I appreciate the authors' efforts to respond to my comments, and those of the other reviewers, and feel that the manuscript has improved considerably. I'm also very glad that they were able to catch the coding bug. (If it is any consolation, on my very first paper I was double checking a calculation during the revision phase when I horrifyingly discovered that the radius of the Earth in my code was off by a factor of 10! Fortunately I was able to redo all the calculations and the key results were unaffected.)

My chief concern with the manuscript is that it is still very long, such that some of the very interesting results (particularly the new applications at the end) might get lost. I've tried to offer suggestions on how it could be shortened, and other typographical suggestions, but leave the final decisions to the authors.

Ed Gerber

Minor comments and suggestions

1) I think it would be easier to describe the log10 coding error in terms of the parameter \delta_z in equation A1. In coding the natural log as a base 10 log, the authors' effectively reduced the amplitude of \delta_z by a factor of ln(10)=2.3, so that instead of the default value of 10 K, it was just 4.3 K. I would still explain why this happened (i.e., that it was an error in the representation of the logarithm), but you could refer to the alternative integrations as \delta_z=4.3 and fit them easily in the Held-Suarez framework. To me, this gives me a more physical sense of error: it was a reduction of the tropical stratification by about 6 K, from 10 to 4 K

I think that it would be better to introduce this alternative configuration earlier in the manuscript. An appropriate location would be when discussing the Newtonian cooling in Section 2.1, at the bottom of section 6. Here you explain the HS94 and PK02 profiles, and could mention that (inadvertantly) the \delta_z parameter was varied in some of the integrations, revealing interesting behavior that will be discussed later in the manuscript.

2) This comment is about how the change in delta_z parameter may impacts the circulation. The authors indicated that it increases the equator-to-pole temperature gradient, but I think it's primary affect might be through the stratification of the tropics and subtropics. It is true that modifying this parameter does increase the equator-to-pole temperature gradient in the upper troposphere, but the effect is on the order of a few degrees, compared to parameter \delta_y which is 60K. As I understand, this parameter was designed to increase the stratification in the tropics and subtropics, mimicking the impact of moisture. (Moist convection drives the tropics to a state of constant moist potential temperature, which imparts a dry stratification which increases with moisture content, which follows the surface temperature).

The stratification in the tropics is important in allowing the Hadley Cell to transport energy (it increase the gross stability, and hence reduces the need to transport mass to transport a given amount of energy). It may also affect midlatitude eddies, as baroclinic instability is sensitive to the stratification, particularly in the subtropics. Reducing the stratification in the subtropics would favor baroclinic instability further equatorward, and so could help explain the shift in the jet.

3) I think the paper reads better with the equations moved to the supplement but as I mentioned above, it is still very long. A number of early figures are needed just to compare against earlier results (Figs 3, 4, 6a). I do see that these are essential, but unfortunately they not very exciting, compared to the results at the end. My concern is that by the time the reader gets to the interesting new case studies in section 5, they might be rather exhausted.

There's also a great deal of information on the parameter sweep experiments in Figs 5, 9-13, and C1-C3 (which are substantially referred to in the text). It's hard for me to pinpoint what might be less important, but I would urge the authors to consider trimming or consolidating plots as much as possible, as to highlight the results you want people to remember.

To help summarize/focus the paper, would you agree that these are the key variations of the model that you consider:
i) \delta_z (stratification)
ii) stationary waves (topography vs. thermal)
iii) sponge layer

Of these, the sponge layer had the least impact on the dynamics of the stratosphere and troposphere. Perhaps this could be put in an Appendix, along with Figs 7 and 8), if you wanted to move the text faster. (This is not to say, however, that the exponential profile isn't an improvement.)

The two key control parameters that you vary are:
a) p_Tw (depth of the vortex)
b) \gamma (strength of the vortex)
These two knobs are not independent, although they do have different impacts on the UTLS region.

