acp-2016-340: Changes in the Width of the Tropical Belt due to Simple Radiative Forcing Changes in the GeoMIP Simulations

Response to RC1

The authors thank Reviewer #1 for their time and their suggested revisions. Regarding major changes, we have updated figures so that they are color-blind-friendly and have added additional discussions of relevant previous work. We have also expanded the discussion of the seasonality of the width changes.

This study examines the response of the width of the tropical belt to an abruptly applied 4xCO2 forcing and an abruptly applied 4xCO2 forcing that is balanced by a decrease in the solar constant ("G1 experiment") in 9 CMIP5 models. The authors find that the tropical width responds unevenly to identical forcing across seasons and hemispheres. The response of the tropical width is correlated strongly with the response in global-mean surface temperature and the attendant increases in subtropical static stability, tropical upper tropospheric temperature, and Arctic surface temperature.

Overall, this paper is very well done. The text is written very clearly, and the figures are straightforward to interpret. What is particularly novel about this study is the usage of the GeoMIP experiments to demonstrate a linkage between tropical belt expansion and global-mean surface temperature. My main criticism of this paper is that the authors fail to compare their results to a number of recent studies that have already examined simplified climate forcings in comprehensive global climate models, including the exact same abrupt 4xCO2 CMIP5 experiments that were examined here. The authors' assertions that "[no previous studies] have examined how comprehensive climate models respond to simplified climate forcings" (lines 8-9) and that "what is lacking is a study that applies simple climate forcings in clearly designed experiments to fully-coupled models" (lines 106-108) are too strong in my opinion. In many aspects, this paper is written more clearly and goes farther than previous studies, but I think it's important to put the new findings in much better context of previous work on the subject. Suggested revisions are detailed below.

Minor Revisions

GENERAL: As stated above, a greater cross-comparison of results with previous studies that used simplified climate forcings is warranted. A handful of these studies have already addressed the tropical expansion issue in some detail:

a) Polvani et al. (2011) force CAM3 with a (2000-1960) greenhouse gas forcing only and find a similar seasonality to the Southern Hemisphere Hadley cell edge response documented here (see their Fig. 13e).
b) McLandress et al. (2011) force CMAM with greenhouse gas forcing only and find no seasonality to the Southern Hemisphere Hadley cell edge response (see their Fig. 8).
c) Grise and Polvani (2014) use the abrupt 4xCO2 experiments from 23 CMIP5 models and find a strong correlation between the magnitude of Southern Hemisphere Hadley cell edge

expansion and the global-mean surface temperature response during all seasons (similar to what is found here). A recent paper by the same authors addresses the influence of global-mean surface temperature warming on Northern Hemisphere Hadley cell edge expansion (Grise and Polvani 2016)

d) Vallis et al. (2015) use the 1%/year CO_2 increase runs from 35 CMIP5 models and find little correlation between global-mean surface temperature warming and the magnitude of Hadley cell expansion (see their Fig. 21)

The authors thank the reviewer for these suggestions.

In response to this general comment, we agree this statement concerning idealized experiments in comprehensive models is too strong. The neglect of these papers was unintentional, and we thank the reviewer for listing these references. We have added a discussion of these papers so that our work is better situated in the context of previous work (see lines 108-117).

Line 39: You might want to clarify here that the strength of the Hadley cell is actually projected to weaken in a warming atmosphere. (Vecchi and Soden 2007)

This has been clarified by referencing Vecchi and Soden (2007) as well as Mitas and Clement (2006).

Line 137: I'm surprised that the circulation metrics adjust to the abrupt forcing in only two years. The point of this paper is that the Hadley cell edge responds to global-mean surface temperature warming, but the global-mean surface temperature warming continues throughout the duration of the 140-year run (as the ocean temperatures slowly warm). More could be said about this apparent contradiction.

Our analysis focused on the equilibrium response. Additionally, our discussion of the results in the initial submission did not argue for a mechanism of Hadley cell expansion but instead a consistent scaling across some climate parameters. We have clarified in the discussion of results that Hadley cell expansion and thermodynamic changes scale but only in the *equilibrium* and not *transient* sense, and that the timescale discrepancy rules out a direct thermodynamic mechanism (lines 399-404). We have also changed the title of the section investigating these correlations to "Intermodel differences in the tropical width response and associated thermodynamic changes".

Line 147: "are" is repeated twice.

Thank you, this has been fixed.

Line 175: "Models with more equatorward edge latitudes in one hemisphere have more equatorward edge latitudes in the other hemisphere." It might be useful to provide the correlation value here.

We have noted this (R^2=0.7, now on line 188).

