Response to the Comments of the Co-Editor

Our responses are given in red below, and the specified line numbers refer to the "New Version" of the manuscript.

…… It was reviewed again by both reviewers. Reviewer 1 remained highly critical and was still convinced that the presented work did not meet the minimum requirement for publication, recommending rejection. Reviewer 2 was supportive of publication, although he raised a few additional points. In trying to come to a decision I've carefully conducted a review of your revised manuscript along with responses to the first round of reviews, as well as the reviewer reactions to your responses. My overall assessment based on all of these is that your manuscript should in principle be publishable in WCD, but that important revisions to the presentation (and possibly additional analyses, revisions to the interpretations) are necessary (see detailed comments below). I should note that my own comments reflect that I feel somewhat sympathetic toward both reviewer stances and I view both as representative of possible reader impressions from the targeted community. In this sense I think both stances should be taken seriously and my comments therefore in part provide an attempt to hit the middle ground.

We acknowledge the balanced and substantive nature of these overview remarks and have sought to formulate our responses accordingly.

In your responses and revisions to this round of reviews please provide responses to all reviewer comments, as well as my additional comments below.

Main/General comments (all line numbers refer to the track changes version):

1) I agree to some extent with the sentiment raised by reviewer 1 toward the observational component of the study. Stating that a few selected case studies "suggest that sub-planetary flow features are prevalent" (abstract), at least based on these case studies alone, seems unjustified (and seems to be the one conclusion one simply cannot draw from case studies). I don't think that this type of statement is needed and recommend highlighting specific aspects of the flow features in such a summary statement instead. These should focus on details that are not already pointed out in DS24.
A similar comment holds for the statement at the beginning of section 4.0, that this study lends credence to the assertion "that the vortex's periphery is very frequently populated with distinctive and dynamically significant sub-planetary scale flow features". If you remove "very frequently" from this statement then it seems justified.

(i) The text has been modified to eliminate the references (- see Abstract, 400, 837, 853 and 855) to the prevalence of the studied features. As set out in our response to Reviewer 1 the changes are as follows: - in the Abstract the term '*prevalent*' has been deleted and emphasize placed on the objective of the first part of the study. In Line 400 we have replaced '*prevalence*' with '*existence*'. In Line 837 we have replaced 'is' by 'can be', in Line 853 we have replaced '*prevalent*' with '*occurrence*'. In Line 855 we have replaced '*often*' with '*possibly*'.

The validity of remainder of the manuscript's text, conclusions and discussion ARE NOT reduced or modified by these changes.

Again, isn't it the nature of a case study that you cannot draw conclusions about the frequency of the phenomenon of interest? As a reader one wonders why you choose to highlight the frequency of occurrence of the phenomenon instead of common characteristics based on your case studies.
As noted above the four words/phrases that referenced the prevalence of the features have been removed from the text.

That said, given that between DS24 and here you presented several snapshots at very different times demonstrating the existence of these flow features I think it's fine to speculate about the frequency, as long as the respective statements are declared as such (I will note in passing that as a casual statement I'd agree with the authors -- as someone who follows the vortex evolution every winter, I've seen these flow features fairly frequently myself. Nevertheless, given the evidence _provided_ in the manuscript the statement remains unjustified.)
In our response to Reviewer 1 we have supplied (near Line 395) the following pointer to the 'frequency of the occurrence': …. at the 2mb level on every January 15th of the 41 year ERA-Interim data set, SSS features and/or an annular-like structure were present on 24 occasions, the PV vortex was severely disrupted on another 16 occasions (of which 11 were related to major or minor SSW events at 10 hPa), and on one occasion there was a single pole-centred PV vortex.

I also recommend to better motivate the case studies and this should be fairly easily doable: my impression is that the selected cases are meant to contrast: 1) fairly typical, i.e. somewhat dynamically disturbed vortex behavior in the NH (case 1, in your words "comparatively non-descript"), 2) dynamically less disturbed SH vortex (case 2), 3) highly disturbed behavior associated with a developing SSW (case 3). I recommend modifying the sentence on lines 74-75 accordingly.
This recommendation has been followed in the revised script both in the Abstract and (Line 77-79).

