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

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General comments:

Menzel et al., use correlations in reanalysis and a range of models to understand the disconnection between the Hadley Cell edge and the subtropical jet latitude. They argue that the disconnection is due to the STJ latitude being closely related to angular momentum conservation, whereas the HC edge is more closely linked to mid-latitude eddies. This is a really interesting study and helps address a gap in our dynamical understanding of the global circulation.

However, I would like to see more analysis or back ground literature to support your findings. Correlations are not sufficient on their own to explain the disconnect, and the correlations are moderate (around r=0.5 in the reanalysis), so could only ever be one part of the story. You do attempt to understand the mechanisms explaining the disconnect by improving the basic model by decreasing its static stability, and find that moist or radiative processes are not relevant. As you do not extend the analysis beyond this point, I was left with the impression that static stability should explain the mechanisms behind the disconnect, without an understanding of why. The profiles of static stability (Fig. 5) in the more complex WR18 and WR18Z simulations are further from that of the reanalysis in the subtropics (around 30 degrees) than the most improved (deltaZ=30) simulation. As there is no correspondingly better correlation between the different metrics (Fig 4), I am also not convinced that static stability could be the whole answer.

I admit I am not familiar enough with Hadley Cell dynamics to determine how much analysis is required, or if citing relevant literature is sufficient, or a combination of both. It may be sufficient to explain the role of static stability more fully and clearly. Alternatively, your argument could be supported by comparing rates of change with the HC edge and midlatitude eddies, or deeper analysis into the different experiments. In either case, I think this paper will be very useful once this additional information is added.

Specific comments:

Line 16: It is unclear what ‘them’ refers to. I think you mean the upper tropospheric and lower tropospheric metrics? I suggest you reword to make this clearer. Also, not clear if these metrics are for the STJ, HC, or both.

Line 40: Following from previous comment, this hypothesis motivates your study but does not clearly emerge as important from your introduction. I suggest reworking and trimming the introduction to really highlight and support why you are addressing this hypothesis.

Line 42: ‘the most idealized … model’ reads strangely. It also doesn’t tell me specifically what the model is. In line 186, the model is described as the ‘most basic idealized model’, is ‘basic’ missing in line 42?

Line 50/51. Not clear what ‘its behaviour’ refers to. ‘Its’ could be the model simulations or the STJ. I think you mean the STJ, but I suggest you reword to make this sentence clearer.

Line 60: You say you use three reanalyses, but only present the results for one. I could not find information around S-RIP that suggests averaging over these three reanalyses. Is that what you have done here? If so, you need to state this. Do the results vary across the three reanalyses?

Line 64: Which season? Later in the text you mention DJF, but it would be good to state here, as well as in the introduction and abstract.

Line 64: Is this data detrended before analysis? A strong trend in the Hadley Cell edge would correlate well with a corresponding trend in the mid-latitude eddies, and may provide misleading results about how well connected the HC edge and eddies are on an inter-annual time scale.

Line 71: Is it appropriate to use CMIP models. Are they suited for looking at large-scale circulation relationships? Why CMIP5 not CMIP6?

Line 84/Equation 1: What do the deltas δ symbolise? Delta is explained later in the text, but should be covered here.

Lines 138-140 As mentioned in earlier comment, I suggest you move this paragraph describing season used in this study to be in the methods and abstract.

Line 149, and elsewhere where relevant: There are strong seasonal differences in the HC, STJ, EDJ locations and strengths, how do these differences impact your results for the southern hemisphere vs the northern hemisphere?

Line 151 and 153: its not it's

Line 185: Are you planning on finding the physical mechanisms responsible for the disconnection? I do not think you come back to this point. I think you can exclude a couple of mechanisms (moist and radiative processes), but what might explain the physical mechanisms?

Line 195: 'does not' not 'down not'

Line 236: I think it would be good to move (or repeat) the physical interpretation of delta-z in the methods.

Lines 227 – 229 While the correlations do contrast with MB16, they are also quite different to the reanalysis, and I am not convinced being within the range of the CMIP models makes the correlation accurate. Do the statistically significant, if weak, negative correlations between the STJ and HC, and STJ and uv suggest the STJ is more eddy driven in this model? What are the implications for this moderate, negative correlation?

Line 261: This is the first time that deltaZ = 30 has been described as having an improved basic state, and you may wish to state this earlier in the text to make it clearer. How realistic is this deltaZ value? Are there implications for having a much stronger zonal wind in the higher latitudes than the other deltaZ values? The static stability is still much stronger in the tropics than in reanlysis of WR18 or WR18z; is this an issue?

Figure 2: The correlations are the model-mean for CMIP5, are they the mean across the 3 reanalyses produces in S-RIP? Please update the caption accordingly. Do you get very different correlations if you look at the individual reanalyses, or individual models?

Figure 3: It is hard to pick the temperature contours from the colour bar, making it difficult to visually compare to the model equilibrium temperatures. Could a more distinct colour bar be used (e.g. with more colours than shades of red)?

Figure 4. The correlation between Hadley Cell and uv latitudes go off the edge, I suggest you widen slightly. Do you really expect a 100% correlation at deltaZ = 10?.

I wonder if it's helpful to reverse the order of the metrics such that CMIP5 (or preferably reanalysis) is on top and deltaZ = 10 is on the bottom. I intuitively assumed the highest deltaZ was on the top and initially thought increasing static stability made the correlations stronger, the opposite to the real result but an easy mistake to make with a quick glance at the plot. Reversing the order has the added benefit of making the reference value (reanalysis) easier to pick.

Figure 5: Do you find differences in profiles for the southern hemisphere? It might be nice to add to the supplementary material as you show the southern hemisphere data in the earlier plots. To avoid confusion, I also suggest you add 'vertical' to the dotted and solid line description to clarify you are talking about the STJ and HC latitudes, not the stability or wind profiles. What does the CMIP5 profile look like?