Line 197: Could the non-uniform stratospheric cooling be due to variations in the strength of the Brewer-Dobson circulation, for example?

We have added a discussion noting this as a possibility (now on lines 207-209).

Lines 199-201: This is consistent with IPSL-CM5A-LR having one of the higher climate sensitivities of the nine models examined, and CCSM4 have one of the lowest. It might be useful to note somewhere on Figure 2 the climate sensitivities of the models.

Thank you for this suggestion, the equilibrium surface temperature responses have been added to each subplot for the 4xCO2 and G1 experiments.

Line 262-263: The lack of robustness in the Northern Hemisphere tropical expansion could reflect the compensating effects of two large robust responses, the effect of warming land on the tropical circulation and the effect of warming ocean on the tropical circulation (see Shaw and Voigt 2015).

Yes, this could certainly reflect these competing processes. This reference has been added to lines 274-277, thank you.

Line 274: The upper stratospheric cooling appears to be similar in the two subsets of models. It's just the lower stratospheric cooling that varies.

This is an interesting point that we had not appreciated – this certainly explains why the differences are also not significant. We have noted this in the text on lines 285-286.

Line 326: "The change in" is repeated.

Thank you, this is fixed.

Lines 389-402: Another potential mechanism to mention here is the upper tropospheric-lower stratospheric meridional temperature gradient. Certainly, increased subtropical static stability and increased tropical upper tropospheric temperatures go hand in hand. But, cooling in the polar lower stratosphere can shift the circulation poleward (e.g., Butler et al. 2010), and this has nothing to do with tropical heating or static stability. Both factors though change the meridional temperature gradient near the tropopause.

Yes, we agree and have noted this further possibility on lines 412-416.

Table 1: Why are the radiative forcings listed in Table 1 different than those documented in Table 1 of Forster et al. (2013) for CMIP5 models (4xCO2)?

These are the actual equilibrium radiative forcings for 4xCO2, whereas the table in Forster et al. (2013) displays the radiative forcings for a doubling of CO2 only. We use the values from Hunneus et al. so that the forcings from the G1 experiment can be directly compared.

If the forcings in Forster et al. are doubled they equal the forcings listed here and in Hunneus et al. 2014.

Figures 2 and 3: I believe that IPSL-CM5A-LR is mislabeled as IPSL-CM5A-MR.

Thank you, this is indeed in error.

Figure 6: Are these figures composited about the total width of the tropics (NH + SH)? If so, have you tried compositing about the NH and SH tropical edges separately? Are the results similar? Would you get the same composites if you subset the models by their global-mean surface temperature increase (instead of their Hadley cell widening)?

This is a good question. Yes, these are composited on the total change in width. There is not a substantial difference between the separate composites on Northern and Southern Hemisphere expansion, which is ultimately the reason we only show the composites on total width. However, there is slightly less dependence of the individual hemisphere's expansion on stratospheric cooling. We have noted this in the text on lines 286-288. For compositing on the change in global-mean surface temperature, the plots are essentially identical to Fig. 6 (this is probably apparent from Fig. 9).

Figures 7-10: How do these relationships vary seasonally? Are the correlations uniform year-round, or do they have a distinct seasonality?

There is indeed a seasonality to the correlations which we have not commented on. The existing discussion of seasonal expansion generally reflects the seasonality of the correlation between the change in global-mean surface temperature and seasonal expansion.

We have briefly noted some of the correlations in the text on lines 338-342. To summarize here, for the correlations between expansion and global-mean surface temperature, in the Southern Hemisphere the correlation is strongest in MAM (R^2=0.43), the season with the strongest mean expansion. A similar result is found for the Northern Hemisphere – the strongest correlation is in SON (R^2=0.31), the month with the strongest expansion. In the other seasons, there are no significant correlations between Northern Hemisphere expansion and the increase in global-mean surface temperature – though this could probably be inferred from Fig. 5. We have commented that these correlations generally reflect the strength and robustness of expansion in each season.

Tropical upper tropospheric warming has little seasonality. Arctic warming, on the other hand, is most correlated with both global-mean and tropical upper-tropospheric temperature changes in DJF (R^2~0.63), JJA (R^2~0.65), and SON (R^2~0.76). It is somewhat less correlated in MAM (R^2~0.56), though this is generally due to the CSIRO model, which is a significant outlier (it has far more warming in MAM compared to the other models, given its modest increase in surface temperature). The magnitude of Arctic warming is lowest in summer and highest in winter, consistent with previous research. For brevity we have only noted that these indices are correlated with the change in global-mean surface temperature seasonally on line 352.

Going to finer timescales necessarily reduces the magnitude of the correlations. However, in general, models with a stronger response in one of these measures of climate have a stronger response in the others.