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Interactive comment on "Tropospheric eddy feedback to different stratospheric conditions in idealised baroclinic life cycles" *by* Philip Rupp and Thomas Birner

Anonymous Referee #1

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Tropospheric eddy feedback to different stratospheric conditions in idealised baroclinic life cycles

Philip Rupp and Thomas Birner

The authors report on a set of baroclinic lifecycle experiments aimed at better understanding the influence of the stratospheric vortex on tropospheric jets. Consistent with previous works, they find that life cycles are substantially modified by presence or lack of a stratospheric vortex. They point out the importance of the winds in the lower stratosphere and identify an amplifying effect of surface friction on the near surface response.

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Discussion paper

The experiments are interesting and potentially quite informative - more idealized experiments such as these are, in my opinion, under-utilized for studying this difficult problem. In particular I found them to be a very clear illustration of the importance of non-linear phase of baroclinic lifecycles in understanding the nature of the tropospheric eddy feedbacks. However, I found some of the discussion to be a bit unsatisfying. I think the most useful suggestion I can give is to strengthen and clarify the comparison of the present results with previous studies on baroclinic lifecycles. Section 1.2 does set up some past results, but it's never made that clear how the present results fit in. For instance: the Wittman papers emphasize changes in linear growth rates, while the present paper emphasizes the non-linear decay phase as the key period of difference, finding (from what I can tell) relatively weak changes in the growth rates. Is this discrepancy consistent with the Smy and Scott emphasis on PV in the sub-vortex region? How much does this have to do with the different basic states?

To be clear - overall this is a solid peice of science and should ultimately be worthy of publication. I have some further specific comments and suggestions below; some of which might involve significant further efforts. I do not require all of these suggestions to be pursued, but I do think the paper would be substantially improved if this main point can be substantially addressed.

Major comments/suggestions

PV structure

The PV structure of the initial conditions is emphasized in Fig. 1, but it is not really made reference to later in the interpretation. For instance, how do the perturbations considered in Section 4 modify the PV gradients? One clear difference between T and TS is the presence of narrow region of positive pv gradient near 20 km at 60 degrees in the presence of a stratospheric jet; is it really the winds in the lower stratosphere that are key, or is it the PV gradient structure? Moreover, how is the zonal PV structure changed over the evolution of the lifecycles?

Energetics

It seems that one way of interpreting these results, consistent with the discussion of Barnes and Young (1992) in Section 1.2, is that the presence of the stratospheric jet allows for a greater conversion of APE to EKE and ultimately MKE; and that this is further enhanced by including surface friction. If this is a fair characterization of the present results, how does the available potential energy differ in the various cases? Could this result be simply explained by the presence of more APE in the cases with a stratospheric vortex?

A related comment: in the presence of surface friction there isn't really final steady state as MKE will continue to dissipate. This is very briefly discussed right at the end of the discussion (l430), but until the reader gets there it's not really clear how the authors defined the final state. Would it be possible to put the figures in the supplementary information on more equal footing by computing the energy lost to surface frection in the runs in Fig. S2, showing instead the net EKE and MKE generated by conservative processes?

Mass fluxes

There is some discussion of the 1000 hPa 'Geopotential height' anomalies as a measure of the surface NAM or AO signature of the baroclinic adjustment. One substantial point is that equation (2) on line 270 assumes gradient wind balance holds near surface. This is ok for case with no friction, but not for the case with surface friction, which will modify dominant the force balance. This could be corrected by adding in surface friction on right hand side.

But I wonder if it might be more directly useful to consider the evolution of the surface pressure, and the nature of the mass fluxes in these experiments; in particular I wonder if these are substantially modified by the presence of an Ekman layer in the cases with surface friction.

Specific/Minor Comments:

l76: perturbation to what?

l188: could this alternatively be explained by greater meridional shear in TS simulation?

l213: Are EKE and MKE integrated over depth of troposphere or the depth of atmosphere? I would think that the MKE of polar jet is small but not negligible.

l310-317: I found the grouping of these experiments to be initially quite confusing, thinking that the natural ordering would be to group TS>10 with TS<10, and TS>25 with TS<25. I did ultimately figure out that the logic is intended to emphasize the wind anomalies in the lower stratosphere (to be fair, this is stated in the text, but preconceived notions can be hard to shake sometimes).

This is ultimately just an issue of presentation. But I did wonder if it would be clearer to readers to start with (what seems to me) the more natural a priori sorting instead of anticipating the results? If not, perhaps naming the sets something more descriptive, such as 'Weak winds/Strong winds' instead of 'Set 1/Set 2'?

l325 discussed

Fig. 7: How does u at 15 km vary over this set of integrations? This might be more illuminating than the mid-stratospheric winds.

l413-414: what is this statement based on? explicit calculations or assumption of linearity?

l423: The additivity of surface winds also is not perfect.

l450: This final paragraph also contributed to my feeling unsatisfied by the discussion. What is the explanation for the downward influence suggested by the present results? How can we use these experiments to quantify the eddy feedback? These aren't unreasonable claims, but the authors should explain them more explicitly.

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