"Stratospheric intrusion depth and its effect on surface cyclogenesis: An idealized PV inversion experiment"

Authors: Barnes, Ndarana, Sprenger, and Landman

Recommendation: Major Revision

Overview:

In this study, the authors perform a series of idealized experiments in which they invert QGPV anomalies of various sizes, shapes, and vertical depths for their associated horizontal circulations. These circulations are then used to identify QGPV configurations that are likely to be more influential on surface cyclogenesis. In my opinion, while the analysis does not necessarily offer any new qualitative dynamical insight beyond what has already been garnered from the application of a PV framework in prior work, the experiments performed herein do provide a nice systematic quantitative treatment of how nuances in the structure of QGPV anomalies contribute to surface cyclogenesis. This quantitative assessment is novel from my perspective, and justifies the value of this study.

That being said, there several instances within the text in which I felt the present work could be better motivated and described with improved precision. Additionally, the intensification rate of surface cyclones is an important component of the interaction between upper-level PV anomalies and the surface. The temporal evolution of surface cyclones is not considered as part of the analysis but is quantifiable using diagnostic PV tendency inversions. Last, the analysis does not necessarily consider the role that static stability plays in modulating the character of these interactions and may represent an additional experiment that the authors can consider integrating into their analyses. Given the extent of my comments below, I have recommended the manuscript be returned for Major Revisions.

Major Comments:

1. There were several instances within the text in which I found myself a bit confused regarding the interpretation of figures (see minor comments below). I believe that this confusion could be remedied with a thorough review of the text to improve the precision of the discussion and through better definition of various terms. For example, it was difficult for me to differentiate between the physical interpretation of the minimum relative vorticity and the cross-sectional minimum in relative vorticity.

2. The introduction and motivation for the present work could be made clearer. Namely, it might be effective to construct a figure that highlights the diversity of PV intrusions and how these structures are associated with different surface cyclone intensities in real data. This figure could more effectively frame the idealized experiments performed in this study. There are also several instances in the introduction where the authors emphasize that such a study has not been performed in the Southern Hemisphere. But, to my knowledge, there is no reason to expect that PV anomalies will behave in a dynamically different way compared to the

Northern Hemisphere. Therefore, I recommend this discussion should be either more strongly motivated or eliminated from the text.

3. The authors do not perform a temporal diagnosis of the evolution/development of cyclonic circulations at the surface in their idealized experiments, but such an analysis can be performed in a diagnostic sense using either QGPV (e.g., Breeden and Martin 2018) or Ertel PV (e.g., Davis and Emanuel 1991). I wonder whether the application of this diagnostic framework for examining the instantaneous intensification rate of surface cyclones would bolster the analysis. It is also not clear in the text why QGPV is adopted over Ertel PV. At the very least, this choice should be justified in the context of the proposed applications.

4. One element that is not explicitly considered in the idealized experiments is the role of static stability. Namely, those PV anomalies that are situated in a less stable environment are able to more effectively induce cyclogenesis. Could an experiment be run that considers varying the static stability of the environment? Additionally, the inversion of QGPV requires the specification of a reference atmosphere. It is not clear from the text what the authors have selected as their reference atmosphere, unless I may have missed it.

5. It is somewhat difficult to compare the various experiments because the summary figures (e.g., Fig. 9) feature different values along their y-axes. To better enable a comparison between experiments, I'd recommend standardizing these y-axes across all similar plots.

Minor, Specific, and Typographical Comments:

Abstract
L15–16: I am a bit confused by the discussion in these lines. Namely, L15 states that horizontal extent is more important, whereas the next line states that vertical depth is important in dictating the strength of the circulation – both of which can be used to characterize cyclone intensity. Could these lines be clarified to better describe the respective influences of the vertical and horizontal extent of the stratospheric PV anomalies?

L17–18: This relationship in this sentence has to be true by definition, and I wonder whether it could be deleted to make more room to better clarify the nature of the relationships described in L15–16.

1. Introduction
L41: The specification that "high PV" corresponds to negative values should occur earlier in the manuscript when this terminology is first used. I also believe it could be made clearer earlier in the introduction or abstract that the focus of the manuscript will be on Southern Hemispheric anomalies to avoid any potential confusion imparted on a reader.

L44–45: The vertical depth of the circulation induced by the PV anomaly in Fig. 1 is also a function of the static stability. Namely, the lower static stability in the troposphere compared to the stratosphere allows the circulation induced by the PV anomaly to penetrate deeper towards the surface. Some reference to the thermodynamic environment in which the PV anomaly is embedded could benefit the discussion at this juncture in the text.

L51: In the context of this study, cyclogenesis is described as a near-surface phenomenon. Consequently, I found the reference to cyclogenesis occurring throughout the stratosphere to be a bit confusing. Could this line be revised for improved clarity?

