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PART I:

SUMMARY of the response to the comments of Reviewer # 1 on 'egusphere-2025-3017'
Transient Flow Patterns of an Annular-like Stratospheric Polar Vortex

Huw C. Davies and Michael A. Sprenger

Below a summary of the Reviewer's comments is provided in italic and in black type and our response is given in blue

The thrust of the Reviewer's remarks were that :-

- (i) *the observational component of the study was incomplete and that the derived results did not add to our knowledge of the features under discussion, and*
- (ii) *in the theoretical component, insufficient reference to, and discussion of, previous studies of barotropic instability were made in our manuscript, and that the derived results might well be neither novel nor substantive.*

We have already provided a detailed and vigorous rebuttal of Reviewer #1's remarks (- see <https://doi.org/10.5194/egusphere-2025-3017-AC1>), and here we indicate the changes made to the manuscript in response to those remarks.

These changes are to counter the Reviewer's remarks by the addition of two sub-sections: - a further sub-section at the end of Section 2 (viz. sub-section 2.6); and further sub-section at the end of Section 3 (viz. sub-section 3.5). Some of this material already appeared in the final section of the original manuscript and this in turn has enabled us to streamline the manuscript's final section (Section 4).

The criticisms made in point (i) above are addressed in sub-section 2.6 by

- (a) noting briefly the aim and objectives of the observational component,
- (b) indicating why the Reviewer's desire that we compile a climatology is premature, and
- (c) listing and re-emphasizing some of the important results/deductions of our case studies,

The criticisms in point (ii) above are addressed in sub-section 3.5 by

- (a) stressing briefly that our overarching, and clearly stipulated, aim was not to replicate or refute the results of previous studies but to acquire a deeper understanding of barotropic instability and stability as it pertains to the stratospheric polar night jet.
- (b) amplifying the previously terse discussion of the strengths, limitations and relevance of previous barotropic instability studies in so far as they relate to the present paper, and re-emphasized that there are also possible alternative or mixed forms of barotropic-baroclinic instability at stratopause levels,
- (c) noting and accounting for the similarities and a key difference between the results of the

present study and those of the most pertinent previous papers,
(d) re-emphasizing our results that are new and substantive, and in particular drawn further attention to the novel extension of conventional instability studies of the polar night jet to the evolution of non-normal modes and its possible implication for vortex break-up at stratopause levels, and

Note that much of the additional material in these two sub-sections also represent responses to remarks by the second Reviewer.

PART II

RESPONSE TO RC2 (Reviewer # 2, Peter Hitchcock) on 'egusphere-2025-3017'

Transient Flow Patterns of an Annular-like Stratospheric Polar Vortex

Huw C. Davies and Michael A. Sprenger

Below the reviewer's comments are in italic and black type and our responses are given in blue

This is an interesting manuscript that discusses the dynamics of synoptic-scale vortices that form near the edge of the upper stratospheric polar vortex. The authors present three well-chosen case studies, then develop a barotropic theory for their origin in terms of the instability of an annulus of vorticity on a polar beta plane. The linear version of this theory suggests a fastest-growing zonal wavelength for these features that depends strongly on the geometry of the annulus (narrow annuli are more unstable and break down into smaller-scale features). This is extended to non-linear evolution through numerical solutions of a contour dynamics code; this is used to identify the tendency for the development of closed regions of high PV with similar scales and geometry to those identified in the case studies.

Relatively little attention is paid to the upper stratospheric vortex, and in my view this work clearly demonstrates the fact that there are interesting and distinct dynamical processes that shape this part of the atmosphere. I have some questions about the theory, and some concerns about the presentation, but none of these are central to the core ideas of the paper. I would recommend minor revisions.

We thank the Reviewer for his careful and considerate review and we appreciate his constructive and insightful comments. We either agree with those comments and/or recognize the need to further clarify our text on various issues.