2) (This point was not raised by one of the reviewers, but I feel is important nonetheless.) The potential significance of the studied flow features suffers from showing PV on pressure surfaces (this also holds for DS24). This choice is neither motivated, nor are justifications for it presented. It furthermore conflicts with the authors' statement that they "adopt a PV perspective (Hoskins et al., 1985)" (line 36), and that "its evolution is rendered more transparent when examined on isentropic surfaces" (line 123, which is part of the main point of the Hoskins paper, so to me is part of "PV perspective"). So why use the less transparent rendering on pressure surfaces, especially when the later claims about necessary condition for instability (a la Charney & Stern) demand the use of isentropic surfaces? The fact that Figs. 7, 10 do show PV on isentropes suggests that the relevant data is available to the authors.

Perhaps the authors did test the sensitivity of their conclusion w.r.t. choice of vertical coordinate, but I couldn't find any statement about this in DS24 or the current manuscript. The shown temperature maps do suggest that thickness variations are an important ingredient of the highlighted PV structures and I'd suspect that these thickness variations are partially masked by using pressure instead of potential temperature as vertical coordinate. In any case, at the least some justification needs to be provided for why the use of pressure surface suffices for the presented analyses.

I: The underlying rationale for our approach is as follows:

(i) the full three-dimensional PV structure of the studied features at a given instant can be portrayed in various vertical coordinate systems, and each would be self-consistent. WCD readers might be more accustomed to assessing the instantaneous structure of primary variables (or associated derived quantities) above 10hPa in displays showing their distribution on a series of appropriate pressure (or height) levels or equivalent vertical sections. (In the last twelve months there were circa. 35 figures spread over 7 papers in WCD that used a pressure (or height) coordinate system to show features above 10hPa, whereas there were no figures that showed isentropic displays.)

(ii) to capture (a) the time-development of the PV pattern, (b) the possible Lagrangian-conservation of the PV of the features under consideration, and (c) to assess the dynamics and possible instability of the flow, then it is appropriate to show a time-sequence of displays on pertinent isentropic surfaces.

These remarks are now rephrased (Lines 125-129).

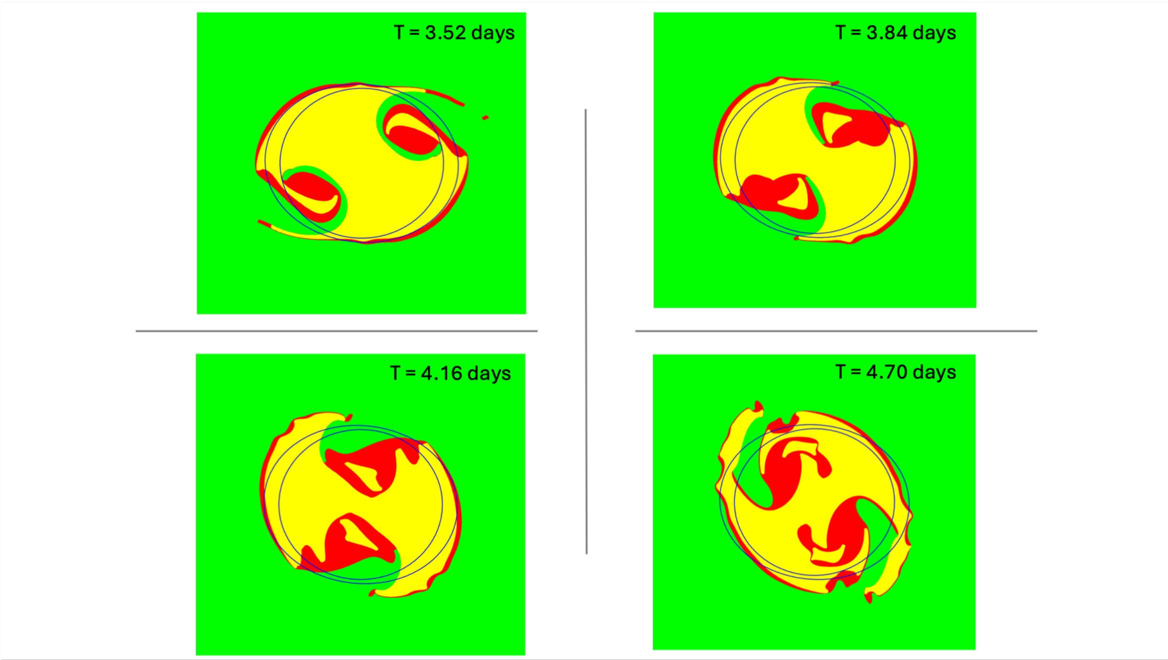
II: We agree that the structure of PV features can indeed be masked (or in some settings over-emphasized) when using pressure as the vertical coordinate. Statements related to this point were in our "September Text" and have been retained in the new text, but also noted explicitly in Lines 129-132.