In terms of the results, it seems that the model's tropospheric jet stream tends to fall into one of two states: a fairly stable jet located around 30 degrees or fairly stable jet located around 40 degrees. Increasing gamma or increasing p_Tw tends to pull the jet further poleward, towards the high latitude state. When a control parameter tends to shift the jet between these states, however, you can observe regime behavior with a more variable distribution. But if the jet start too stably in the 30 degree state (i.e., with reduced \delta_z) or too stably in the 40 degree state (i.e., with thermal topography), there is no transition and the control parameters have a limited impact on the jet.

In the stratosphere it seems that three states are possible. When the vortex is very weak, there is limited variability of the vortex. When it is increased to moderate levels, the vortex is highly variable (at least ina relative sense) and the temperature is far from the equilibrium state due to a strong overturning circulation. And finally, when the vortex becomes sufficiently strong, it tends to hover near the equilibrium state, with reduced variability.

These stratospheric transitions are not necessarily tied to the tropospheric jet stream transition (as was the case in Wang et al. 2012). This does make it trickier to explain all the behavior. But if the figures could be organized to highlight these results, you might find it easier to focus the paper and trim.

4) I feel the case study in section 5.1 is ideal for highlighting why this new EMIL model will be so useful. To my knowledge, this examination of the impact of dynamics on photolysis would simply not be possible in other models. This was a very interesting section.

I felt the monsoon case in section 5.2, however, to be less developed. For example, I found myself wondering about the statistical robustness of the eddy shedding and splitting in Figure 17. More importantly, it seems that this sort of work could be done with any dry dynamical core with HS94 style forcing. I would suggest to consider saving this monsoon case to another paper, when you can highlight the troposphere-stratosphere tracer transport, a topic that would be much harder to explore with a traditional dry dynamical core model. Saving this example for a later paper would also permit you to reduce the appendix, cutting equations A12-16, which include description of the temporal parameters that are not used in this manuscript.

Very minor comments and typographical suggestions by page:line number

2:7 Maher et al. 2019 is now published.

2:13 ...for testing the dynamical cores of atmospheric models, the...

2:16 consider "all thermodynamical processes, e.g., radiation and convection, by a relaxation toward a prescribed equilibrium temperature profile, and the surface boundary layer by Rayleigh friction ...

3:18 changes on the transport of

3:20 this question has received a lot of

6:2 I believe i.e. should generally be followed by a comma, here and other places in the manscript

6:5 consider change the last sentences to "We describe the submodel in the next subsection, and provide technical details of the model setup (namelist choices, etc.) and implementation in the supplement.

6:14 consider removing the parentheses here.

6:16 you could cut this last sentence

6:25 Consider moving the sentence starting with "In section 4.2," up one sentence, to follow "winter hemisphere."

Figure 2 -- as I noted in the major comments, figure 6b might fit here, as a second panel. I appreciate that you would want to use a linear pressure, but otherwise it would put all the information on T_eq in one place.

7:4 There are a lot of parentheses in this sentence, which with the numbers 1) 2) and 3) were hard to parse.

7:11 The function names imply the intent of these diabatic heating/cooling options (i.e., to simulate climate change, stationary waves, and a monsoonal circulation), but these goals aren't fully explained in the paragraph. It might be good to explain both what each option allows and how it has been used in the past. For example:

The tteh_cc_tropics option allows the user to apply a zonal mean heating tendency with a Gaussian shape in latitude and pressure, as detailed in equation A10. The default values are taken from Butler et al. (2010) to approximate the impact of global warming on the atmospheric circulation.

Note that I would not call this exponential decay, which I would associated with exp(-x), not exp(-x^2).

7:12 (diabatic heating and cooling)

8:13 ... specified for each simulation in Table B1 and figure captions.

8:13 Consider rephrasing the sentence about it being favorable to convergence of the climatology is reached. I appreciate that the authors had to optimize their use of computational resources to obtain reasonable confidence about their results. This is particularly challenging for integrations with regime behavior, and is reflected in large uncertainties in some of the integrations.

