Response to comments from Anonymous Reviewer 1

[Reviewer comments in gray italic style

Our responses in Normal text style]

The "Impact of ITCA width on global climate: ITCZ-MIP" outlines an experimental protocol for a MIP, and presents some preliminary statistics of circulation shifts, ITCZ width and strength changes, and radiative budgets. The authors then pose three questions which the MIP seems well-suited to address.

I think this paper is a fantastic opening discussion piece for the ITCZ-MIP, that the writing is mostly-well-organized, easy to follow, and well-cited, and that the figure production is exceptionally clean and consistent. I think if I were to contribute to the MIP, this paper would give me a good template to follow. I recommend publication pending minor revisions. I have a couple of minor points about scientific clarity and reproducibility and some similarly minor points about the text itself.

 # Questions/comments/suggestions

I've been around some of the discussion of this MIP as a possibility, but I haven't been active in this community lately. So my main question may be one that is obvious to anyone focusing on ITCZ widening: what is it about the proposed and prototyped experimental protocol in particular that makes it well-suited in the authors' opinion to shedding light on the mechanisms of ITCZ? The introduction mentioned work by co-author Byrne with Schneider and others, and it sounded like the MIP was going to address possible mechanisms for ITCZ width – such as they are. The two questions posed on lines 40–45. But it seems like it will do so indirectly; most of the discussion concerns inter-model differences, etc., rather than by directly addressing the relevance of proposed mechanisms.

Our initial ambition, that catalyzed the project, was to come up with an experimental design that would both allow us to study the mechanism for how the ITCZ width varied and also look at its effects on features important to global climate like the subtropical cloud decks that drive shortwave cloud feedbacks, Hadley cell width which intuitively seems likely to relate to ITCZ width, extratropical circulation, and the hydrologic cycle. We originally set out to design a MIP because those have been useful for related topics, like the ITCZ location, tropical-extratropical interactions, among many others. Also, this seemed to be a good model for a group of early career scientists to be able to collaborate together, each contributing from different models they (we) were familiar with using.

Actually coming up with a protocol that works, and in multiple models, turns out to be challenging. We found that it is hard to come up with an experimental design fit for getting at all of the questions we initially hoped to be able to answer. What we did come up with, though (described here), we think is fit for the purpose of answering some questions that are interesting - specifically, the effect of ITCZ width and its variations on global climate.

We are less certain that the experimental design described here will allow us to explore the mechanisms that control the ITCZ width itself as well as we would like. But we do think it might still be useful for shedding a little more light on the mechanisms, somewhat indirectly, especially further exploring the strong relationship between ITCZ width and strength.

In considering your comment, we realized that the first sentence of the abstract probably arose more from our initial ambitions than from what we have actually accomplished and described in the manuscript. We think revising this sentence in particular will go a long way to preventing this misdirection; it had already been excised out of the introduction.

While the protocol seems to get good bang for the buck as far as strongly shifting the ITCZ while maintaining a recognizably earth-like climate, I can't think of anything in the text that explains why the protocol is precisely what it is—or what alternatives were considered (and ultimately rejected for the prototype).

We tried a few different approaches, inspired by the literature, including adding heat fluxes directly to the atmosphere (rather than via a slab ocean), and we considered a few different ways of doing this inspired by various of the literature we cite in the manuscript. One of the notions we worked from is that anything that was hard for any one of us to do would not be very likely to be successful as a MIP, in terms of making it easy and therefore more feasible and likely for modeling groups to run. So we did not push particular hard on methods that couldn't be readily implemented in one of our models, or methods that didn't change the ITCZ when we thought they might.

So while we are confident that this method does vary the ITCZ width, it is probably not the only way to achieve the outcome, it is just the first one that worked in the models we were familiar with.