Two other remarks related to the pressure versus isentrope displays: (a) The displays shown in Fig. (6a) and Fig. (7e) show a close correspondence of both the individual features and the rim-like structure on pressure and isentropic surfaces.

(b) Contrariwise the rapid vertical increase of the background PV can act to diffuse/sharpen the equatorward edge of an annular feature on a pressure surface above/below the stratopause relative to its structure on a contiguous isentropic surface. We have inserted comments that reflect this point near Line 263 and near Line 130-132.

3) Similar to reviewer 2, I don't find the claimed relevance of the barotropic model results for split SSWs to be convincing. Unlike reviewer 2, I don't find the language in the relevant sections to be speculative enough. For example, a similarity between Fig. 17 and the development of a split SSW is claimed on line 748/9, but seems unjustified. The situation in Fig. 17c looks nowhere near the situation just before a vortex split and as far as I understand

the evolution in this case does not produce a split at later times either (correct me if I'm wrong): the shown behavior is not due to an instability but rather comes about via transient (non-normal) growth. So I suspect that the shown wave 2 perturbation does not grow much further at later times and the canonical peanut-shape vortex structure indicating an ensuing vortex split (e.g., Matthewman & Esler, 2011), in fact, does not develop.
In dimensional terms the time-span of the simulation in Fig.17 was 3.2 days. To help clarify some of the issues raised above, the four panels in the figure below shows the development after a further 0.32, 0.64, 0.96 and 1.6 days.



The successive panels show (i) a more pronounced splitting of the annulus into two absolute vorticity centres, (ii) trailing PV streamers within the surf zone, and (iii) significant incursion of mass from the initial quasi-annular domain and from the weak outer domain into the innermost domain. The former reduces the relative vorticity of the two centres and the latter constitute two comparatively strong anticyclonic features.

In light of the above the latter part of the statement in the "earlier text" that is deemed unjustified has now been amended to read (Lines 760- 763) "and the pattern and its subsequent development (not shown) bears a modicum of resemblance to the split classification pattern that can emerge during an SSW event. However the simplicity of our model cautions against overstating the relationship to realized SSW events."

So I don't see support of the claim that the "vortex breakdown is attributable explicitly to the existence of the annular band" (line 756).

The second null-experiment reported in the September Text (at Lines 752-756) is for a simulation with a vortex of uniform absolute vorticity and an initial jet of the same strength and location as that for Fig. 17, and this is akin to the configuration adopted by Matthewman and Esler.

The support for our statement is that the latter configuration is stable and the peanut shape is retained in time. The inference, at least for this setting, is that the break-up of an annular structure is accomplished more easily than that for a comparable vortex without an annular structure as stated explicitly in Lines 848-850.

(In metaphorical terms, the canonical peanut-shaped structure requires splitting the entire peanut, whereas the presence of an annular band simply requires splitting the empty shell.)

The portrayed "synergetic mechanism" (second to last paragraph in section 4.0) seems to represent speculation, but is not sufficiently clearly framed as such. The shown results by themselves don't seem to represent support for this conjecture;

In the new script the offending statements have been rephrased far more circumspectly (Line 860-864)

… fully non-linear simulations actually showing the vortex breakup seem to be needed.

See our earlier remarks related to Fig. 17.

The conjecture also seems to be at odds with the claim that the PV band very frequently develops sub-planetary flow features, whereas we know that vortex breakdowns/SSWs happen much more rarely.

These two statements are not mutually incompatible. The first of the two null-experiments reported in the text (Lines 764-766) shows that in the absence of an initial wave $m=2$ disturbance results in the expected development of sub-planetary features. In contrast the second null experiment (766-770) shows that, in the presence of such an $m= 2$ disturbance together with an annular structure, the development of the sub-planetary features are inhibited and a double vortex-like structure evolves rapidly.

Another related question is how the envisaged relation of the barotropic model results to SSWs are similar or different to documented quasi-barotropic SSW-dynamics in the literature?