8:20-25 You could also compare your results to the plots in the original HS94 paper, or at least refer the reader to the paper, which show some of these statistics as well.

8:21 It might be good to define the T63L19 notation here; I think it's the first time we've seen it. [You do define in the next paragraph.]

8:26 with a higher top

8:29 equatorward with T42L90MA resolution, and the eddy variance is generally reduced.

Figure 3: Consider using red-blue color scale for panels b,c. If panels e and f had the same color scale, it would give a better impression of the relative error.

9:1 Could you briefly explain what you mean by "resolution of convergence"? In Gerber et al. 2008 (which does not need to be cited) we found that the aspect ratio of resolution appears to matter, so that some of these effects (e.g., the shift in the jet stream) still appear at high horizontal resolution if the vertical resolution is sufficiently high.

10:13 It might be good to direct the reader to the relevant details on the mountain, e.g., equation (1) of Gerber and Polvani 2009.

8:30 What do you mean by "in a agreement with Wan et al. (2008)" Did they also show these effects of horizontal and/or vertical resolution?

Figure 5 caption and legend. I was initially confused about the meaning of \mu here, thinking that it was some parameter. Consider defining it here and the first time it is used in the text. For the caption, you could say "the average windspeed at this location, denoted \mu, is given in the legend".

12:12 Consider a paragraph break after (Butler et al. 2010).

13:3 free tropospheric jet (see Fig. C1).

13:9 (2009), however, the jet was shifted to higher latitudes in the simulation with a weak response. Thus, ...

13:21 What do you mean by non-physical? Gravity waves due end up providing a sink of momentum in the stratosphere, so there is some physical basis for this. But they do not behave like a Rayleigh friction, particularly when you start modifying the climate with parameter gamma, etc.. Shepherd and Shaw (2005) show why it's important that gravity wave parameterizations conserve momentum, but I think this is a finer point than you intended here.

13:27 but generally increases exponentially with decreasing presssure, not quadratically.

Could Figure 6b be incorporated into Figure 2?

14:1 set-ups naturally differ within

14:2 temperature also extend

14:3 There are also a lot of differences in the tropics, where you get jets. Are these well converged? Consider adding ... in particular the high latitudes and the tropics, where alternating jets form, reminiscent of Quasi-Biennial Oscillation winds albeit fixed in time.

14:11 since for both a flat surface and

Figure 8. The stippling dots were rather large here! (I don't mean too be picky, but you could describe these as polka dots!)

15:5 This is the first time you introduce the SPARC climatology, but it was used in Figure 8. You could just move this up to the preceding paragraph.

5:14 I'm not sure what is meant by unrealistic "step". I think the main problem is that the PK02 damped the winds to the climatology, but as the authors describe in other places in the manuscript, the climatology is warmer than radiative equilibrium in the high latitudes due to the overturning circulation (in the TEM framework, or equivalently due to meridional heat fluxes in the traditional, Eulerian framework).

17:17 I had trouble parsing this paragraph. I've made some suggestions below, but consider reworking it, as it is highlighting an interesting result.

17:19 polar vortex increases nonlinearly with increasing \gamma. In line ... at 10 hPa a the polar vortex accelerates more strongly with when \gamma exceeds a critical value (...

17:22 the polar stratosphere, i.e., change in parameter \gamma, the polar vortex...

17:28 This is an interesting paragraph, but I think you might be able to explain the regime transition more clearly in the context of the literature. There are two regimes here: for a moderate vortex, the winds are westerly, allowing Rossby wave propagation (Charney and Drazin 1961). This is allows waves to propagate up, breaking up the vortex and raising the temperature considerably above radiative equilibrium. When the vortex becomes two strong, however, stationary waves (or quasi-stationary waves) can no longer propagate fast enough to keep up with the flow, preventing waves from enter the vortex. Hence there's no dynamic tendency to offset the radiative forcing (at least until you get to the gravity wave drag layer much higher).

