Review

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

by Michael A. Barnes, Thando Ndarana, Michael Sprenger and Willem A. Landman

Summary

The authors investigate the impact of stratospheric intrusions on the flow/vorticity and pressure field on the surface by quasi-geostrophic PV inversion. The authors perform several sensitivity experiments to look at the contribution of different parameters like intrusion depth and anomaly scale on the surface response.

The results are not surprising but might be nice to have in a kind of summary of different parameters. However, the manuscript is quite lengthy and written colloquially. I miss a thorough context of recent work in the introduction and the conclusions and a dynamical explanation of the experiments next to pure observational descriptions of the results. Additionally, I miss a thorough revision of the manuscript regarding some of the major concerns of both reviewers. Therefore, I still recommend major revisions before acceptance to meet the high standard of the journal.

Major comments

1. Response to first review
The authors should respond more carefully to both reviewers. Both reviewers made perfectly clear that there have been gaps in the interpretation and especially the presentation of the results, but only the few basic examples of the reviewers have been modified. The authors should check very carefully:

a) the colloquial writing (terms like „amount of surface cyclogenetic forcing“ - throughout the text, „development driver“ - L461, „drive stratospheric air into troposphere“ - L407) is problematic and often leads to a difficult or even wrong interpretation (e.g. „High-PV anomalies of stratospheric air are often advected into the troposphere by Rossby wave breaking„ - L38)
Amount of: Surface cyclogenetic forcing is measured by means of changes in the induced relative vorticity and surface pressure for each intrusion scenario.
L407: RWB events, which are associated with isentropic transport of stratospheric air
L461: This re-emphasises that upper-level processes induce both the surface cyclone and
L38: Rossby wave breaking (RWB) is often associated with the isentropic transport of high-PV (large negative values in the Southern Hemisphere) anomalies of stratospheric air into the troposphere

b) interpretation of results: the authors mention in their response to reviewer 1 that a dynamical explanation is given following Experiment 5. But I cannot find any. The same holds true for the other experiments.
This study links the vertical geometries of idealized PV intrusions to its affect on surface cyclogenetic forcing. We discuss these geometries in terms of different weather systems. For example, in Experiment 5 where we link our experimentation with PV intrusion width with different types of RWB events and for example in Experiment 2-3 with increased tropopause heights in a tropical/summer scenario. We feel that this adequately interprets the results in terms of observed weather systems.

c) title: following the major comment of reviewer 1, I suggest to replace cyclogenesis in the titel, since just the cyclonic response of the stratospheric intrusion is investigated and no cyclogenesis
The title has been changed as per the reviewer's suggestion.

2. Recent work
The authors start the introduction directly with the properties of potential vorticity and the

advantage of potential vorticity and how the flow around stratospheric intrusions looks like.. I would certainly argue that this paragraph could be shortened to a great deal. What I completely miss is a discussion about the importance of stratospheric intrusions in relation to cyclogenesis and cyclones to motivate the current study further. What about the difference between baroclinic and barotropic cyclogenesis? This discussion would also help to motivate the use of a barotropic model. This point also holds for the discussion. The only studies the author relate to are the first authors last studies. I do really miss a more detailed context within the manuscript.

The presence of high-PV intrusion of stratospheric air in terms of surface cyclogenesis are well established. The authors have tried to motivate this study as per the reviewers suggestion by pointing out a variety of studies that show this in fact to be the case:

"Several studies have shown cases of cyclogenesis and their development in the presence of a stratospheric intrusions of high-PV (eg. Davis and Emanuel 1991; Davis 1992a; Iwabe and Da Rocha 2009; Barnes et al. 2021c). Bierly (1997) confirm this link through composite analysis and show the importance of the upper-level intrusion during cyclones initial development. Many studies have focussed on rapid cyclogenesis. A landmark case study shows a tropopause fold that developed in relation to the President's Day cyclone over the east coast of the United States (Uccellini et al. 1985). Rapid cyclogenesis has since been linked to the presence of a PV tower – an alignment of three distinct PV anomalies, in the upper troposphere, lower troposphere and surface (eg. Čampa and Wernli 2012)."

Minor comments

General comment

Especially in the introduction the manuscript is quite confusing regarding the northern and southern hemisphere since both perspectives are used. It would certainly improve the understanding, if the authors make clear at the beginning (not only in the abstract) that their work focus on southern hemispheric PV and then stick to it. That is, a stratospheric streamer on the southern hemisphere is in my understanding a low-PV anomaly or negative anomaly, not a high-PV (negative) anomaly (e.g. L14).

In this work, we deal with large negative PV values. We have avoided using the term "low-PV" values as tends to imply smaller (less intense) values. In order to solve this issue we clarify in the beginning of the manuscript that the convention of this work is to use high-PV values within the context of this study to indicate large absolute PV values and since we are working in the SH, we deal with large negative values as being "high-PV" values.