This point is commented upon below in our response to the query raised regarding Line 85-87

For example, Matthewman & Esler study essentially barotropic dynamics of SSWs, but from a different perspective than adopted here - this paper is currently only referenced in passing in the introduction, but not in the section on barotropic model results.

In fact I couldn't find any references listed along with statements about SSWs, which seems odd given that quasi-barotropic SSWs have been studied in a few papers (this was also raised as a general critique by reviewer 1). For example, Scott (2016, https://doi.org/10.1002/qj.2788) studies repeated split/merger vacillations in a barotropic model and seems relevant (e.g., the split part of those dynamics would involve large wave-2 amplitude growth without an ensuing irreversible vortex breakup). It's clear that the background flow setup used here is different from past work, in particular related to the annular PV band. But it'd be helpful to put the results into perspective, ideally with pointers toward how one may distinguish the portrayed mechanism(s) in the present study from those in the existing literature (e.g., based on real atmospheric data).

The Matthewman & Esler study is now further referenced upon near Line 801 and the September text is now amplified to pinpoint the overlap between studies akin to those of the Matthewman & Esler and the present study (near Lines 810-812).

The distinction to the difference in the two kinds of developments is noted in Lines 864-868.

line 27: the 2009 Harvey et al. ref. is missing in the reference list

The relevant paper was already referenced in the September text. It is now referenced again in Line 63.

line 85-87: this may feel a bit unsatisfying; a reader may wonder what these earlier studies could already tell us about the "observed features" and what specifics of the barotropic model used here make it more useful than any of the existing models?

Most of the early barotropic studies were concerned primarily with the possible instability of the graver modes as a possible trigger for SSW. Here we seek to determine the factors determining the occurrence of the observed sub-planetary and synoptic-scale structures in the upper stratosphere, and concurrently reconsider the stability of the graver modes.

The utility of the present model is introduced at various junctures in the September text and consolidated in Section 3.5. This sequential approach is appropriate (and to a measure unavoidable) because the measure of our model's explanatory power and utility, relative to that of previous studies, can only be assessed after the evaluation of our results.

A summary of the model's utility is that it:- incorporates a simple and excellent representation of the beta effect for a vortex confined to poleward of 45^0 ; is analytically tractable and thus readily yields information on the wavenumber and growth rate of the most unstable normal mode for a wide range of basic state configurations; enables an insightful dynamical interpretation of the underlying instability dynamics; facilitates the interpretation of the results of earlier instability studies; and is flexible enough to examine the linear and non-linear development of non-normal mode initial perturbations.

These factors are for example exploited to:- demonstrate that the break-up of the PV annulus is more likely to yield sub-planetary and synoptic-scale features for a wide range of vortex settings; and to pinpoint the limitations of the basic state selected in many of the key earlier barotropic instability studies postulated arguably unrealistic structures and associated usually unrealizable instability of the graver modes.

line 190: clarify or spell out "SSS"?
Abbreviation now introduced near Line 155

AND line 226: clarify or spell out "PRW"?
The abbreviation PRW was already introduced in Line 63

line 383-384: I see that this was added in defense of the criticism raised by reviewer 1, but since the same statement could be made based on DS24, I don't find it helpful here; I recommend reformulating this to simply state that a climatological description of these features would be highly valuable.
Text rephrased (Line 394-398). It now refers to the desirability of establishing the features climatology. It also now provides one statistic (quoted above) related to the occurrence of the studied features

line 393: "ahead of ... SSW" - not sure I agree; the SSW is well on its way at the diagnosed time steps and it seems much more likely that the stratopause-level PV features are simply part of the developing large-amplitude non-linear dynamics of the SSW itself (and thereby different in nature from the other two cases).
The sentence has been rendered weaker - it now reads (Line 407-408)
"opens the possibility that the band might participate in, and contribute to, the initial development of such an event".

line 425: initially I thought this is a typo and that it should say n<1 (which is also how you sketch it in Fig. 11b). But then I realize that because you define Z to be "system-relative" (i.e., without the f_0=2\Omega-contribution), that n<0 is correct. In that case you need to adapt Fig. 11b, though, and add a comment in the caption that f_0 is not included in Z.
Figure redrafted

line 483: should the subscript on the first component be "a" instead of alpha? Corrected

line 512: remove one "located at" Removed

section 3.2.4: this is a subsection within 3.2 "Normal Mode Analysis", but the authors' response states that this "deals with a non-normal mode form of development" - I recommend to reconsider appropriate placement/naming of the section
We believe the naming to be correct. The final sentence refers to its upcoming relevance to Section 3.3. (Note also that sub-section 3.2.3 has been split into two parts in the revised script, so that Section 3.2.4 now becomes 3.2.5).

line 655: remove one "that" Removed

line 677: should "and a value of" read "and in"? Corrected

Response to Reviewer 1

Our responses are given in red below, and the specified line numbers refer to the "New Version" of the manuscript.

