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Interactive comment on "Sensitivity of the tropical climate to an interhemispheric thermal gradient: the role of tropical ocean dynamics" *by* Stefanie Talento and Marcelo Barreiro

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We thank the reviewer for his/her constructive comments and suggestions.

We have given full consideration to the comments in the revised manuscript which includes: a discussion of the results of Green and Marshall (2017) and Schneider (2017) as well as figure modifications to make them easily interpreted by the reader.

Please find below a point-by-point reply to the questions raised. A marked-up manuscript version (with tracked changes) converted into a pdf is also uploaded as a supplement.

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Discussion paper

Anonymous Referee #1 Received and published: 24 December 2017

General Comments

The main motivation of this manuscript titled “Sensitivity of the tropical climate to an interhemispheric thermal gradient: the role of tropical ocean dynamics” is to understand how the addition of a simple dynamic ocean model in the tropics affects the climate response to idealized extratropical forcing with different signs in each hemisphere. Four coupled simulations are presented, two that are coupled to a slab ocean model (SOM) and two that are coupled to a reduced gravity ocean (RGO) model. The extratropical forcing is applied to one simulation in each of these model pairs. The main results are that the tropical precipitation shift in response to the forcing is weaker in the model with the RGO and that the seasonal cycle in the tropics has large change in response to the forcing in the model with the RGO but not the SOM. To my best knowledge, the examination of ITCZ shifts to extratropical thermal forcing with a climate model coupled to a reduced gravity ocean has not been done yet. This examination is timely given the results presented in Kay et al. (2016) and Hawcroft et al. (2017) that show a clear role for ocean dynamics in affecting tropical precipitation shifts. That said, this manuscript misses a few key studies from the past year that are very relevant: Green and Marshall (2017) and Schneider (2017). These two studies provide a mechanistic explanation for how tropical ocean circulation damps an ITCZ shift through coupling of the ocean and atmosphere by wind stress (see comments below). The results in this manuscript support the arguments in these two papers, and I believe that further discussion in light of Green’s and Schneider’s arguments would strengthen this manuscript. Otherwise, I find this manuscript clearly argued and written, and I only have a few other minor comments, mostly regarding figure quality and additional related literature. I recommend this paper for acceptance pending a major revision to include discussion of Green and Schneider and the minor issues listed below.

Specific Comments

Major comment 1:

Green and Marshall (2017) (DOI: 10.1175/JCLI-D-16-0818.1) and Schneider (2017) (DOI: 10.1002/2017GL075817) have both shown that an ITCZ shift is damped by tropical ocean circulation, specifically by meridional heat transport through Ekman transport by the subtropical cells. Because the Ekman transport is driven by the trade winds, the ocean circulation is coupled to the Hadley circulation and the ITCZ. The heat transport by the subtropical cells decreases the amount of heat that needs to be transported by the Hadley circulation because both are transporting heat in the same direction. Green and Schneider have both quantified (in their model frameworks) how much the ocean circulation damps the atmospheric circulation. Here, comparing the models with the SOM and the RGO (which includes a representation of Ekman transport) is an additional test of Green's/Schneider's arguments. Green/Schneider's work suggests that the model with the RGO should have less of an ITCZ shift in comparison to the model with the SOM, and that is exactly the result presented here. This manuscript provides a nice confirmation of these previous results, and should be presented as such. If possible, it would be useful to calculate how much the ocean damps the atmospheric heat transport in the RGO as compared to the SOM (a factor of 4 in Green and 3 in Schneider). I also suggest adding discussion of Green's/Schneider's results to the introduction and summary/conclusions, and possibly to a couple of these other relevant sections in the text: L168-169, L171-172, L181-183, L270-274.

********** Agreed. Discussions about the results of the two mentioned papers have been included in the Introduction and Summary and Conclusions sections. Also, the damping of the atmospheric heat transport and energy flux equator shift have been calculated (1.9 and 1.5, respectively) and included in the text, in section "Results, annual means". **********

Minor Comment 1 (Figure quality): Please make the figures more easily digestible for the reader by placing key information about what is displayed in each panel both on

the panel itself and in the caption. The closer the visual information is to a description of what it is, the less hunting the reader needs to do and the less likely the reader will become confused or give up. This includes: labels with units on the colorbars themselves (all colorbars lack this) and labels on the panels that state which experiment and model are shown in that panel (e.g., for Fig 1a. could have a label that says "NOAA SST, Pacific" or Fig 4a. could have a label that says "Forced_slab"). Also, in figures with maps it appears that much of Central America is missing. Is this missing in the model or just an artifact of the plotting?

