Response to Reviewer #1

We thank Amy Butler for her comprehensive review of our manuscript. Her comments have significantly improved the quality and clarity of the paper.

Major Comments

Our basic idea was that the temperature response resembles the imposed heating. This is due to a down-gradient (oriented away from the heating) eddy heat flux response, consistent with your 2011 paper, which we now mention in the revision. If you increase T in the mid-latitudes by heating there, you shift the region of maximum baroclinicity poleward, which must shift the tropospheric jet according to thermal wind balance (TWB). Thus, our argument is that the jet responds geostrophically to the altered meridional temperature gradient. The paper's focus is on the tropospheric jet response, as opposed to the surface (eddy-driven) jet. There, eddy-feedbacks with the flow are more important, which we acknowledge in the revision. We do note, however, displacements of the surface winds are very similar to displacements of the tropospheric (850-300 hPa) jet. And that surface wind displacements correlate well with displacements of the maximum Eady growth rate (now included in the revision). Since the Eady growth rate is proportional to dT/dY, this is consistent with our argument, and consistent with the notion that storms tend to form in regions of high baroclinicity. However, additional work is required to better understand displacements of the eddy-driven jet.

We have added an additional figure (Figure 5 in the revision) that shows the thermal wind estimated zonal wind response (U') for the lower-tropospheric heating experiments. It reproduces most of actual U response (significant differences generally only occur at low-latitudes, as would be expected), adding further support to our argument. The close resemblance between U' and U is particularly true for the mid-latitude heating experiment, which we feel is this paper's most significant result—that mid-latitude heating shifts the baroclinicity poleward, which results in relatively large poleward jet displacements.

Minor Comments

All minor comments have been addressed. Specific changes of note include the following:

1. Butler 11 and Wang 12 references have been moved to a more appropriate place.
2. Brayshaw 08 and Chen 10 references have been included.

3. We agree that in actuality, maximum stratospheric ozone depletion occurs in the SH during SON. However, we choose to focus on MAM displacements in our idealized experiments because that is the season where the jet response is generally largest.
4. We agree that our equilibrium experiments will feature more SH warming than observations and coupled ocean-atmosphere GCMs, and this will affect how much the SH jet shifts. For example, compare our 2xCO2 response with that in Kushner 01. There, with a coupled ocean-atmosphere model, the Southern Ocean and high latitudes warm less, and the jet moves poleward. Our focus, however, is not on estimating the true magnitude of the jet displacement, but on understanding how heterogeneous heating affects the position of the jet, and the associated mechanisms.
5. We have estimated the tropospheric/stratospheric annual mean zonal wind response using TWB. Results are very similar to the actual wind response. This new figure is included in the revision (see Figure 5).
6. Confusion over the two different non-linearities has been clarified.
7. We've removed the explanation that the stronger MMC in $LTHT_{TR}$ is due to TWB. We do note, however, that Brayshaw 08 found a similar response—low latitude heating led to a stronger MMC. Although we do not have an alternative explanation, we do note that the weakened tropical circulation in transient GHG experiments primarily occurs in the Walker circulation, as opposed to the Hadley circulation. And this response is generally explained in terms of a thermodynamic argument. Circulation must spin down if boundary layer moisture increases at a faster rate than precipitation. So comparing the GHG response to $LTHT_{TR}$ may not be a robust comparison. In your model, there is no moisture. So what causes the weaker MMC? As you point out, your tropical heating experiment extends to 45N/S, and this, in turn, leads to mid-latitude warming. Maybe this is why the MMC spins down in your simulations?
8. Williams JAS 2006 also uses a tropopause explanation. Lu 07 also shows a robust relationship between extratropical tropopause height and expansion of the Hadley cell. Held and Hou 80 also show the meridional extent of the Hadley cell is related to the height of the tropical tropopause (for axisymmetric circulations).
9. Our stratospheric pathway for NH poleward jet displacement has been clarified. We show that most of our experiments are associated with an increase in wave refraction—particularly in the NH during DJF/MAM—which is associated with increased poleward momentum transport and EP flux divergence at mid/high latitudes. We have replaced "wave-mean flow interaction" and "tropospheric-stratospheric coupling" with "wave-modulated stratospheric pathway".

   We expect this pathway to be most important in the NH, as opposed to the SH, because planetary waves are more prevalent in the NH due to land-ocean contrasts and topography, which lead to zonal asymmetries in the flow. We also expect this pathway to be most important during the time of year such waves can propagate into the stratosphere (DJF/MAM for the NH). Based on our experience with CAM, the model appears particularly sensitive to such interaction through MAM, which may be related to a fast westerly wind bias. For example, May zonal winds at 60N and 100 hPa are about 8 m/s compared to 4 m/s in reanalysis. We also note that your experiments may have a muted stratospheric pathway-type response due to lack of topography (and hence, more zonal symmetry).