General Comments:

1) Choice of basic state

The parameters of the basic state are chosen such that the absolute vorticity in the inner region over the pole is fixed at zero, then for $C > 0$ increases with co-latitude away from the pole,

Assuming the existence of an annular region in the upper stratosphere then to replicate this in our model requires the absolute vorticity to increase with co-latitude at least somewhere poleward of the annular region.

and the character of the instability is controlled in some sense by the choice of surf-zone vorticity through the parameter n or r_c .

Agree, and note that for replicating the atmospheric situation then $-1 < (nC) < 0$.

This seems a bit strange, partly because the stable state is one in which the absolute vorticity increases with co-latitude, opposite to

The existence of an annular structure and flow instability requires an extremum of PV.

the stable case that usually obtains in the lower stratosphere in which the absolute (or potential) vorticity increases with latitude.

Agree.

I suppose this would be inconsistent with global constraints on PV on the sphere, though that probably doesn't have any implications on what is being concluded here.

On the sphere the Circulation Theorem imposes an integral constraint on the net (i.e. globally area averaged) relative vorticity.

In addition the further requirement of inertial/symmetric stability of a purely barotropic fluid would require that for an axisymmetric basic state the absolute vorticity in the NH (SH) is locally positive (negative), and hence the relative vorticity must also be zero at the equator.

On a polar beta-plane there is no à priori equivalent to the integral constraint. A condition of zero relative vorticity imposed at the beta plane pseudo-equivalent of the equator ($r_c^2 = 2$) would imply for our model that a state of (unlikely) neutrality to symmetric perturbations would pertain in the entire *b-to- r_c* domain (in effect the surf zone).

The results of our idealized model can be viewed as internally consistent provided the disturbances decay sufficiently rapidly in the far-field.

We have determined that this is the case in our contour dynamics simulations and this supports the Reviewer's surmise. We have drawn attention to this issue in our revised text.

It would seem to me like it would be more intuitive to fix the surf zone and the vortex edge, then vary the inner vortex vorticity.

First it is appropriate to note that the free external parameters for the specification of the basic state are:- the absolute vorticity of the inner, annular and outer regions and the location and width of the annulus. In addition there are constraints regarding inertial instability and the character of the flow in the outer region. In the limit of specifying these parameters and meeting the constraints the instability problem is uniquely determined.

Our approach has been to select a basic state absolute vorticity profiles (and with it the corresponding velocity profiles) that ensure a relatively quiescent inner region, a jet capable of spanning the range of observed values, and a value for the absolute vorticity in the outer region that is both consistent with inertial (as opposed to barotropic) stability and yields realistic values for the zonal velocity and /or the relative vorticity at our model's equivalent of the sub-tropics.

To limit the number of free parameters we have set the value of the absolute vorticity in the inner region to be that equivalent of the earth's rotation and close to that of observations. We also determined that reasonable variation of this parameter would not result in significant change to our results. For the observed relatively quiescent inner region, this would be a minor change and we have now noted this in our revised text. Formally the modified form of the key equations (Eqs. 10 & 11) are now given as Eqs. (18 and 19) in one of the new sub-sections.

The Reviewer approaches the issue from diametrically the opposite standpoint. It is to first fix the value of r_c and the outer radius of the annulus. (This equates to specifying the location and width of the surf zone and the subsequent results hinge upon the

reasonableness/representativity of this specification). In addition this leaves open the specification of either the jet strength or the parameter determining the amplitude of the ir-rotational component of the flow in the outer zone. Specifying either parameter determines the net area averaged relative vorticity of the two inner regions.

In essence the difference between our approach and that of the Reviewer is that we specify the relative vorticity distribution in the inner region to be weak and verify that this is reasonable whereas the Reviewer's approach fixes the structure of the outer region.

2) Section 3.2.4

I am mostly convinced by the theory in this work, but this section is a significant exception. The dynamics being described here are an initial condition in which there is significant Rossby wave activity on the outer PV contour, but no wave on the inner PV contour. If the basic state is in fact annular ($n < 1$ so that the absolute vorticity gradient reverses) the state is barotropically unstable, and the waves grow. This is basically the instability identified by Eqns (9) in section 3.2.1 (although it's not clear to me why these conditions aren't identical; (13) has the same form as the normal mode assumed in 3.2.1 and I don't see any approximations of the dynamics).