"It should be noted that this takes place in the southern hemisphere atmosphere where large negative values of PV are associated with cyclonic motion, contrary to the northern hemisphere where cyclonic motion is associated with large positive values. For the purposes of this study, high-PV values are associated with large negative values of PV."

Detailed comments

L13: Do you refer to the strength of the cyclonic flow? What do you mean by amount?

"Amount" has been changed by "intensity" to clarify

L14: high-PV (negative) anomaly, s. above

The "negative anomalous" has been changed to "large negative PV" to avoid confusion.

L23: add e.g. before reference of (Røsting and Kristjánsson 2012)

Changed as per suggestion

L32: add e.g. before reference of (Davis and Emanuel 1991)

Changed as per suggestion

L34: Remove „the" before based on

Changed as per suggestion

L36: add e.g. before Lackmann 2011

Changed as per suggestion

L38: high PV anomalies -> low (SH)

Brackets "(large negative values in the Southern Hemisphere)" have been used to clarify our meaning

of “high-PV”.
L38: high PV anomalies are often advected into troposphere: pure advection would not lead to a mixture of stratospheric and tropospheric air without the influence of nonconservative processes as e.g. latent heat release, radiation or turbulence/mixing.
Changed the word “advected” to “transported” to avoid confusion.
L40: can? In my opinion a positive anomaly (NH) is always associated with cyclonic flow
The word “can” has been removed.
L41: has been -> have been
Changed as per suggestion
L42: has -> have
Changed as per suggestion

L57: few studies have used -> which?

L61: „The results show that stratospheric intrusions with a -1.5PVU tropopause associated with 250hPa COLs that extend to 300hPa or below, are more likely to result in surface cyclogenesis„. - What are 250hPa COLs?
In the context of the study mentioned, we refer to COLs at the 250hPa level. This has been clarified by changing the phrase to “COLs detected on the 250hPa pressure level”
L71: again, what do you mean by amount?
“Amount” has been changed by “intensity” to clarify
L90: not integrating.. inverting!
Changed as per suggestion
L91: Davis 1992 investigates PV inversions under non-linear balance.. due to the non-linearity of the equations it is not expected that the resulting variables after piecewise inversion add to the full fields. However, you are considering PV inversion under quasi-geostrophic balance, that means the resulting variables add up to the full field. Hence, no sensitivity is expected how the inversion is performed.

Eq(3), Eq(4): I think in both equations a minus sign is missing
Minus sign was missing in Equation 4. It has been rectified.

L407: reference to figures wrong.. -> Fig. 14 and 15 and not 15 and 16.
Unclear where the reviewer is referring. No reference to Fig 14 and 15 on L407. All references to Fig 14 and 15 look accurate.

L459: „show the development of an amplifying trough“, where do you show that? no development can be observed without time evolutions investigated
The temporal factor of this has been removed to only indicate that the PV anomaly simulates upper-level trough as is expected by theory. “… show that the high-PV anomaly results in a trough, as is expected by theory”

L532: Barnes et al. 2021 referenced twice Figures and tables
Fig1: * remove one represent in caption
Changed as per suggestion

* think -> thick
Changed as per suggestion

Can figures 2,3, and 4 combined to shorten manuscript? All figures really necessary?
We feel that all are necessary as they link to the experimental figures in the below sections directly.

Table 1: * Experiment 5 - typo 100km-100km?
We refer here to 100km-800km in steps of 100km. No changes made.

* Experiment 4 - either -1:-0.1:2 or -2:0.1:-1
Changed as per suggestion

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

Authors: Barnes, Ndarana, Sprenger, and Landman

Recommendation: Minor 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 tobe more influential on surface cyclogenesis. The authors have done well to address my prior comments on the manuscript. At the moment, the large share of my comments are textual in nature, and offered to help improve the clarity and precision of the discussion. I have certainly found the results to be interesting, and believe that the manuscript will be ready for publicationafter a round of minor revision.

Minor, Specific, and Typographical Comments:

1. Introduction
L22: Rather than “down to”, consider “drawn from” as a potential substitution in the text.
Changed applied as per reviewer suggestion

L26: I think the ending of this sentence is a bit unclear. Is this sentence referring to the ideas of PV invertibility or quasi-geostrophic theory? Consider a revision that improves the clarity of this sentence.
Sentence restructured to emphasize we are referring to the ideas of PV invertibility:
“PV invertibility became more refined and generalised through the development of quasi-geostrophic theory (Charney and Stern 1962) and is still continually being developed and improved on today.”

L49: The first two sentences of this paragraph are a bit redundant. Consider a revision to streamline the text a bit more.
Sentences have been restructured as suggested.

L52–54: I find this particular sentence to be a bit vague and confusing. Could it be rewritten for improved clarity? I view this sentence to be important for setting the stage for the forthcoming analyses.
Sentences have been restructured as suggested.