We are not suggesting the stratospheric pathway interferes with the Expansion Index. We are noting that the EI-jet relationship appears weakest in DJF/MAM for the NH, which is when the stratospheric pathway appears to be most important. Regardless of the cause, if the jet moves poleward, there should be a tropospheric adjustment to the temperatures such that the baroclinic zone also moves poleward. Which is what the EI is measuring. We do note the EI is a simple metric that looks at the relative warming in three, broadly defined latitude bands based on the traditional definitions of low-, mid- and high-latitudes. Perhaps other latitude bands are more important during NH winter, when the jet is farthest equatorward. It is also possible—as you suggest—eddy feedbacks to the flow are more important during NH winter, when baroclinicity is largest. Our main point, however, is that we are able to explain up to 70% of the tropospheric jet displacements across a wide range of simulations, with a relatively simple metric, the Expansion Index.

10. Possible reasons for the different tropical heating responses between this work and Butler 10,11 has been modified. The difference does not appear to be related to the magnitude of the heating/temperature response. A newly conducted LTHT4x$_{TR}$ experiment also features negligible poleward jet displacement (-0.06 in the NH and 0.26 in the SH). We note that our LTHT2X$_{TRML}$—which features heating from 0-60 N/S—yields significant poleward jet displacement. Since your tropical heating experiment features heating up to 45 N/S, comparing it to our LTHT2X$_{TRML}$ seems more appropriate.

Response to Reviewer #2

We thank Reviewer #2 for their thorough review of our paper.

General Comments

We no longer suggest the EP flux explains the poleward jet displacement in some of our experiments. We note that our analysis of the wave refraction versus jet displacement relationship was not limited to Figure 3. We attempted to quantify the relationship over all of our global warming experiments, and the suite of stratospheric cooling experiments, in addition to all 2xCO2 CMIP3 models for all seasons. The NH wave refraction changes were included in Table 6. We also note that we estimated the correlation coefficient between the change in wave refraction and jet displacement. The corresponding correlation for all 2xCO2 CMIP3 models is 0.44 in DJF and 0.43 in MAM. These are the two seasons we suggest the stratospheric pathway may be important in poleward jet displacement in response to global warming. Again, however, we acknowledge we have not shown causality. We do retain some of our EP flux calculations and use it as a diagnostic tool to measure the importance of the stratospheric pathway.

We have also estimated the zonal wind response using thermal wind balance. A new figure in the revision shows the estimated zonal wind response is similar to the actual zonal wind response for the suite of LTHT experiments. Thus, a geostrophic adjustment to the altered meridional temperature gradient appears to account for most of the response.

We have also added GFDL AM2.1 lower tropospheric heating (LTHT) experiments. Results are similar to that based on CAM, suggesting the responses are robust—particularly the poleward jet shift in response to mid-latitude heating.

Specific Comments

1. We no longer use the acronym "TJ". However, the reason for using the acronym was to emphasize that our definition of the jet does not distinguish between the subtropical jet and the mid-latitude eddy-driven jet. Our jet definition, however, primarily represents the subtropical jet since we use all tropospheric pressure levels from 850-300 hPa. We also note that most studies use the surface wind as a metric for the mid-latitude eddy driven jet, which we do not include. To make things more clear, we have separated "Measures of Tropical Width and its Changes" into two sections, "Tropospheric Jet" and "Other Measures".

2. Our intent is not to evaluate the magnitude of actual changes, or the actual magnitude of seasonal responses, but to gain an understanding of how heating/cooling of certain latitude bands affects the location of the tropical

circulation. Our choice of a uniform 10% ozone reduction was based on Newchurch et al., 2003. Based on their Figure 1, the actual change over this time period is larger, especially for the SH. We acknowledge that these values are for the annual mean, and do not include seasonal variations. We have added a sentence stating this in the revision.

3. We realize this is a subtle issue and have revised the manuscript to clarify the issues. We have not changed our measure of the jet, as we believe it primarily represents the subtropical jet and we show that results are not qualitatively sensitive to the various measures of the jet or other measures of the tropical edge. For example, displacements of both our 850-300 hPa jet and the surface jet are highly correlated (based on CMIP3 2XCO2 experiments, r=0.83 in the NH and 0.90 in the SH). We have since found a significant correlation between displacements of the surface jet and the maximum Eady growth rate. Since the Eady growth rate is proportional to dT/dY, it appears displacements of both the tropospheric jet and the surface jet is related to corresponding displacements of the latitude of maximum dT/dY. We also note that most studies actually do not use the 850 hPa wind as a measure of the eddy-driven, mid-latitude jet. Most studies use the surface wind for such purposes, as we have done. The focus of our paper, however, is not on the mid-latitude eddy-driven jet. Additional work is required to better understand displacements of this jet.