It would have been nice to say something about how sensitive the ITCZ is to heat fluxes,

The experimental design has multiple amplitudes of forcing (20 and 40 W/m2 at the equator) specifically so the sensitivity to magnitude of heat fluxes can be studied. It's extremely linear in the range we tested. It is much more linear than we expected, which we tried to state in the manuscript. When behavior is so linear, it doesn't leave a lot else to say. Effects are half as big with 20 W/m2 peak forcing as with 40.

We also tested the sign of the forcing, and were also surprised at how linear that is.

This is all reflected by the excellent fit of the linear regression lines to each model shown on the scatter plots, and is described in the text.

and how a MIP design that forced ITCZ movement some other way may be less effective because of damping from SST changes (in a coupled ocean simulation). Q-fluxes seem to be the way to go, but I recommend explaining this more clearly in the manuscript.

In a slab ocean configuration, SSTs freely evolve, so interactions between the ITCZ and SSTs are also captured in these simulations, which focus around the slab ocean. All the experiments here are run in slab ocean, with the exception of itcz-SST, whose sole purpose is to calculating the forcing for slab ocean simulations - it is not intended for interpretation or analysis.

I think the discussion of the confounding effect of global mean temperature change was very good.

It’s interesting to me that the MIP is named ITCZ-MIP, but it is so squarely focused on ITCZ width and strength, when there are other important aspects that could be addressed in an ITCZ-MIP. For example, the protocol in the submission is intentionally designed to keep the ITCZ at the equator, so another ITCZ-MIP might be necessary to analyze changes in ITCZ location. That said, the ITCZ location has been the subject of considerable work lately, so I don’t see the same urgent need to focus on it. But maybe this is really an ITCZW-MIP?

Yes, this MIP is focused on ITCZ width, and does not aim to address every aspect of the ITCZ and its changes. Every time we have tried to change the name, we have failed to come up with anything else that sticks, and we go back to calling it ITCZ MIP. Since this name, while not perfect, is already in use, we prefer to leave it as it is.

 # A few line-by-line items

Line 17: “width of the ITCZ and its changes” is syntactically ambiguous. Context helps here, but maybe re-word this sentence.

Thank you. We will change this to “width of the ITCZ and its changes in width” which we think resolves the ambiguity.

Line 26: “since” suggests that the following cited papers would be after Byrne et al. (2018), but Harrop and Hartmann (2016) is obviously not. I wonder if the citation on line 27 should be written as (Dixit et al., 2018; see also Harrop and Hartmann, 2016).

Thanks for this suggestion. This was awkward to articulate because Byrne et al (2018) talked about Harrop and Hartmann (2016)’s theory, but Dixit et al (2018) was an important update subsequent to the writing up Byrne et al. (2018). This is a good solution and we will implement it.

Line 63: “Most simulations have a slab ocean with a 10-m mixed layer depth.” I wonder whether more needs to be said about this. There’s no seasonal cycle, so maybe it’s not an issue. Is this something that future MIP contributors need to get right?

The importance of slab ocean depth in aquaplanets (with a seasonal cycle) was discussed by (Donohoe et al. 2014); our understanding is that in the middle range of slab ocean depths (like 10 m), the slab ocean depth should hopefully not affect the behavior qualitatively. Of practical relevance, a system with higher heat capacity will take longer to spin up. Basically, we don’t expect it to be the most important factor in the protocol setup, but we thought we should document some a default choice of it for the purpose of the protocol. If MIP contributors had 8 or 12 m depth, it seems like that shouldn’t be cause for concern. On the other hand, if a group were to choose 2 or 50 m instead of 10 m, then based on previous work we might expect to see qualitative changes in behavior of the system. And practically, for 50 m more spin up time would need to be excluded from the beginning of simulations.

To reflect this in the text, without dwelling on it excessively since we don’t expect it to be of particular importance, we will add a brief reference to Donohoe et al., (2014) and a mention of the expected practical impact on spin up time.

