Review of

Replicating the Hadley Cell and subtropical jet disconnect in idealized atmospheric models

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General

The authors employ a model hierarchy approach to examine the disconnect between the position of the subtropical jet (STJ) and the edge of the Hadley cell (HC), which was found in previous work, and is in contrast to theoretical expectation. The main finding is that the disconnect occurs in a simple dry atmosphere, and therefore does not necessitate processes related to moist convection, radiation, and to some degree, zonal asymmetries in the thermal m background state. The topic is important and timely, and the methodology used is well suited for the research question at hand. However, I find that some potential criticisms of the methodology and findings are not sufficiently addressed. I therefore recommend accepting the paper after a major revision. My comments are listed below.

Major comments

1. The argument in lines 165-168 does not make much sense to me. In the upper level, the edge of the HC is found where $f\bar{v}$ vanishes. Therefore, the eddy momentum fluxes term must be balanced by the vorticity flux, rather than “dominating” the balance. This is the gist of the Korty and Schneider (2008, “Extent of Hadley circulations in dry atmospheres”) argument, which is unfortunately not mentioned in the text. Based on the Korty and Schneider argument, I would like to offer an alternative framing of the problem.

The upper-level zonal momentum balance (Eq. 2) can be rewritten as

$$f\bar{v} - \frac{\bar{v}}{a\cos\phi}\frac{\partial(u\cos\phi)}{\partial\phi} = \frac{1}{\cos^2\phi}\frac{\partial}{\partial\phi}\left(\left[\overline{u^+v^+}\right]\cos^2\phi\right)$$

Under this balance, the EDJ, STJ and HC edge should all be located, because at the latitude where $f\bar{v} = 0$ (HC edge) there exists a solution where the mean momentum flux and eddy momentum fluxes are maximal, so that their gradient vanishes there. Also, this indicates that the peak of $u\cos\phi$ should be used to calculate the positions of the STJ and EDJ. The question then becomes why is this balance not manifest in the metrics? One simple explanation is that since the STJ and EDJ metrics, as well as the ψ_{500} metric, are calculated at different levels, the lack of covariance of the STJ and EDJ metrics is in large part due to their calculation at different levels. Similarly, at the 500 hPa level, mean momentum fluxes are much weaker, and so the dominant balance is between the eddy momentum fluxes and $f\bar{v}$, explaining their closer relation. In conclusion, it is important to reject the null hypothesis that the disconnect between the STJ and HC is not a mere feature of the definition of the metrics (i.,e., calculation at different levels), rather than representing a fundamental physical property of the MOC. The authors conclude that the relationship between the STJ and HC is nuanced and

level-dependent. They should therefore convince the reader that there is more to the results than this level sensitivity of the metrics.

2. Since there is no seasonal signal in the idealized model, the concept of inter-annual variability seems artificial and potentially misleading. The variance would be strongly controlled by the relaxation constant, which is not stated, but is likely small since the authors state that 1 year is sufficient for spinup. Please justify this methodology and the physical meaning of the correlations shown in Figure 4.

3. The 200 hPa isobar in Figure 3 suggests that there are instabilities in the mean state of the MB16 simulations. This likely affects the signal to noise ratio of the results. Please comment on these or consider averaging over a shorter simulation period.

Minor comments

1. Shouldn't the title read … Hadley cell edge and the subtropical jet position disconnect …?
2. In Figure 5, it is hard to distinguish between the MB δ_z 20 and 10 simulations. Also, add in the caption, …thin vertical lines…

Comments by line

15 ~~there is~~

34 CO_2 —> changes in greenhouse gas concentrations.

94 and elsewhere What are you referring to here in terms of "accuracy"? For an idealized model, how can accuracy be defined? I can understand aiming for a mean state that mimics observed conditions. But accurate does not seem like the proper terminology to use here.

111 The rate of relaxation would be determined by the relaxation constant, which is not specified.

138 focused view of the

172 I think you what you mean here is that the relation is stronger in the winter hemisphere, in which the AMC limit is more prominent. This should be clarified.

194 The zonal winds are **more** barotropic compared to the other circulations but are definitely not barotropic.