L59: I am still not sure why it should be expected that the effect should be different in the Southern Hemisphere versus the Northern Hemisphere, especially in an idealized environment. Consider corroborating the relevance of this statement more or simply keep the focus of the study on systematically examining the characteristics of these intrusions from an idealized perspective.
We agree that from a mathematical stand-point that the hemisphere in which we are working may be insignificant. However, from the perspective of science in the global south, we feel that this study is significant as most of the work has been done from a northern hemispheric perspective. We would therefore like to keep this distinction in this work, even if this point is made subtly

2. Methodology
L181: Consider revising the text to read as, “…is comprised of…”, for greater clarity.
Sentences have been restructured as suggested.

Overall: I love the table to summarize the various experiments – a great resource while evaluating the results.

3. Results

L212–213: I view this as a bit of a "chicken or the egg" type of description. I'm not sure I'm comfortable with saying that COLs are generated by stratospheric intrusions of high PV, since they *are* stratospheric intrusions of high PV. The generating mechanism, then, is what *causes* the intrusion. Consider a revision to the text accordingly.

The concern regarding the wording is understandable. We have changed the phrase suggesting COLs are "generated by" stratospheric intrusions to "associated with"

L216: I believe the figure reference should be to Fig. 6b in this line, rather than Fig. 7b. Figure references appear to be off by 1 in many instances after this point in the manuscript.

Corrected. There was an issue with figure references which have been corrected.

L221: This quantity should be negative since we're in the Southern Hemisphere.

Correction applied as per suggestion.

L224: Consider referencing any specific figures from this prior work that may help direct the reader to better verify this connection.

We have added in a reference to the Table in this previous work where this is shown.

L239: I find the figure referencese to be unconventional. Consider using a more standard a,b,c,d,e,etc. label for panels rather than mixing numbers and letters multiple times (i.e., avoid 7b2 and stick with 7a, 7b, etc.).

The naming convention we have used references the p[art of the figure we are referring to. Ie. Here we refer to column B, row 2 of Figure 7 (Figure 7-B2). To clarify this naming convention we have included a description of this convention in the first instance of this (Fig 8)
"By convention, in-text figure references refer to the column letter and the row number of the panel referenced (ie B2 refers to the panel in column B and row 2)"

L292: For additional clarity, it may be worthwhile to emphasize that this text refers to the austral summer.

Change applied as suggested

L330: At the same time, the reduced tropospheric static stability in the high tropopause case canalso allow for the circulation induced by the upper-level PV anomaly to penetrate to lower altitudes. I wonder if this is why you still see an effect of a lowered surface relative vorticity in Fig. 10 for the high tropopause cases, but its muted due to the competing effects between the penetration depth of the circulation and the height of the anomaly?

L341–342: These two sentences are a bit repetitive, could one be deleted for improved concision.

The sentence has been combine and reduced as suggested:
"Enhanced cyclonic circulation is induced at the surface in the lower tropopause scenario as shown by an increase in the amount of cyclonic surface relative vorticity."

L369: This is largely semantics, but I view the use of the word, "intensity" throughout the manuscript to be a bit confusing. Namely, when I see intensity I instantly think "magnitude", but here the discussion refers to radial depth. Consider reviewing the text in the manuscript to improve the precision with which these changes to the PV anomalies are described. For example,in L380, "magnitude intensity" are the same words, from my perspective. Could it be possible tojust keep reference to intensity in terms of anomaly magnitude and use radial depth or vertical depth to refer to changes in the anomaly's geometry?

The confusion of the different "intensities" is very well noted. The word depth is however also

ambiguous in this work since we also refer to depth with regards to the intrusion's "reach" towards the surface. In order to clarify we have changed our references to "vertical intensity" referring to the increased radial depth by referring to "vertical extent" or "radial height" or both.

L424–425: I believe this sentence refers to the wider PV anomaly, correct? If so, consider a revision to the sentence clarifying this point.
Correct. Sentence was incomplete. It has been rectified.
"Conversely, the wider intrusion results in an increase in the maximum mid-tropospheric meridional velocities compared to the standard configuration (15m.s^{-1} compared to 11m.s^{-1} in Experiment 0)."

L434: Typo: One "shallower" should be removed from this sentence.
Change applied as per suggestion

L438–441: Consider providing a physical explanation for this (i.e., the horizontal scale of the surface pressure distribution is larger for the broader anomalies, and thus the pressure gradient does not necessarily get much stronger as you increase the radial width of the anomaly).
A physical description similar to that offered by the reviewer has been added.

4. Discussion and Conclusions
L478: Consider emphasizing that you are referring to the height above ground level in this sentence to promote further clarity.
Height AGL has been specified as per suggestion

L516: Consider adding the word, "environment", after baroclinic in this sentence.
Change applied as per suggestion

Figures and Tables:
Fig. 8: I remain a bit confused as to what the difference between minimum relative vorticity and minimum cross-sectional relative vorticity is. Could this difference be more clearly identified within the body of the text before this first figure is introduced? Apologies if it is described earlier in the text and I missed it.
We have added in an explicit definition as per the reviewers suggestion at the first reference of minimum cross-sectional vorticity in the text.