

# 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)
- 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.
- c) title: following the major comment of reviewer 1, I suggest to replace cyclogenesis in the title, since just the cyclonic response of the stratospheric intrusion is investigated and no cyclogenesis

#### 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.

## **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).

### Detailed comments

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

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

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

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

L34: Remove „the“ before based on

L36: add e.g. before Lackmann 2011

L38: high PV anomalies -> low (SH)

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.

L40: can? In my opinion a positive anomaly (NH) is always associated with cyclonic flow

L41: has been -> have been

L42: has -> have

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?

L71: again, what do you mean by amount?

L90: not integrating.. inverting!

L91ff: 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

L407ff: reference to figures wrong.. -> Fig. 14 and 15 and not 15 and 16.

L459: „show the development of an amplifying trough“, where do you show that? no development can be observed without time evolutions investigated

L532: Barnes et al. 2021 referenced twice

### Figures and tables

Fig1: \* remove one represent in caption

\* think -> thick

Can figures 2,3, and 4 combined to shorten manuscript? All figures really necessary?

Table 1: \* Experiment 5 - typo 100km-100km?

\* Experiment 4 - either -1:-0.1:2 or -2:0.1:-1