Lines 42–45, 315–330: There are five big questions in this paper. The first two, which I call A1 and A2, are said to be addressed by this paper:

A1 - What mechanisms connect the ITCZ width to the width of the Hadley cell and the location of the storm tracks?

A2 – does equilibrium climate sensitivity depend on the ITCZ width?

A1 is addressed a little bit, as the Webb and Lock (2020) hypothesis is not rejected. But no alternative hypothesis is suggested.

First addressing the comments on these two points, which were found in Lines 42-45 of the original manuscript.

The paragraph referred to by the reviewer is the final paragraph of the introduction. It is common in our field to provide an outline of the rest of the manuscript in this paragraph, which is what we were trying to do with it. So, in it, we described the sections contained in the manuscript. This manuscript has been submitted as a "model experiment description" paper; consistent with this, the main contribution is not to answer any big questions, but rather to document a model experiment. The big questions that motivate the project are not addressed in this manuscript. We do make some comments on small questions that are straightforward enough to be addressed without too much text (e.g. the effect on the Hadley cell and storm track locations).

As far as climate sensitivity, we thought it was interesting that these 4 models have differences in sign. And since the model output presented here made it possible to comment on the Webb and Lock (2020) hypothesis, a segment of the authors team was eager to do so. [That hypothesis is very relevant to climate sensitivity and not very relevant to the Hadley cell width, so we will respond on the assumption that Reviewer meant A2 instead of A1 in reference to Webb and Locke 2020.] In order to properly test the hypothesis, more analysis (e.g., full climate feedback analysis applying radiative kernels and/or cloud kernels) is required; this is beyond the scope of this model experiment description paper, and one among multiple big questions the overall project aspires to address.

The main point of the project is to understand the ITCZ width and its variations, and how they affect climate. The main point of this manuscript is to document a set of model experiments: describe why we think they might be useful, what the experiments are, our small test of them, delve into some things that made us a bit nervous but we conclude are not fatal flaws for intended purpose.

In the text, followed the Reviewer's suggestions (below) and revised these lines in the final paragraph of the introduction away from framing these as "questions" and rephrased the questions as statements. While we did not break the questions out of the paragraph and into bullets, we did number them to try and make them clearer to readers.

The other three questions, which I call B1–B3, are written as follow-ups to be addressed by contributors and users of MIP simulation data:

B1 – Something about the robustness of the strength and width of the ITCZ.

B2 – Why do models disagree on "the relationship of climate sensitivity to base-state ITCZ width, even in sign."?

B3 – What is the mechanism "…of action for the consistent changes in ITCZ width that are generated by the addition of heat via surface fluxes?"

I think the paragraphs about these questions should be revised to make the questions clear—and the sentences themselves should be stated plainly as sentences. Maybe even express them in a bulleted list.

Also, maybe bullet the first two questions, and discuss them in a way that the reader can't mistake them for the three follow-on questions.

Lines 315-330 are the final part of the last section of the paper; it is common to use this space to talk about what next steps follow from the research described in a manuscript. Here, we were trying to talk in some specifics that we think follow from the initial findings, that were not obvious to us when we set out on this investigation, mostly because the pilot MIP provides the beginnings of something concrete to work with. That's what we were trying to articulate here.

We've revised this to call these "avenues for further investigation" rather than questions, and revised these paragraphs overall to try and make them clearer.

Bulleted lists bring a lot of emphasis, visually, to their contents. We think they are best used sparingly in scientific papers, and that these were not the points we wanted to emphasize, so we leave them as in paragraph form. But we did revise the words that cue the three avenues we try and articulate.

Figure 7: The "1e10" at the top of the colorbar would be clearer as "×10^10" (only formatted, without the ^).

We will make this change.

Additional References

Donohoe, A., D. M. W. Frierson, and D. S. Battisti, 2014: The effect of ocean mixed layer depth on climate in slab ocean aquaplanet experiments. *Climate Dynamics*, **43**, 1041–1055, https://doi.org/10.1007/s00382-013-1843-4.