An Overaching Response.
In the last section of the earlier manuscript, entitled "Further Remarks", we began by framing our discussion around two assertions (Lines 838-839) and Lines (842-844). The Reviewer appears to have interpreted these assertions to be our main conclusions.

In point of fact the new results and the conclusions of our two-part study are set out earlier in the text in respectively Sub-Sections 2.6 (- entitled "Assesment of the Results") and 3.5 (- entitled "Model Critique and Assessment of Results"). The introduction of the 'assertions' in the Section 4 (- entitled "Further Remarks") was to provide a convenient framework and platform for making additional more general remarks.

To avoid possible ambiguity regarding our introduction of the 'assertions', we have qualified the expression 'lending credence' in Line 837 to read 'lending further credence'.

Unfortunately the authors have disregarded nearly all my comments, and the deficiencies in the original manuscript remain. The authors did write a long rebuttal but this was a lot of "smoke and mirrors" which involved mischaracterization of my comments, and did not respond to my actual comments. I will not respond in detail to these comments and instead re-iterate why I think this manuscript is not acceptable for publication.

In my view (and I believe this is a widely held view) a published paper needs to (i) provide support for its key conclusions, (ii) contain conclusions that make be a significant advance on what is already know, and (iii) accurately characterize what is already known. Unfortunately this paper does not do this.
We did previously, and do now, beg to differ with the Reviewer's assessment of our study.

The first key conclusion (or as the author's call them "assertions") is that "the vortex's periphery is very frequently populated with distinctive and dynamically significant sub-planetary scale flow features".
In line with our overarching remark above, this assertion was not introduced as a key conclusion of our study.

do not regard this As I said in my original review, 3 case studies is not enough to claim anything about the frequency of these events. Based on this manuscript a reader has no idea if these features are observed only 3 times in ~40 years and if always present. So there is not support for this conclusion. Note, I did not (as they authors made up) ask for a climatology, I just stated (point 1 in original review) that the authors cannot make conclusions on the frequency based on analysis in the paper.

The request to specify the 'frequency of event occurrence' could be construed as constituting a climatological query. A climatological study could indeed indicate the frequency and variation in the frequency of the events over the winter season, but ours was not a climatological study.

In reply the text has been modified as follows :

(i) In harmony with the Co-Editor's comments, we have eliminated the statements referring to the 'prevalence of the studied features'. These remarks occurred in the Abstract and Lines (400, 837, 853 and 855). In the Abstract the term '*prevalent*' has been deleted and emphasize placed on the objective of the first part of the study. In Line 400 we have replaced *'prevalence'* with *'existence'*. In Line 837 we have replaced 'is' by 'can be', in Line 853 we have replaced *'prevalent'* with *'occurrence'*. In Line 855 we have replaced *'often'* with *'possibly'*.

The validity of remainder of the manuscript's text, conclusions and discussion ARE NOT modified by these changes.

(ii) Elsewhere in the text (Line 394) we have added that a compilation of the features' climatology would indeed be desirable, and noted that …. at the 2mb level on every January 15th of the 41 year ERA-Interim data set, SSS features and/or an annular-like structure were present on 24 occasions, the PV vortex was severely disrupted on another 16 occasions (of which 11 were related to major or minor SSW events at 10 hPa), and on one occasion there was a single pole-centred PV vortex.

The other key conclusion is that "there is merit in considering the dynamics of a jet-like barotropically unstable flow configuration comprising an annular band of enhanced absolute vorticity to help account for the character of the forementioned observed features." I agree with this statement, but this is not a new result. Again, as I clearly said in original review, this statement could be made based on results of previous studies that have looked at barotropic instability. I know "significant" is subjective, but I think "merit in considering" is a weak conclusion and as this is not a new result I don't think this is "a significant advance".

The reference here to the 'second assertion' being a key conclusion was responded to in our overarching remark.

The additional knowledge, utility and physical understanding accrued in our model analysis is set out in the manuscript at various locations in the various sub-sections of Section 3. See also remarks below.

