

“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.
Thank you for this suggestion. We have made additions to the figures, text and captions as specified in the relevant suggestion below.
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.

The reviewer makes a very valid suggestion. The work was inspired by two papers preceding this work – one of a case study of PV induced COLs and surface lows and another of a climatology of COL extensions associated with PV intrusion depths. Both are cited in this work. It is felt that this serves as justification for the diversity of PV intrusion scenarios tested in this study. Although real world cases could additionally be shown here, this may take away from the idealized feel of the work and make it rather lengthy. We appreciate this suggestion, but have opted to not include at this stage.

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.
The reviewer proposes a very interesting question regarding temporal diagnoses. The idealized setup reflects instantaneous changes of the parameters that are induced by the various PV anomalies. Although we did consider including temporal aspects *visa* case studies as was performed in Breeden and Martin (2018), it was decided that this would confuse the direction of the work. We therefore decided to the work limited to instantons changes and analysis. We hope that future work will entail the utilization of a dynamical core etc which we can then use to analyze temporal changes such as those proposed by the reviewer.
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.
The reference atmosphere is described in the experimental setup section. The analysis of varying static stability environments was not considered here. We would however like to do this in future more intensely by looking at various aspects of the stability of the environments including low-level inversions etc. It believed that this could be best achieved by use of a full dynamical core which would also include temporal aspects of the stability of the environment (for eg. How is a low-level inversion degraded by PV anomaly?)
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.
Thank you for this suggestion. We have standardized all of the y-axes in the plots specified.

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.

The abstract has been reconfigured to clarify the above as suggested. In essence, wider anomalies result in a greater decrease whilst relative vorticity on increases marginal whilst the reverse is true for surface relative vorticity changes.

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.

The abstract has been amended in order to introduce the idea that the present study takes place in the Southern Hemisphere and introduce the idea that high-PV are negative anomalies in this study.

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.

The static stability settings used within this study are added into the experimental setup section to reference the thermodynamic environment used within this study

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?

The use of cyclogenesis for the theoretical experimental framework and results have been removed which should clarify the lines referenced by the reviewer

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.

The study in reference here is in relation to a climatology of PV intrusions in relation to COLs. The hemisphere in which it is performed is therefore highly relevant to the distribution of these phenomena etc.

The mathematics and theories of the influence of PV anomalies is hemispheric independent and so for the purpose of this study may not be critical to the result obtained, as the reviewer suggests. We have therefore trimmed our references to it being a southern hemispheric study extensively. However, we have left some reference to this study being from a southern hemispheric perspective as we believe that this type of study in a SH context is rare. We hope

that the fact this study is performed from a SH point of view can help to stimulate further work in our region.

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

The typo has been removed and reference to “tropopause” only is made.

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

The acronym COL has now been defined

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.

The COL extension climatology by Barnes et al. (2021a) was based on real-case reanalysis data. As reanalysis data, climatological averages and composites were used, the isolated effect that the PV intrusions studied by Barnes et al. (2021a) had on surface cyclogenesis was not considered.

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.

The mathematics and theories of the influence of PV anomalies is hemispheric independent and so for the purpose of this study may not be critical to the result obtained, as the reviewer suggests. We have therefore trimmed our references to it being a southern hemispheric study extensively. However, we have left some reference to this study being from a southern hemispheric perspective as we believe that this type of study in a SH context is rare. We hope that the fact this study is performed from a SH point of view can help to stimulate further work in our region.

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

We agree! The sentence has been deleted and incorporated into L70.

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?

We have tried to reword this section in order to clarify this.

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

The specified DT is the height of the tropopause needed for the algorithm

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.

The reference to “(right)” has been removed. The intention was to discuss Fig. 3 holistically

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?

The discussion concerning these cyclogenesis thresholds has been removed. The aim was to try and quantify the strength of cyclogenetic forcing but reviewers and readers found this comparison to cyclogenesis (with a temporal component implied) confusing. We have therefore removed this section

3. Results

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

Description of the surface pressure in terms of isobars has been removed as a result of our effort to separate the induced response and cyclogenesis type arguments.

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.

This description has changed as a result of our effort to separate the induced response and cyclogenesis type arguments.

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.

The intention was, as you refer, to refer to the vertical extent of the anomaly. We have tried to clarify this point with some minor wording tweaks.

The results in Experiments 1 and 2 imply that the proximity of a stratospheric intrusion to the surface has a larger impact on inducing deeper and enhanced cyclonic circulation at the surface than the vertical extent or size of the intrusion itself.

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.

We have revised the text as suggested. In essence, there are minor differences in the meridional mid-tropospheric velocities but none really at the surface.

“Minimum cross-sectional relative vorticity decreases by $3 \times 10^{-5} \text{ s}^{-1}$ from the low to high anomaly amplitude scenarios, whilst the maximum meridional velocity decreases by 1 m.s^{-1} around the anomaly. Anomalies of all magnitudes tested induce similar cyclogenetic forcing upon the surface. Both the induced surface pressure and relative vorticity are comparable throughout the scenarios tested.”

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.

For some reason our reference was left out of this sentence. This should clear up this issue.

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.

We have tried to improve our arguments with respect to the lines indicated
“The intrusion width changes result in a change in the geometry of the resultant jet core which appears thinner and shorter the thinner anomaly, whilst the broader anomaly results in a visibly broader and longer jet core, affecting almost the entire cross-sectional domain. Although stronger velocities are observed in the troposphere as a result of broader intrusions, the mid-tropospheric relative vorticity increases sharply for broader intrusions (Figure 16). The larger magnitude relative vorticities induced by thinner intrusions are the result of the circulation, even though with lower velocity, being confined to a smaller horizontal region around the anomaly.”

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.
The line specified has been updated as per the suggestion in order to indicate that we are referring to a surface pressure anomaly

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.

In this paragraph we state that it is the height of the intrusion AGL that is more critical than the intrusion size itself. The previous paragraph talks to the depth experiments taking into account a similar tropopause height.

Figures and Tables:

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

Added as per reviewer suggestion

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.

As a result of this suggestion and that of the other reviewers we have converted this flow chart to a Table in order to improve how the variation of these experiments reads.

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](https://doi.org/10.1175/1520-0493(1991)119<1929:PVDOC>2.0.CO;2).