4. We've changed "stratospheric vortices" with "stratospheric zonal winds".

5. We acknowledge that we cannot separate cause and effect when it comes to the EP flux and jet displacements. We have toned down this section and no longer suggest the EP flux explains the jet response.

6. We do not claim tropical heating yields equatorward jet displacement. Although the annual mean jet displacements for tropical heating are negative, they are small and not significant. Moreover, $LTHT4x_{TR}$ yields SH poleward displacement (although not significant). Our conclusion was that tropical heating yields negligible jet displacement. Our CAM results for $LTHT_{TR}$, $LTHT_{ML}$ and $LTHT_{HL}$ are robust to model choice. We have since conducted analogous experiments with the GFDL AM2.1 GCM. Similar results are obtained, particularly for the mid-latitude heating experiment. The revised manuscript includes the new figure based on GFDL AM2.1.

7. The point of Figure 5 was to introduce the following results: Experiments that feature poleward (equatorward) jet displacement also feature poleward (equatorward) displacement of the latitude of the maximum meridional temperature gradient (T_y). This is why Figure 5 showed displacements of T_y, to complement the jet displacements shown in Table 4. As with nearly all of

our plots, this figure showed significance with symbols. And as can been seen, most of the responses that are significant at 850 hPa are also significant throughout most of the troposphere. We apologize if the confusion was related to the small size of our symbols; the revision features larger symbols. We also note the correlation between displacements of the jet and the maximum T_y is 0.81 in the NH and 0.92 in the SH. To more clearly show the correspondence between T_y and the jet, we have also added the corresponding displacement of the jet (as a function of pressure level), in addition to displacements of T_y, to this figure (Figure 4 in the revision).

8. If the 850 hPa response is significant, it is generally significant throughout the troposphere. Please see Figure 4 in the revision.

   We now place more emphasis on the different responses between low-, mid-, and high-latitude heating. For example, we now include LTHT results from the GFDL model. As this paper stated, we are also in the process of writing a companion paper that focuses on the effects of (more realistic) mid-latitude heating.

   We apologize for the mistake in the SH T_y response in Figure 5 and thank the reviewer for spotting this. It has been corrected and the noted T_y displacements are more consistent with the jet displacements in Table 4. As we acknowledged in the manuscript, the SH jet displacement in our CAM 2xCO2 and 8xCO2 experiments is small, unlike the CMIP3 ensemble response (e.g., CAM 2XCO2 SH jet dY = 0.09° versus 0.73° in CMIP3 2xCO2). We do not have an explanation, but we did note that other metrics, including P-E and MMC, show greater SH poleward displacement, in better agreement to CMIP3 (but still smaller, as shown in Table 5). Interestingly, the NH response in CAM3 2xCO2 is quite a bit larger than that in CMIP3 2xCO2 (e.g., 1.08° versus 0.46°).

   While it is surprising the results are so nonlinear, we performed long runs to ensure that this was not the result of sampling variability, and this is reflected in the uncertainty estimates.

   We also note that we have conducted transient CO_2 CAM experiments over 50 years (1970-2010) using 10 ensembles and obtain similar results—minimal SH jet displacement. So it appears CAM3 is less sensitive to CO2-induced SH poleward jet displacement, relative to other CMIP3 models.

9. We acknowledge the shortcomings of using the EP flux to account for cause and effect and we have toned down saying our EP flux analysis explains the response. We actually showed (Table 6) the wave refraction as a function of season, for most of our experiments, as well as that for all 2xCO2 CMIP3 models. We also quantified the wave refraction versus jet displacement relationship for several seasons via correlation. Although it is true we only

showed one figure with the EP flux response, we attempted to quantify the EP flux response for all experiments, including all of 2xCO2 CMIP3, over all seasons.

As explained in the text, our experiments were conducted for 70 years, the last 30 of which we use in our analysis. This is longer than most similar studies and should be sufficient to obtain a significant climate response, if one exists. This was also the rationale for conducting not only the suite of LTHT experiments, but also the suite of LTHT2x experiments. Double the heating rate (0.2 K/day) results in a similar, but more significant response relative to our baseline heating rate (0.1 K/day).

10. The latitude-restricted heating experiments were not shown in Figure 9 because they were used to develop the Expansion Index. Nonetheless, we have added these experiments to the figure, as small symbols. The $LTHT_{ML}$ and $LTHT_{HL}$ experiments fall on opposite sides of the zero line. For the latitude-restricted heating experiments, EI accounts for 94% of the variation in annual mean jet displacements.

11. We thank the reviewer for pointing out this useful simplification. It has been added to the revision.

12. We did indeed check that as suggested, but had not included a figure demonstrating how geostrophic the responses are. This figure is now included in the revision (Figure 5 in the revision).