Finally, the discussion of previous work is limited. In response to my comment on this in my original review the authors have responded with the large number of papers they cited, and then dismissed my suggestions. However, number of references is not a good indicator of discussion of previous work. For example, in the case of the barotropic instability the previous studies are "discussed" around line 795 after their analysis and the focus is on differences in the model set up. These previous studies should be discussed before and the key conclusions should be summarized. If the authors did this it would show that their 2nd assertion could be made with no new analysis. Even if they disagree this would show what is new (other than model set up).

In our response to a comment of the Co-Editor we stated that

"The utility of the present model is introduced at various locations in the September text and consolidated in Section 3.5. This sequential approach is appropriate because the measure of our model's explanatory power and utility, relative to that of earlier studies, can best be assessed after the evaluation of our results".

In our response to the Co-Editor we then provide a summary of the model's utility in line with the portions of text noted above.

In similar vain, I will again stress that the discussion of annular PV structure is incomplete. The paragraph around line 50 misses the Harvey study (whichever one they think is most relevant)

The relevant paper was already referenced in the September text. It is now referenced again at Line 64.

and has no mention of Mars even thought there has been many papers on this over last 5 or so year, that include analysis of data and idealized modeling.

Our study seeks to relate the characteristics of certain phenomena observed near the stratopause of planet earth to the results of our barotropic model. The dimensionless parameters selected for our basic states and that are key to the dynamics are chosen to be akin to those at the stratopause.

Request for discussion of Martian studies:

Many geophysical flow systems exhibit features relatable to barotropic instability. For example almost all the solar system's planets that possess an atmosphere (plus Titan) also have a polar vortex and some also have small-scale flow features embedded within that vortex. (We have referenced six such papers). Again there are smaller-scale features embedded within the low-level flow of both hurricanes and extratropical cyclones. (We have referenced two such studies).

However these various phenomena can (and generally do) differ in terms of the values of their key dimensionless parameters, and in our study the values adopted pertain to the Earth's stratopause. To note this difference and account for why we have not considered it necessary to describe studies related to these other phenomena, we have inserted an additional sentence into the text (near Line 86).

Response to Reviewer 2

Our responses are given in red below, and the specified line numbers refer to the "New Version" of the manuscript.

This is my second review of this manuscript.

I was asked by the editor to assess the comments of reviewer one and the response of the authors. I am sympathetic to the concerns raised by the reviewer: clearly developing a climatological analysis of these features would be valuable; there have been similar features and analyses carried out in other geophysical contexts (and to some extent of the features on which the present study focuses); and it would be helpful to have some clarity about how the present theory differs from previous theoretical work.

Nonetheless I am basically satisfied by the authors' responses to these concerns, insofar as I don't see them as a reason to reject the manuscript:

1) It is reasonable to try to identify the key dynamical features before proceeding with a climatological analysis, and the present work adds meaningful theoretical understanding to the results of DS24.

2) It is still of value to study features in Earth's stratosphere and to understand the relevant the parameter regime even if similar features have been identified in other geophysical flows.

We concur with all the above remarks and moreover, in our response to the comments of the Co-Editor and Reviewer 1, we have eliminated reference to the ‘prevalence’ of the features under study.

3) The authors have presented a review of existing literature, and have pointed to distinctions between the present work and other similar studies (although in my view it is often unclear to me what is really a substantial difference in analyses rather than a superficial detail, and I encourage the authors to emphasize the former and reduce the latter).
(i) With regard to the observational component, the present study first confirms that of DS24 and then extends it by the more detailed consideration of three disparate cases. The cases draw attention for example to the features’ depth, developoment on isentropic surfaces, presence during SSW events, and to some attendant aspects of the signals in the humidity and ozone fields.
(ii) with regard to the theoretical component we have referred to:-
the differing values of key dimenionless numbers that pertain to the occurrence of features in other geophysical and planetary settings (Lines 86-89), the similarity and more trenchantly the difference to other stratospheric barotropic instability studies (Section 3.5), and we believe the consideration of the non-normal mode growth allied to the non-linear simulations to be novel, new and potentially of relevance to the unfolding of SSW events at tropoause levels.

I've raised a few points where I see remaining inconsistencies or places where the revised text is still unclear that I would like the authors to revise, but once these are addressed I support publication.