L61: Why is it expected that the influence of PV anomalies will be different in the Southern Hemisphere compared to the Northern Hemisphere? I believe this claim may require stronger motivation/explanation.

L62–63: The term "stratospheric tropopause" is not accurate, since the tropopause represents the interface between the troposphere and the stratosphere.

L64: The acronym, "COL", has not yet been defined in the manuscript.

L64–66: It is not clear to me how this conclusion follows from the previous sentences in the paragraph. Consider a revision to improve the clarity of the discussion.

L72: As in L61, it is not clear why the physical influence of cyclonic PV anomalies will differ between the Northern and Southern Hemisphere simply because the sign of PV is negative in the Southern Hemisphere.

L72–73: This sentence is somewhat redundant with the statement that ends in L70. Consider whether it could be deleted.

2. Methodology

L89: The Davis (1992) study focuses on the inversion of Ertel PV rather than QGPV, for which the system of equations for performing the inversion features nonlinear terms. For QGPV, the differential operator is linear, which does return a unique solution using successive over-relaxation.

L114–119: I found this discussion to be a bit confusing. Could you clarify more as to why the tropopause is defined differently within the inversion algorithm?

L121: Is the "specified DT" the height of the tropopause above ground level needed for the algorithm or the –1.5 PVU isosurface?

L127: Is the intent to refer to the left panel of Fig. 3 in conjunction with this discussion? I ask because the right panel of Fig. 3 does not show any pressure contours.

L205–210: I found this discussion concerning vorticity thresholds to be a bit confusing. Are you basically looking for areas near the surface that feature vorticity with the magnitude described in the text? Or are you looking for areas where the circulation induced by the upper-level PV anomalies features vorticity of a particular magnitude at the surface.

L213–215: To what extent are the results sensitive to the selection of these vorticity thresholds?

3. Results
L244: If referring to a line of constant pressure here and elsewhere in the manuscript, "isobar" is more descriptive than "isohypse".

L297–298: This particular sentence, as currently written, is a bit confusing. Would it be possible to rewrite it for further clarity? Similarly, I found L300–301 to also be confusing, which may require an edit for further clarity.

L359–365: Could a plot of static stability be produced to help illustrate this effect more clearly?

L392: Arguably, this claim may be best reserved until after the final few experiments have been introduced (i.e., anomaly intensity has not been considered yet). Unless the goal here is to refer to the vertical extent of the anomaly. If so, a revision may be necessary to make that point clearer.

L406: Would it be possible to expand further on how this result may be an artifact of the basic state?

L421–427: I'm having a bit of a difficult time verifying some of these values against those plotted in Fig. 14. Could the text be revised to more clearly reference where these results are drawn from.

L436–440: Could a figure be produced that shows the characteristic PV structure associated with these categories in real cases. This may help to visually motivate the forthcoming experiment.

L445–446: It is not clear what this particular statement is referring to.

L459–460: This statement is a bit difficult to verify. Namely, Fig. 16 suggests that the magnitude of the relative vorticity decreases with increasing width of the anomaly, which is counter to the discussion in this section. I think my confusion here may stem from difficulty understanding how the cross-sectional relative vorticity is calculated/defined.

L462: For greater specificity, it may be worth stating that this 1 hPa corresponds to a pressure *perturbation* – as a "lower central pressure" would typically correspond to a stronger storm.

4. Discussion and Conclusion
L520–525: These few lines may be a bit redundant with the discussion in the previous paragraph. Consider whether these lines could be deleted without any loss of content.

Figures and Tables:
Fig. 2: Would it be possible to specify the contour interval for PV in the caption?

Fig. 5: It is not clear to me from the flow chart why all the experiments connect with the basic state box. It may be clearer to put the basic state box at the top of the image and then have all experiments flow beneath it.

Fig. 6: Could the contour interval for the meridional wind be included as part of the caption?

Fig. 9: It is not clear to me how the cross-sectional minimum relative vorticity is different from the minimum relative vorticity value. Could this be explained a bit more clearly in the text?

Fig. 9/11/13: Would it be possible to make the limits along the y-axes the same in all of these plots so as to allow for better comparison between experiments?

References:

Breeden, M., and J. E. Martin, 2018: Analysis of the initiation of an extreme North Pacific jet retraction using piecewise tendency diagnosis. *Quart. J. Roy. Meteor. Soc.*, **144**, 1895–1913, https://doi.org/10.1002/qj.3388.

Davis, C. A., and K. A. Emanuel, 1991: Potential vorticity diagnostics of cyclogenesis. *Mon. Wea. Rev.*, **119**, 1929–1953, https://doi.org/10.1175/1520-0493(1991)119<1929:PVDOC>2.0.CO;2.