In my major comment above, I thought there might also be a third regime, when there is no vortex and easterlies prevent any wave propagation; this would a summer vortex situation, but again, you end up pretty close to equilibrium for the lack of wave driving circulation.

Figs 10, 11, 12. It would aid the reader to adopt a natural progression in the color scale, say a rainbow from red to purple as you transition along a parameter. That is, ptw=100 could be red, ptw=250, ptw=450 would be purple. When you have multiple configurations (ln vs. ln10) would be to use dashes for one and solid lines for the other.

21:3 In general I find that footnotes overemphasize the point, as the reader stops and moves down to figure it out. I have actually worried about this issue when varying the tropopause height in other simulations (a slightly different strategy, but with similar impact). It is good to warn the reader that if you lower the vortex too much, the distortion in T_eq could be a real issue. [My worry here is that you have a nice parameter knob in the code, but people could get into trouble if they use it as a black box.]

22:8-9 The transition is a bit awkward here. You first emphasize that other aspects of the integrations are different. But then you appear to talk about the simulations "also" exhibiting the same behavior.

22:9 Might be good to refer the reader back to Figure 10c here.

22:17 in the case of the

23:2 hPa, respectively

23: first paragraph. I had trouble parsing the argument here, but I think the authors primarily mean to say that for these integraions, the surface jet latitude is very sensitive to the vortex strength (Fig. 12b) but the 500 hPa jet is not (Fig. 12a)

Fig 14 Caption

First line: \overline{u} at (missing space in caption, and perhaps you could put the bar to emphasize zonal mean)

Later on, you refer to "sudden polar vortex decelerations" and SSWs, presumably referring to the same events. Please consider a consistent nomenclature.

Figure 14 right panel and surrounding analysis. I think these results are very interesting, but the time period choices seem somewhat arbitrary, and I worry about the statistical significance of the result. As this is a very interesting result, it would be nice if you could take a number of SSW events and consistently average before, during, and after the event to obtain the profiles with more statistical confidence.

[I appreciate that the authors mean for this to be a case study, rather than a proper research study, but this is one of the most novel results of the paper, and would benefit from more rigor.]

24 The footnote isn't necessary here. You could just state that these were \delta_z =4.3 integrations.

25:3 Where are the mixing ratios anomalously high? Do you mean in the high latitudes, or in the tropics? (Or everywhere?)

25:23-24 I was a bit confused here. How can the integration have weaker upwelling in the lower stratosphere as well as stronger downwelling in the midlatitudes? I think the authors just need to be a bit more descriptive here. There is weaker upwelling in the tropics, and all the downwelling in the extratropics is pushed to the vortex edge.

Section 5.2. Please do consider saving these results for future publications, as I felt that they were underdeveloped. Here are a couple small comments.

28:2 investigated in several publications

Figure 16 and surrounding discussion. Are splitting / westward eddy shedding events associated with eastward eddy shedding events, as implied by this single case study, or was this just a fortitous coincidence.

33:23 and equation A9. Would this drag work if the user specified a different set of vertical levels, other than L90MA. It might be good to include a reference to L90MA here as well.

34:15 If the model is starting from a cold start, I'm curious if you really need to spin up the monsoon slowly? [This is a very minor point!]

Table B1 The parenthesis after * and # confused me at first. Consider omitting them.

Figures C1-3 Consider also a natural progression with the color scale, though with just 4 curves, it's not so hard for the reader to parse.

Fig C2 and C3, panels d. I'm curious what clips the vortex at 150 m/s? Was this just how you plotted the curves (in which case you could just prevent the curve from falling to zero), or does the model prevent it from going faster? I do appreciate that this is 540 kph, pretty far beyond what you'd ever expect to see in the real world! I wonder what would happen when the vortex closes in on the speed of sound? (Clearly not a discussion for this paper!)