

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.

Anonymous Referee #2

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General comments:

This is an interesting manuscript which surely deserves publication. It tries to shed light on the recently reported tropical expansion, by performing GCM simulations with “idealized” forcings meant to elucidate the mechanisms. This exercise, albeit with some inevitable limitations, is surely worth publishing.

My main concern about the paper is that it claims to offer explanations for the expansion observed in the model which I find largely unconvincing. Just to give one example, from the abstract: what does this sentence mean?

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Responses to stratospheric cooling are consistent with a wave-mean flow interaction involving increased wave refraction, and downward propagation of the stratospheric wind anomaly.

First it is obvious that the signal has to come down, since that is where the forcing is: how could the anomaly propagate upwards? Second the evidence offered for wave refraction (horiz:vert EP flux ratio) is a chicken-and-the-egg conundrum: the authors argue that because the waves are refracted the response is what it is; but it is equally likely that the waves are refracted because the response is what it is; which comes first? That is the whole key to establishing causality; the cause must come before the effect. Third I don't understand what it means to be “consistent” with wave-mean flow interaction (presumably it means “not being inconsistent” with it; in other words, it could be a million different things, and one of those might be wave-mean flow interactions...!?!), nor do I understand how that conclusion is reached since the authors have not run a model with wave-mean flow interaction turned off (e.g. a zonally symmetric model – with no eddies) and therefore are not able to reach that conclusion.

Let me be clear: these are interesting results, and I recommend their publication. However, I think the authors would do themselves a favor by clearly documenting their model results while refraining from offering hand-wavy pseudo-explanations. I offer specific instances below, together with other more detailed comments.

Specific comments:

1. Do we really need the acronym “TJ”? It only saves one character since you could use “jet” in every instance TJ is used without anyone misunderstanding.
2. Section 2.2. The stratospheric cooling experiments are highly unrealistic: ozone losses have a huge seasonal cycle and prescribing a uniform-in-time 10% de-

pletion is far from mimic the actual forcing. I am not asking the authors to redo anything, but they should at least note that this choice of forcing is not representative recent ozone changes.

3. Section 2.3. I take issue with the choice of describing “the jet” by using the 850-300 hPa mean. This conflates the upper level subtropical jet, which is tied to the Hadley circulation and (presumably) the tropical expansion, with the near surface midlatitude jet which is eddy driven and may or may not be directly related to the tropical circulation. Most other studies have used the 850hPa wind max to define the midlatitude jet, precisely to avoid this confusion.
4. Last line on page 4. The sentence

the stratospheric vortexes in both hemispheres intensify in accord with thermal wind balance

makes little sense, since there is not polar vortex in the SH stratosphere in MAM. Also, for future reference: 1 vortex, 2 vortices.

5. Section 3.1.2. I found much of the discussion in this section of highly unconvincing, together with the panels in Figure 3 (which are hard to read and overcrowded with details). If these diagnostics were believed to be useful **explanations** of the jet shifts, they would have been widely adopted by now (the literature on this subject is rather large). The simple fact, sad to say, is that they are likely to be very non-robust and, in the end, they tell us very little about what is causing the tropospheric jet response to the forcing (this goes back to my main comment above). However, if the authors wish to keep this kind of “interpretation” in the paper, it is their prerogative to do so. I do not, personally, think that it adds very much.
6. Section 3.2.1. This is a most interesting result: the high-lat and low-lat tropospheric heating yield equatorward jet shift, but mid-lat heating yields a poleward

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shift. This is truly unexpected, and I wonder how much of this result is robust to the model configuration (i.e. would this be observed with other GCMs?).

7. Figure 5. Perhaps I read too fast, but why are we shown the T_y max to indicate tropical expansion? Why not the 850-300 jet maximum? And can we please put some error bars on these responses? One has no sense if any of these are statistically significant.
8. Figure 5: More importantly, I think any reader who looks at Fig 5 will conclude that (1) only at 850 hPa any “real” signal exists and (2) most of the shifts are equatorward, except for the peculiar ML case. This highly non-monotonic behavior is a real issue, that I think is not being emphasized enough.
9. Figure 5. The $2\times\text{CO}_2$ and $8\times\text{CO}_2$ simulations are also **exceedingly odd!** From Table 4 we note that (in the annual mean) the SH jet DOES NOT SHIFT, and only the NH jet does. This totally contradicts what the CMIP3 ensemble shows (Fig 1b). Could it be that this model is, for some crazy reason, a weird outlier? And in Fig 5 (top) the SH shift for T_y maximum appears to be equatorward by over 6 degrees for the $2\times\text{CO}_2$! How can we reconcile this with Table 4? And why is not the $8\times\text{CO}_2$ also plotted in Figure 5? In all honesty, I am a little lost here.
10. Table 4. Actually, I am really concerned about the robustness of most of these results. Say we focus on the $8\times\text{CO}_2$ run, where one would expect the largest (and possibly cleanest) response: the SH jet trend is nearly zero (0.03) in the annual mean, but this comes from a large cancellation between the poleward MAM trend (1.27) and the equatorward DJF and SON trends (-0.21 and -1.05). So, the seasonality of the model response is VERY large. How can then one cavalierly interpret the model response with intricate EP flux diagnostics stories (cf. Fig 7) and only show one season in one simulation? I am ready to bet that if one looks at ALL season in ALL simulations, the interpretations offered in Fig 7 will prove not to apply, in most cases. Of course, the authors cannot possibly

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show all these things, but the reader is left with a sense of utter confusion as to how this model is responding and as to the value of the diagnostic interpretations. In summary: I fear the model integrations are not long enough to give statistically significant results.

11. Figure 9. Perhaps I misunderstood something, but why are we not shown ALL the model simulations in this figure? Notably it would be good to see the ML and the HL/TR simulations, which have opposite-signed jet shifts? If the "expansion index" is truly a good predictor of the jet shift, we would expect to see the ML and HL/TR simulations to fall on opposite sides of the zero line.
12. Also, I have a little trouble understanding the new "expansion index" (*EI*). On page 7, left column, we are told that first that

$$AMP_{ML} = <\Delta T>_{30-60} - <\Delta T>_{0-30}$$

then we are told that

$$AMP_{HL} = <\Delta T>_{60-90} - <\Delta T>_{30-60}$$

and finally we are told that

$$EI = AMP_{ML} - AMP_{HL}.$$

Simple algebra the yields

$$EI = - <\Delta T>_{0-30} + 2 <\Delta T>_{30-60} - <\Delta T>_{60-90}$$

which looks like a second derivative in latitude of the temperature response. Frankly, this looks like a very odd quantity (a "curvature" of the temperature response?)

13. Last item. The first sentence at the top of page 8 (left column) says:

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Figure 9 further supports the idea that part of the jet shift can be thought of as a geostrophic adjustment to an altered temperature profile -- not only when certain latitude bands are heated, but also for global warming experiments like LTHT and 2xCO₂

This idea that the response looks like a "geostrophic adjustment" of the winds to the temperatures is repeated in several spots in the paper. What bothers me about about this is that it is unnecessarily speculative: you have the temperature response from the model integrations, so **why don't you compute** the balanced wind that accompanies those temperature changes instead of waving your hands? Maybe things look qualitatively adjusted but the actual amplitudes are off by a factor of 10: how do we know? As mentioned at the beginning of my review, this kind of speculative statements, of which the paper is replete, detracts from the otherwise interesting modeling results.

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