Review of WCD-2021-24: Stratospheric intrusion depth and its effect of surface cyclogenesis: An idealize PV inversion experiment

Overview: This manuscript sought to explore the role of Southern Hemisphere stratospheric intrusions (lowering of the stratosphere into the troposphere, realized as a lowering of the dynamic tropopause) on tropospheric circulations. The authors used a simplified atmospheric state to compute a variety of PV inversion experiments examining the relative impacts of a deeper intrusion, an intrusion from different tropopause heights, a combination of the two, a stronger intrusion, and a wider intrusion. The results are generally as expected from existing PV thinking – the closer the cyclonic PV anomaly to the surface, the stronger the influence at the surface. Some interesting results were either affirmed or introduced (in particular regarding the intrusions from a higher dynamic tropopause, and the role of wider/narrower PV anomalies), but in general the results re-affirmed known relationships that are applicable to either hemisphere. I hesitate to say that the article isn't introducing anything new – often these relationships are taken for granted through theoretical arguments but aren't shown through simple modelling exercises – but little about the results seemed new. Perhaps more importantly, the connections drawn by the authors to both cyclogenesis (rather than just a cyclonic response to a PV anomaly) as well as to real-world scenarios were not robust enough to make it clear that this presented a substantial contribution to the science. There were also concerns about the model set-up in the first place, which appeared simplified to the point of violating key laws of the mid-latitude dynamics that appeared to be the focus of the manuscript. Lastly, the manuscript was written too colloquially and lacked focus in many places. I do believe that with a careful and attentive revision the paper will represent a good contribution to the field, and as such I recommend a major revision and re-review.

Major Comments:

1. *Model and Experiment Set-up:* The study seeks to study, and compare to, mid-latitude dynamic processes (a prescribed latitude of 43°S) that occur in a baroclinic environment, but the model itself is run as a barotropic model. The authors need to clearly justify their model set-up and how to interpret their results appropriately. In the Barnes et al. 2021a paper that they base the climatology off of, the authors made clear that the COLs of interest occur in a baroclinic environment (which is required for the jet to occur under thermal wind balance). The reviewer recognizes that idealized barotropic models are an important tool in trying to diagnose these questions, but the authors need to make a much more clear and stronger justification for doing so.

We thank he reviewer for this suggestion of adding in more justification for our use of experimental design. The idea behind this choice of barotropic model was made in order to strip the environment of any other forcings and isolate the PV intrusion itself. We would like to test PV intrusions in a baroclinic environment as well but it is felt that this could be for further study which include temporal aspects and additional advection aspects as well. We have added this aspect into the manuscript

Regarding the experiment set-up, the authors also to more clearly justify their decisions for the spatial extent and intensity of the PV anomalies. There are many studies out there that have studied PV anomalies, so a more clear justification for why they determined a PV anomaly to 'look' the way it did is necessary.
Of particular focus needs to be a clear justification of the horizontal and vertical extent, as well as a justification for a sudden and total relaxation of the PV gradient in the anomaly itself once the -1.5 PVU threshold is met (eg. Fig. 4).

The relaxation of the PV gradient within the anomaly itself was chosen for ease of controlling the magnitude of the PV as shown in the resulting PV intrusion. In these experiments we use the

PV anomaly to attempt to recreate PV intrusions as closely as possible to as would be seen in reality ie. A tongue of high magnitude PV extending from the stratospheric pool of high PV. Within the numerical framework used. Restricting the gradient within the anomaly was the most effective way at ensuring a tongue of -1.5 PVU resulted. We do agree that there would be a PV gradient in a real-world PV intrusion. In Experiment 4, the magnitude test looks to test this out by adding additional PV values within the 1.5 PVU and shows that it results in little effect on the cyclogenetic forcing induced. We have added this argument into the manuscript during our discussion of the anomaly in the experimental setup.

"It is acknowledged that within real-world PV intrusions, there would be a PV gradient within the PV intrusion lower than PV intrusion boundary is defined. However, within the experimental framework the interior of the PV intrusion is kept constant to more easily control the magnitude of the PV intrusion that results from the PV anomaly."

2. *Conflation of the idea of cyclogenesis and a cyclonic response:* The authors routinely refer to a cyclogenesis term (based on thresholds of cyclonic vorticity – lines 213-215) but are looking at the response at the surface given the existence of a PV anomaly at the tropopause. In other words, by using a PV inversion (rather than integrating a model forward where a PV anomaly is introduced, and the surface is allowed to evolve in response), you are not looking at cyclogenesis, but instead the existence of a cyclonic circulation due to the existence of a PV anomaly. This undermines several components of the results, both when the authors discuss the surface circulation due to the PV anomaly as well as when they make points on the potential evolution of the anomaly were it allowed to evolve in time. Further, their discussion of these points leave concern about a lack an understanding of PV dynamics. If the PV anomaly and surface cyclone are vertically stacked (as they are in the results), and this is a dry barotropic environment, the vorticity anomalies are the only factor at hand that can influence the system, meaning only movement is allowed rather than intensification. Thus, the surface cyclone cannot undergo further development from an intensification standpoint, and the same holds for the upper level cyclone – thus leaving the question of how the system can ever 'develop its own closed, cyclonic circulation (or COL)' (line 303). A very careful examination and reworking of this discussion is critical for the interpretation of the results.