To pinpoint the difference between our and the Reviewer's view of this Section we note that: (i) Section 3.2.4 deals with a non-normal mode form of development (- which allows the structure, as well as the amplitude, of the perturbation to evolve with time) and therefore it differs from that of a normal mode analysis.

(ii) In particular for a basic state with an annular structure, the initial growth rate and wavenumber of the fastest growing non-normal mode can be, and generally is, significantly different that of the fastest growing normal mode. Our analysis shows that for a specified initial unperturbed state there exist $m = 1$ and 2 non-normal mode perturbations that would have a growth rate surpassing that of the corresponding fastest (and synoptic-scale) normal mode.

(iii) The above result underlines the significance of Section 3.2.4 and is illustrated by the simulation reported in Fig. 17 along with the comment in our accompanying original text. The corollary is that this points to the potential efficacy of the gravest-mode Rossby-wave forcing to the break-up of an annular stratospheric vortex.

We have sought to further emphasize these points in our revised manuscript.

If the basic state is in fact annular ($n < 1$ so that the absolute vorticity gradient reverses) the state is barotropically unstable, and the waves grow.

Note that $n < 1$ is only a necessary (but not necessarily sufficient) condition for instability see for example our Eqs 8 (a, b).

However, when the system is stable, the growth of the wave on the inner contour is just a description of wave propagation from the outside contour to the inner one. There may be growth of the perturbation in the sense that the poleward propagation will focus more pseudomomentum into a smaller radius (and that will have potentially a larger impact on the

winds given the smaller moment arm).

We fully agree with the gist of this argument but for the present purpose this is not the key factor. In line with our related remark above, we distinguish between: -

- (a) the derived criterion for normal mode instability which is $\Delta > 0$ (see Eq. 9a), and
- (b) the necessary (but not necessarily sufficient) condition for instability which is $(1 - n) > 0$.

In Section 3.2.4, Eqs (14a, b) imply that “the inner and outer waves can grow synchronously irrespective of the value of Δ . In effect there can be transient synchronous wave growth. We have clarified this point in the text, and it differs from the mathematical setting postulated by the Reviewer.

Calling this 'scale selective secular growth' is both wrong (the growth is not secular) and an unnecessarily complicated description of a an already familiar process.

This term has been removed in the text.

It's kind of mathematically satisfying to see this expression of familiar dynamics, but the interpretation, in my view, is unhelpful and distracts from the main results of the paper.

See points (i) and (ii) above

If this section is meant to motivate Fig. 17, I am a bit lost on the particulars as my understanding is that the latter is in the unstable part of parameter space.

See point (iii) above

Presentation issues

1) Use of NCEP/NCAR and ERA Interim reanalysis products

These structures are to some extent robustly captured by different reanalysis products, as was shown by earlier work by the authors. However, the reanalysis products (especially the NCEP/NCAR one) are very old, and many studies including the SRIP report have identified serious concerns with the use of these reanalyses for the upper parts of the stratospheric vortex. I would strongly urge the authors to use MERRA 2 output or another more modern reanalysis which is constrained by assimilation to some of the limb sounding satellites that provide information at these levels.

I don't anticipate that this would change anything consequential for the conclusions drawn here, which is why I consider this a presentation issue. But of the relatively small number of people who will be interested in the dynamics of this region, my guess is that there are a significant number that would see the use of these old reanalyses and immediately disqualify this paper on the grounds that they are known to be untrustworth in their details.

We are sensitive to, and appreciate, both the content and sentiment of this comment. For comparison purposes, three new figures have been included in a new Appendix of the revised manuscript. Each of these figures displays key PV patterns of the present study alongside corresponding ones based upon the MERRA2 Reanalysis. (We deem the choice of PV as the selected variable to be appropriate since it involves both the rotational component of the velocity and the thermal field whilst avoiding the divergent

wind component that can be / is influenced by the differing treatment of inertia-gravity waves at stratopause levels in the various Reanalysis fields).

