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Interactive Comment

Interactive comment on "The equilibrium response to idealized thermal forcings in a comprehensive GCM: implications for recent tropical expansion" *by* R. J. Allen et al.

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Summary

The authors impose a variety of thermal forcings in a comprehensive GCM in order to better understand observed tropical expansion. As in previous research, they find a poleward shift of the tropospheric jets for stratospheric cooling experiments and an equatorward jet shift for heating at high latitudes; they also find that mid-latitude heating produces a poleward jet shift while tropical heating alone produces no shift. They propose a new metric, the Expansion Index, which accounts for over 70% of the variance in annual mean jet displacements in model experiments. This index is essentially

a measure of how much the mid-latitudes warm compared to other latitudes.

General Comments

As the lead author of a study that was cited in this manuscript, “The steady-state atmospheric circulation response to climate change-like thermal forcings in a simple general circulation model” [Butler et al., 2010], this reviewer was certainly interested to read of similar and more in-depth experiments conducted in a comprehensive GCM. I felt the authors did an excellent job of both conducting and presenting a variety of experiments to test the impact of thermal forcings on tropospheric jet/tropical width expansion. The tables are useful and clearly presented with significance testing when relevant. I like that different seasons and different metrics for tropical expansion were considered. The figures are also visually appealing. The analysis regarding the authors' new Expansion Index and the importance of mid-latitude warming is interesting and worthy of publication.

The main area where I feel the authors' discussion or interpretation could be strengthened is Section 4. I'm not convinced by the argument that it is mostly “thermal wind” and then in some seasons “wave-mean flow interaction” is important (this is what I took away from Section 4- if this is not the argument than your proposed mechanism is not clear to me). The tropospheric zonal wind responses are largely barotropic, not baroclinic, in nature and cannot be explained by thermal wind alone. If meridional temperature gradients are changed, and the flow is changed, waves/eddies are necessarily changed as well (and season shouldn't matter in the troposphere- the annular modes and the wave-mean flow interaction necessary to maintain them are present year-round). I would argue (admittedly with some bias) that the authors' results provide support for the Butler et al. 2011 hypothesis (or see Lu et al. 2010)- that the impact of various heatings on the tropospheric baroclinicity drives down-gradient changes in eddy fluxes of heat. The changes in eddy fluxes of heat near the surface have to impact EP flux convergence aloft and thus the flow aloft. I think the authors need to reassess their proposed mechanism; or else instead of arguing that the proposed Expansion

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Index explains "why" the jet expansion occurs, focus on the fact that it's an improved "metric" for estimating jet expansion.

Specific Comments and Technical Corrections

1) p. 31647, line 6. Change first "warming" to "temperature changes" as cooling experiments in the stratosphere were also conducted. Line 8- change "polar warming" to "polar surface warming".

2) p. 31647. Line 8-9. This is not the best place for these references (it's also not "a similar response"- it's the exact same response as the same forcing was used in each study). I will indicate later on in these comments where these references might be more appropriate.

3) P. 31647- Along with these mechanisms, you may briefly want to include recent studies discussing the impacts of SST/surface temperature gradient changes on Hadley cell expansion/jet shifts, such as Brayshaw et al. 2008 (JAS) and Chen et al. 2010 (GRL), particularly because these have direct relevance to the authors' suggestion that mid-latitude temperature gradients/baroclinicity appears to be closely related to the width of tropical expansion.

4) Section 3.1.2- it seems a little strange to pick MAM and NH jet displacements to focus on when in reality most of the high-latitude ozone depletion will be in the SH in SON (though I realize that here the forcing is symmetrical in both hemispheres- though the topography is not).

5) P. 31653, line 15- the maximum spring response is likely due to two factors: I agree about the presence of solar radiation. What is meant by "zonal flow conducive to strong planetary wave-mean flow interaction"? I assume you mean that the stratospheric flow is still westerly that time of year, at least potentially through April (unlikely in May). This might be stated more explicitly.

6) P. 31653, line 23-25 and Figure 3- how much (what percent) of the zonal wind

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response in Fig 3 can be explained by the NAM/SAM?

7) P. 31654, line 6- “an increases” should be “an increase”

8) P. 31655, line 1-5- It might be mentioned that in fully coupled ocean-atmosphere models (and in the observations), the SH won’t have nearly as much warmth at the surface as shown in these figures due to the uptake of heat by the Southern Ocean, which will certainly affect how much jet shift occurs.

9) P. 31656, lines 4-6; P. 31660, line 7-8; and p. 31655, line 18- - as stated in the General Comments, I don’t think it can be just thermal wind balance- there has to be an eddy-driven component if the jet is shifting in a barotropic manner. Thermal wind plays a role, and balance must be maintained- the winds have to become more westerly with height if the temperature gradients are increased (this is clearly what is happening in Fig 4, LTHT exp, above 200mb). Below 200mb, you generally don’t see a uniform increase/decrease in winds, but a shift in the jet that is barotropic in nature up to the height of the tropopause- this must be a response caused by eddies (this is seen in both the mid-latitude experiment, and the high-latitude experiments, at least in the NH). Thermal wind balance is still maintained because of the induced residual circulation. While the results in Fig 8 are consistent with thermal wind balance, I wouldn’t consider this a “mechanism” for driving the jet shift (to see this, theoretically one could calculate the predicted wind change by thermal wind alone for a given temperature response. The total response could be divided between a thermal wind component and a component that can only be explained by eddy feedbacks with the flow).