The authors understand the confusion of our arguments with respect to cyclogenesis and cyclogenetic forcing. We have removed our interpretation of the cyclogenetic forcing resulting at the surface with respect to cyclogenesis to remove the temporal implications of our arguments. Instead with only compare the cyclogenetic forcing in terms of pressure reduction and cyclonic vorticity that results on the surface due to the existence of the PV anomaly comparatively between the experiments. We do acknowledge that future work should look at the temporal aspect of these PV intrusions utilizing a dynamical core or similar.

3. *Dynamic interpretation/explanation of experimental results:* The results here are interesting – but lack a fair bit of interpretation from a PV framework regarding why the responses are occurring. The authors make some efforts on this front, but more needs to be done beyond just reporting the results to really enhance the impact of this study. For example, experiment 4 (changing the intensity of the PV anomaly) shows almost no change in response despite a presumably stronger PV gradient (though it might not be that much stronger given the experiment set-up). The results are interesting – but there's little to no interpretation for why we see the response we do. The same goes for experiment 5 – the authors report the change in tropospheric circulation but provide little interpretation for why. For example – why do we see a decrease in cyclonic relative vorticity with a widening PV anomaly? How can this be interpreted in a PV framework? Why is the surface circulation so much stronger?

We have tried to strengthen the dynamic interpretation of this work as the reviewer suggests. In experiment 4, we agree that is the lack of the change of gradient within the PV intrusion that is the likely cause of the lack of change in the circulation around the anomaly. However, we make the argument that PV values within these intrusions are unlikely massive values far greater than that of the dynamical tropopause and therefore it reflects the real work atmosphere relatively well, despite this limit within the experimental design. In experiment 5, it is explained why we may see an increased relative vorticity value with broadened PV anomaly in the mid-tropopshere.

Minor comments:

General comments:

- The manuscript reads *very* colloquially which is problematic. Please carefully check through the manuscript to identify instances where this occurs – I've identified some examples here, but there are many others throughout:
    - L40: Air can be advected or diabatically altered, but it cannot be 'introduced' to another region of the atmosphere.
      "introduced" changed to "advected"
    - L42-43: The term 'basic' here isn't necessary, and acts to undermine your study (there's little basic about PV theory – it's an advanced synoptic-dynamic topic that readers may not be familiar with).
      "Basic" has been removed as per suggestion
    - L67-80: You use the term 'This study examines/looks at/aims' too much here – aim to rework a bit.
      We have tried to lessen the use of these words as suggested
    - L92: 'diagnostic for reanalysis sets to diagnose' – aim to avoid repetitive words in a single sentence (there were several of these in the manuscript)
      Remove repeated words
    - L237-238 and elsewhere: Unless quantifying, avoid using the term 'stronger' and 'weaker' or similar qualitive statements (other examples include 'meagre' in L370 or 'massive' in L378)
      We have removed the use of qualitative words throughout the text as suggested.
    - L291 and elsewhere: The term 'exponentially' refers to a mathematically derived curved for a set of data points – if it is exponential, prove it; otherwise, please avoid statements that imply something different from what the data shows.

      References to exponential growth have been removed unless mathematically proved as suggested
    - L362 and elsewhere: Aim to avoid injecting opinion or emotion – lines such as 'Since we are dealing with …' should be avoided.
- Definition of terms: In several instances, terms/acronyms were introduced but not defined. As a reader familiar with the topic, I could ascertain nearly everything, but less familiar readers may struggle. Examples include (but aren't limited to):
    - PVU (L37) Updated: PVU ($1\ PVU = 10^{6} K\ m^{2}\ s^{-1}\ kg^{-1}$)
    - COL (L64) COL = cut-off low. First instance has been updated.
    - Reference state (L86) – be sure to define what this is and how you establish it
    - "halo" (L150) A halo is a ring of light that encircles something bright. It is our best word to describe our anomaly setup
    - MSLP (L201) – MSLP = mean sea level pressure. First instance has been updated
    - Sphere of influence (L271) - removed
    - Mid-tropospheric (L347)
    - Total atmospheric system (L478) - removed
- Figures/equations: There were several inconsistencies in the figures that could be tightened up, and equation 6 does not need to be there (it's just a re-arrangement of equation 1 and can be stated as such):
    - Figure 3 and elsewhere: Please always use panel labels (eg. A and B)

consistently. Please also include reference vectors whenever showing vectors that represent direction and magnitude.

We are not sure of the consistency the reviewer is looking for in the figure labels. We have used A and B consistently (A, B, A1, B1, etc) throughout the text

- Figure 5: This would be more clearly communicated as a table rather than a flow chart

  We have converted this figure to a Table (1) as per the reviewers suggestion.

- Figures 9, 11, 14, and 16: Please use the same axis labels amongst these four figures. This is particularly important for Figure 14, which appears to have a large MSLP response based on figures 9, 11, and 16, but in reality is only ~0.3 hPa.

  These have been updated as per the reviewers suggestion

- All captions: Be sure to include all relevant information, such as MSLP contour intervals or wind speed contour intervals.

  Additional information such as MSLP and wind velocity contours are provided as per the suggestion