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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

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We thank the referee for carefully reading our manuscript, and for their constructive comments. In the following we will respond to the various comments and point out any changes we intend to make to the paper based on them. Note that we have not provided exact manuscript corrections at this point, but we have provided the outline of planned changes. Line numbers and figure references in the reviewer's comments refer to the original manuscript. The reviewer's comments are in black italics; our responses are in blue.

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.

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.

We agree that a clear distinction from previous work (especially previous life cycle work) is very important. We think that the changes in the linear growth phase observed in the experiments of Wittman et al. and Smy and Scott can be explained by differences in tropopause-level PV gradients and tropospheric winds between their experiments with and without polar vortex, as was noted in Section 3.2. A direct change in tropopause-level PV gradients (as imposed by the initial conditions) leads to modifications of growth phase of the life cycles (e.g., a change in conversion from APE to EKE via the eddy heat flux). In our experiments we tried to specifically minimize this direct stratospheric impact, which can be difficult to distinguish from sensitivities in tropospheric setup. To emphasise this and other major differences in results we will extend the note about the different basic states used in the respective studies and will emphasise at various places in the manuscript that our setup has almost no change in tropopause-level PV gradient in contrast to the setups used in other studies.

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?

The main reason for displaying the PV gradients in Fig. 1 was to draw attention the regions of negative gradient that commonly appear in the initial conditions of typical life cycle experiments, but in our experience they are often not reported (although we concluded that those regions have no major effect on the evolution of the life cycle).

The PV distribution (and in particular the PV gradient) are certainly very important in determining the dynamics of the system. However, especially the zonal structure of the response is strongly dictated by the (non-linear) evolution during the decay phase of the life cycle (since the initial state has no zonal structure) and it is not trivial to make

statements about the precise mechanisms or dynamical features of the initial condition setup and processes that lead to the observed NAM signal.

Throughout the manuscript we describe most changes in initial state or life cycle evolution via the wind field (since the system is defined via the wind field), but tried to make it clear that the (structure of the) wind of course couples to various other characteristics of the system, like the temperature and PV fields. Part of the evolution (in particular the zonal structure) is described in Fig. 2 in terms of PV. During our analysis of the system we did not find that using the PV field to describe the final state of the life cycle and the corresponding dependency on stratospheric dynamics to be more useful than the wind field and thus we do not think that adding PV diagnostics to the analysis will improve the manuscript. Further finding crucial differences in PV between the experiments can be difficult since the PV field tends to become rather noisy during the non-linear phase (compared to the wind field), which is even more the case for the PV gradient (as it essentially scales with the curvature of the wind). However, to emphasise the coupling between the various fields and due to general importance of PV (gradient) for the system we will add corresponding notes throughout the manuscript.

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?

We find that initial state APE is indeed increased in our experiments with stratospheric jet and ultimately the APE to MKE conversion is enhanced in life cycles with stratospheric jet (consistent with the observed jet shift). However, since this increase in APE corresponds to a change of the temperature structure in the stratosphere it is not obvious that and how the additional stratospheric APE will be converted into tropospheric MKE during the life cycle. Further, we do not find a change in life cycle during the linear growth phase, suggesting the life cycle to be unaffected by the direct increase of initial APE. To address these issues we will extend our discussion of the global energetics and include a note regarding changes in initial state APE and corresponding APE to MKE conversion.

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?

We will add a note just after the introduction of the first experiments including surface friction (Section 3.3) explaining that we define a 'final state' analogous to the case without friction for easy comparison of the two cases. We will further (as also mentioned above) extend our discussion of the global energetics in the discussion section and the supplementary material in order to address the issue of comparability of experiments with and without surface friction.

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.

As the model equations are formulated in absolute pressure surface pressure is not a model variable and corresponding diagnostics are not available. The lack of surface pressure data further does not allow for a direct computation of the geopotential via vertical integration of temperature, which is why we decided to use a definition of geopotential based on the wind field. The geopotential field (on 1000hPa) seems to be a suitable diagnostic to analyse in regards of AO or NAM like phenomena, as is it typically used to define the corresponding indices. We agree that Equation 2 is missing a friction term. However, for friction a time scale of 1 day the missing term ($-a\bar{v}/\tau$) is typically small compared to the Coriolis term ($-af\bar{u}$). We found a difference in magnitude of the terms of about a factor 100 for the final state of our experiments. We will add a note next to Equation 2 to clarify that we neglect the friction term.

Specific/Minor Comments:

l76: perturbation to what?

We will add a 'stratospheric PV' before perturbation.

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

The shear would probably affect wave behaviour via changes in PV gradient, as mentioned. We will add a note to link the PV gradient to the wind structure again as it is probably important to emphasise this connection throughout the paper.

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.

Energies are integrated over the entire atmosphere (NH only). The difference in MKE between initial states T and TS is only a small fraction of the MKE difference in their final states. Further, the difference in final state MKE mostly persists when integrating only up to 100hPa, i.e., when we exclude the stratospheric jet from the energy computation. In order to subtract the minor contribution of the stratospheric jet to the MKE we show the difference to the initial state rather than full MKE. We will modify the corresponding parts of the manuscript to clarify the these points.

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'?

We will change the names of the groups accordingly for more intuitive understanding.

l325 discussed

Will be corrected.

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

Fig 7 shows final state zonal wind at 10km, which is where we find the strongest response. Winds at 15km change qualitatively the same throughout the simulation as also suggested by the rather barotropic final state winds shown in Fig. 5. Initial state winds at any height change linearly with stratospheric jet magnitude uSmax due to the stratospheric jet being superimposed onto the tropospheric jet. No corresponding changes to the manuscript will be made.

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

As mentioned, we performed a set of sensitivity experiments with increased tropospheric jet magnitude (and no stratospheric jet) what showed a similar NAM response as the experiments with stratospheric jet, but the required difference in tropospheric jet magnitude was much larger than the increase induced by inclusion of a stratospheric jet in order to obtain a NAM response of similar magnitude. This suggests that the increase in wind speed at tropopause level alone is not sufficient to explain the observed NAM response in our experiments. We will change the corresponding part to '. . .it was necessary to increase the jet magnitude by order of 10 m/s in these sensitivity experiments. . .' in order to clarify that the statement is based on explicit simulations.

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

We will add a corresponding note to the paragraph in order to slightly weaken the statement made.

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.

We agree that a concise, yet complete and clear, description of our main findings and arguments in Section 6 is important and therefore extended the respective last paragraph, as suggested by the referee. .

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