********** Agreed. All the Figures have been modified to include: headings with experiment name or other key relevant information and the corresponding units in the colorbars. Regarding the longitude-latitude maps: The land-sea mask plotted is the one that the model uses (Central America is missing with this model resolution). We added 1 sentence in the text clarifying this. **********

Minor Comment 2 (L23-26): A bit deeper explanation of the mechanism behind this heuristic would be useful for the readers who are not familiar with this energetic constraint on tropical precipitation. This will probably be useful when adding the explanations in Green/Schneider. There are also numerous additional citations that could (should?) be added here. A couple are: Zhang and Delworth (2005) (DOI: 10.1175/JCLI3460.1), Broccoli et al. (2006) (DOI: 10.1029/2005GL024546), Schneider et al. (2014) (DOI: 10.1038/nature13636), Bischoff and Schneider (2014) (DOI: 10.1175/JCLI-D-13-00650.1), Seo et al. (2014) (DOI: 10.1175/JCLI-D-13-00691.1), Woelfle et al. (2015) (DOI: 10.1002/2015GL063372). Additional relevant references can be found in these papers.

********** Agreed. A couple of sentences have been added to clarify this in the Introduction section. Also, the references of Broccoli et al. (2016) and Schneider et al. (2014) have been incorporated to the manuscript. **********

Minor Comment 3 (L59-60): This sentence is slightly misleading – there has been much work done using a hierarchy of atmospheric models to test these ideas. A couple are: Shaw et al. (2015) (DOI: 10.1002/2015GL0660270), Seo et al. (2014) (DOI: 10.1175/JCLI-D-13-00691.1), Maroon et al. (2014) (DOI: 10.1175/JCLI-D-14-00188.1). Please rephrase to state more clearly that fewer studies have examined this topic in a hierarchy of ocean models.

********** Agreed, the sentence has been rephrased. **********

Minor Comment 4 (L60-65): There are an additional two studies that have tested similar ideas as Kay and Hawcroft: Mechoso et al. (2016) (DOI: 10.1002/2016GL071150) and Tomas et al. (2016) (DOI: 10.1175/JCLI-D-15-0651.1)

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Agreed, Tomas et al. (2016) has been added as reference. **********

Minor Comment 5 (L79): Please state in a bit more depth exactly what the simplified atmospheric physics are. As the complexity of atmospheric physics affect the magnitude of an ITCZ shift to extratropical forcing, an additional sentence briefly stating what these simplifications are is warranted.

********** Agreed. The following text has been added to the model description: "The model includes parameterizations of: large-scale condensation, shallow and deep convection, shortwave radiation (using 2 spectral bands), longwave radiation (using 4 spectral bands), surface fluxes of momentum, heat and moisture, and vertical diffusion" **********

Minor Comment 6 (L84,87): What are the ocean heat flux corrections derived from? Observational datasets? A fully-coupled version of this model?

********** The correction is derived from a previous model simulation in which all the parameters and settings are identical but in which observed SSTs are prescribed (in this case observed SSTs for the 30-years period 1979-2008). In the text the sentence has been modified to include this informations in the following way: “We analyze the outcomes of coupling the AGCM with two ocean models of different complexity. In the first configuration the AGCM is coupled with a slab ocean model; a monthly-varying ocean heat flux correction (derived from a previous 30-year model integration with identical settings but with prescribed observed SSTs) is imposed in order to keep the simulated SST close to present-day conditions.” **********

Minor Comment 7 (L210-211, Figure 9/10): Because of the chosen figure scale, the eastward/westward anomalies referenced in this sentence are not visible, which makes this statement confusing.

********** Disagreed. The anomalies referred in the text are easily seen in the mentioned Figures. **********

Technical Corrections

L241, 243, 246: Incorrect figure references

********** Agreed **********

L249, 298: extra commas in citations

********** Agreed **********

L270- 271: awkward grammar

********** Agreed

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L291: be careful using the word significant if not conducting statistical significance tests

********** Agreed

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Figure 11: The caption incorrectly identifies 2 panels when there is only 1.

********** Agreed, the caption has been modified. **********

Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2017-113/esd-2017-113-AC1-supplement.pdf

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Discussion paper