The similarity of the seminal sub-planetary scale PV features in the ERA-Interim and MERRA2 is striking and testifies to the consistency (and arguably the adequacy) of the two Reanalysis fields representation of these features that are the focus of the present studies. We have referred explicitly to this reassuring comparison in the Appendix of the revised text.

2) Figure 13

Please use different colours, line thicknesses, linestyles or otherwise to distinguish these curves, and include a legend that reveals what parameters are being varied. The explicit values also need to be given somewhere. The text does not make this clear.

The Figure has been redrafted in line with the Reviewer's comments.

3) Figs. 14 and 15

I find both of these figures to be hard to interpret. I think that the main point of Fig. 14 is to argue that m^ is approximately equal to m_0 . It would be much more helpful to simply plot those two curves against each other, or the ratio of m^* to m_0 if you really want to capture more of parameter space.*

The purpose of Figure 14 was indeed to seek to illustrate that m^* is well approximated by the value of m_0 (in accord with Eq.12). Now (m^*/m_0) is a function of five variables (r_c , b , C , ϵ , and m^*), and the challenge was to display concisely this multiple dependency.

In our revised figure we provide two panels that together confirm the close correspondence of m^* and m_0 .

I don't know what I am supposed to take away from Fig. 15.

We apologize for the original form of Fig. (15). In the revised manuscript we have redrafted Fig. 15 and it now has two panels that successively provide estimates of the most unstable normal mode and then sheds light on the factors influencing its growth rate. The accompanying new additional text helps to convey the interpretation of the Figure's content

4) 'Ready Reckoner'

I am not familiar with this expression - is this meant to indicate that m_0 is a good approximation for the most unstable wavenumber?

Definition of Ready Reckoner - a device to facilitate the rapid calculation of a commodity.

The inference here is that the wavenumber of the most unstable mode can be easily evaluated without conducting a further elaborate analysis or calculation. We have clarified this in the revised text.

I am also a bit confused by the discussion around setting the parameter $D = 0$; from (8) this parameter is set by the basic state and does not evolve in time. Is the idea that assuming it is small is a reasonable or useful approximation?

From a PV standpoint the value of the parameter D is fundamental. As stated in the original script the criterion $D = 0$ implies that the counter propagating waves on the two discontinuities are conducive to phase-locking and this in turn can, in principle, lead to

sustained growth of both waves. This circumstance lies at the heart of a physical interpretation both baroclinic and barotropic instability.

5) Use of acronyms

I urge the authors to avoid the introduction of new acronyms like SSS, and please consider avoiding those like SPV, given how close to PV it is.

Use of the SSS acronym is avoided throughout in the revised script, but we have retained the SPV acronym at several locations because it is in common usage (but recognize that its closeness to PV renders it an uncomfortable compromise).

Specific Comments

124-25: Lawrence and Manney (2017) is another notable effort to define the vortex edge across the depth of the vortex. There is also work in the hurricane community on the instability of annular PV structures (e.g. Shubert et al 1999) that seems worth mentioning. Papers are now referred to and referenced.

140: This is to some extent separate from the main developments of this paper, but understanding how and why this annular structure arises would seem to be a very relevant question. It's not obvious from my reading of Shine (1987) that the PV reversal of the basic state is all that clearly understood.

We agree that understanding the origin of the annular structure is an important and open question, and one we have (deliberately) not addressed in our manuscript. Not least because our thoughts on, and understanding of, this issue are decidedly immature.

Calculations (not shown in the manuscript) of the PV structure on isentropic surfaces based upon Keith Shine's 'radiatively determined state' do show an annular structure. However, the procedure of first determining the thermal state and then evaluating the flow required to maintain balance cannot of itself account for the origin of the PV.

Nevertheless its latitudinal location is interesting and probably indicative because the observed annular region is not co-located with the edge of the time-dependent polar night. This would imply that an additional mechanism is required to sustain the semblance of an annulus. We refer obliquely to this issue in the revised manuscript.

