscript is sparking new ideas like this, which are potentially interesting though another MIP or an extension of this MIP with a new focus (on momentum) and new experimental design, e.g., including simulations with variable rotation rates. Though interesting, we think this is distinct enough from and outside the scope of the current work that this kind of commentary would distract from the current manuscript more than it would contribute.

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