10) P. 31656, lines 6-10- this is where Butler et al. 2011 might be considered. In that study, the authors argue that the projection of their chosen tropical heating (the same heating as in Butler et al. 2010) onto the mid-latitude isentropes means that the baroclinicity is weakened in the subtropics but strengthened in the midlatitudes- similar to what you state here. This is ultimately what causes the poleward shift in the jet in their experiments. For whatever reason, in your tropical heating only case, the

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shape of your heating does not seem to project onto midlatitude isentropes, where the baroclinicity will be most affected (you state "small displacements of the maximum Ty generally occur for LTHTTR"). In this sense, our studies are in agreement with each other.

11) P. 31656, line 13-14- in your LTHTTR experiment, I don't see any high-latitude tropospheric warming (I see mostly negative anomalies)- do you mean high-latitude stratospheric warming (which is also the typical El Nino response in the NH winter)?

12) P. 31656, line 21-22- what is the reasoning behind adding the NH and SH jet displacements (instead of averaging, for example)? I assume it's because you want "tropical width", but jet displacement to me indicates how much the jet in each hemisphere is displaced from its climatological/control position. If you add the NH and SH changes- what does "displacement" then mean? Neither the NH nor the SH jet is actually displaced by 0.61°. I would either change the wording to "tropical extent" or else explain that "overall TJ displacement" in this manuscript means NH+SH, not that on average both the NH and SH are displaced by that amount.

13) P. 31657, line 14-15. I think the additive non-linearity (i.e. the sum of the response to individual forcings does not predict the combined response) discussed in the previous paragraph might be different that the linearity by amplitude of forcing in the CMIP3 models. This may be a good place to cite Wang et al. 2011, who also noted a very linear relationship between the amplitude of the temperature change in the tropics and tropical width/jet shift, though that was in a dry idealized model.

14) P. 31657, line 25- in our Butler et al. 2010 paper, we found a weakening and expansion of the Hadley cell circulation with tropical heating. Wang et al. 2011 also find this to be the case for a wide range of amplitudes of tropical heatings in an idealized model. So, I'm not sure how to reconcile your argument that the strengthening you see in your LTHTTR runs is associated with thermal wind balance, when presumably the 2xCO2 and other CAM runs (and in the Butler et al. and Wang et al. papers) also have

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tropical warming and enhanced westerlies aloft yet show weakening of the meridional circulation. Perhaps it has something more to do with changes in atmospheric stability? Convection/moisture? Extratropical eddy dynamics? As pointed out in Lu et al. 2007, "the more accentuated tropical convective heating during El Nino usually drives a stronger and narrower Hadley Cell" compared to the more expansive tropical heating in global warming scenarios in which greater static stability is more critical factor in determining the extent/strength of the Hadley cell.

15) P. 31659, line 21- Only one study has really attributed tropical/jet expansion to increases in tropopause height (Lorenz and DeWeaver 2007), so I might just say "Prior studies" instead of "Most prior studies".

16) P. 31661, lines 18-21; p. 31662, lines 1-3; p. 31655, lines 7-8. Are you using "wave-mean flow interaction" interchangeably here with stratosphere-troposphere coupling? (strat-trop coupling may be a better term for it, as wave-mean flow interaction doesn't necessarily involve the stratosphere at all). Why/how would wave-mean flow interaction interfere with the EI relationship to jet displacement? I'm thinking that perhaps in DJF in the NH for these particular experiments, the NH is colder in the stratosphere with a stronger polar vortex, which is associated with a poleward shift in the tropospheric jet due to the stratosphere-troposphere coupling that time of year (is this the case?). Why does the SON relationship in the SH between jet shift and the EI seem quite linear compared to the active seasons in the NH? Additionally, how does wave-mean flow interaction necessarily result in poleward NH jet displacement?

17) P. 31644, lines 15-25. I do not think the difference between our tropical heating experiments is related to the altitude of the forcing. We necessarily added the heat directly to the upper troposphere because in a dry model, it is otherwise difficult to capture the enhanced upper tropospheric heating due to temperature changes on a moist adiabat. Instead I would argue there are several major differences in our experiments that could contribute to the discrepancy: (1) Our model is dry- while in your model, moisture/convective processes could change the way the heat is distributed both verti-

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cally and meridionally (this is why you can put the heating in the lower troposphere and the largest temperature response is still seen in the upper troposphere); (2) the amplitude of the heating- we use an amplitude of 0.5 K/day, which is 5 times greater than the amplitude in your experiment. Wang et al. 2011 shows what a difference the amplitude can make; (3) Lack of topography in our model; and (4) the meridional extent of the thermal forcing and subsequent temperature response- ours is more like that shown in your 2xCO_2 experiment (Fig 6) and extends all the way to 45N/S, whereas your LTHT experiment extends to just 30N/S. As described in Butler et al. 2011, the projection of this heating into the mid-latitudes may play a critical role in the jet shift.

18) Figure 2/3- in the version I have, it's very difficult to see the different symbols for significance- in these figures, it might be more clear to just pick one level (95%) and just shade for that level.

19) Figure 5- yellow line is very difficult to see- any other color option?

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