151-61: I am a bit confused by what the authors are intending to distinguish in describing these two classes of disturbances. Are the first class meant to be features that arise from in situ instabilities? Or transient versus stationary features? It's not clear to me what the distinction is between 'planetary-scale zonally propagating waves' in class one and 'planetary-scale Rossby waves' in class two.

This distinction has been clarified in the revised script.

1664-670: I'm not so convinced that this is comparable to a sudden warming - at least not for the evolution of the lower/mid stratospheric vortex (e.g. Fig 10d-f). The mixing of low PV air is confined to within a pretty narrow latitude band within the broader vortex edge region.

We do not wish to emphasize a similarity (or lack of) with the structure of an SSW at lower/mid stratospheric levels, and agree that the mixing of low-PV air into the vortex is limited, but note that the break-up of the annulus occurs at a time close to

(and arguably prior to) that of an SSW.

It is also conceivable that, at stratopause level, the break-up of a polar vortex with an annular region of high PV need not necessarily require the advection of air into the core, but merely arise from the aggregation of the enhanced PV of the annulus into, say, two sub-vortices. This was noted in the original manuscript and is re-emphasized in the revised script as it relates directly to the break-up of the polar vortex over a depth of the stratosphere.

Presumably that length-scale is set by the most unstable zonal wave number, which is going to be high for the unstable annular PV structures.

As noted above, the essence of Section 3.2.4 was essentially to assert that, the presence of Rossby wave forcing at small wave number coupled with the existence of an annular structure, the realized pattern would **NOT** be the most unstable normal mode, but the fastest growing non-normal mode which would itself be of low wavenumber. This point has been re-emphasized in the revised script

1680: This is a special case of quasigeostrophy with infinite deformation radius, no?

Formally this is the case in the standard quasi-geostrophic limit.

At any rate there isn't any description of the thermal or vertical structure of the observed vortices, so it seems strange to me to highlight some notional non-QG feature of the present model.

The case for pointing to the non-QG dynamics of the OBSERVED sub-planetary scale features is based upon the strength of their relative vorticity is comparable to that of 'f'. This renders the dynamics of the synoptic-scale features nominally inconsistent with formal QG dynamics. In addition, an inference that relates to the possible efficacy of the barotropic model at stratopause elevations is that our Case Studies indicate that at these levels the influence of the horizontal divergence term in the vorticity equation is comparatively weak. We allude to this in the revised text but acknowledge that it is an observationally based inference.

1688-689: It is not clear from this sentence whether the mid-latitude PV annulus is unrealized in the present work, or in Hartmann 1983. I think the former, but please clarify. The use of 'special' is also ambiguous and seems like an unjustified criticism given the extreme idealizations of the present work.

There is little (or no) observational evidence of a double annular structure at stratopause levels, and certainly there is no such structure in the three case studies considered by us. However, an instantaneous N-S cross section at a given longitude might give the appearance of such a structure provided there is a trailing streamer of high PV present within the surf zone at that longitude. We have referred to this in the revised script. Interestingly inspection of reanalysis fields for the cross-section given in Hartmann indicates that this might well be the prevailing setting in that case.

The use of 'special' is also ambiguous and seems like an unjustified criticism given the extreme idealizations of the present work.

This was not meant as a criticism of the Hartmann paper and the text has been rephrased accordingly. The significance of second (sic. double) annular structure is noted in the revised script. It allows for three kinds of possible unstable modes:- two associated separately with the two individual annuli and of high wavenumber, and a third with a low wavenumber associated with reversal of the PV gradient across the broader PV trough formed between the two annular structures. This is now mentioned in the revised text.

References:

Lawrence and Manney JGR 2017 (10.1002/2017JD027556)

Schubert, W. H. et al. JAS 1999 (10.1175/1520-0469(1999)056<1197:PEAECA>2.0.CO;2)

These papers are referred to in the script and referenced.

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