I do remain unconvinced of some of the authors' claims, but the language is speculative enough that I don't object to any of them being published for the sake of raising discussion.
We have now sought to remove, reduce or explicitly acknowledge any speculative comments.

In particular the authors have not convinced me that the non-normal growth identified in (3.2.4) is anything different from transient wave propagation,
To at least attempt to reduce the Reviewer's lack of conviction, we would contend that the non-normal growth is present only if the basic state satisfies the necessary condition for instability, but can be present even if the corresponding normal modes are stable.
For our model, with uniform but different absolute vorticity in each domain, wave propagation amounts to the interaction beteween the waves on the two interfaces. For a stable basic state the expectation is that pertubation energy/enstrophy would be transferred from one wave to another, but our linear theory indicates (and the non-linear simulation goes some way to demonstrating) that the pertubations grow on both interfaces. This can be regarded as the counterpart of wave over-reflection for an unstable basic sate comprising a continous variation of the absloute vorticity.

and I stand by my skepticism of the relevance of Fig. 17c to the evolution of a split sudden warming, for the reasons I gave in my first review. I don't, however, see anything incorrect about the analysis so if the authors wish to leave that section as is I don't see it as disqualifying.
In our response to a comment of the Co-Editor, we show a figure that portrays the subsequent development of the pattern in Fig.17c over a comparatively short time-span. We find it difficult to refrain from at least comparing some aspects of the resulting pattern with the features of an incipient SSW at stratopause-levels. Nevertheless in the revised text we have weakened our comments and noted that the simplicity of the barotropic model.

Miscellaneous comments:

l60-71: My comment on this text in the first submission was "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."

Neither the response nor the additional text offered by the authors have reduced my confusion. What is the authors' intent in raising this distinction? What does it add to the discussion?

'Confined to and governed by the structure of the SPV' suggests perhaps that the former are meant to be discrete internal modes while the latter are meant to be a continuum of externally forced modes? However, the latter are still at least in part governed by the structure of the SPV, and the former are not necessarily confined to it. The examples don't help; smaller-scale synoptic variability on the vortex edge is probably not easily described in terms of normal modes of some kind of confined vortex, and all of the variability is subject to non-linearity and wave breaking. The fact that planetary-scale Rossby waves still show up in both categories continues to throw me off.
In the re-revised text we have sought to clarify the distinction between the two classes and

their relationship to the SPV's structure. We take the first class to refer to phenomena confined to the stratosphere and not directly linked to wave-forcing from below. (The steady 4-day zonally-propagating wave would fall into this class and it has been conceived as an internal mode). The second class is regarded as referring to upward propagating waves and, as you point out and we now note explicitly at this juncture, are also influenced by the SPV's structure.

l372-373 vs. l186-188: The earlier sentence is asserting that the static stability exhibits strong maxima colocated with the isolated PV features of interest, while the latter says that they do not exhibit thermal structure. Given the aspect ratio of these PV features one should expect the former to be present; and I would call static stability anomalies a thermal structure, so the latter conclusion is unjustified.
Line 373 should have (and now does) refer to the temperature pattern on a pressure surface located near the stratropause.
The dynamical linkage that then implies that the two sentences are mutually compatible derives from simple QG arguments. They indicate that a deep isolated PV feature would be characterized in the vertical by an accompanying strong static stability, whilst at the level of the feature's maximum PV amplitude there would be a comparatively small horizontal temperature anomaly.
The text (Lines 383-384) has been amended accordingly.

l680: The discussion around quasigeostrophy (QG) still seems unclear at best and potentially misleading. At various points the authors talk about the dynamics not being QG. Sometimes I think the authors are referring to the fact that the observed flow has elements that are inconsistent with QG scaling (e.g. relative vorticity of the same order as absolute vorticity), which is clear enough and unobjectionable. But in other contexts the discussion implies that the barotropic vorticity equation being used here is somehow more general than QG, despite it being a limiting form of the QG equations. The authors never spell this out but I get the sense that their argument is that since one can derive the barotropic vorticity equations through some other set of assumptions that aren't equivalent to QG, they are capturing some non-QG dynamics. This does not imply that the dynamics described by the model are not present in QG, and these implications should be removed.
We have amended/weakened the text at appropriate locations so that it now merely notes that:- (i) the relative vorticity of the features are of the same order as absolute vorticity and the magnitude of the divergence in the re-analysis fields is comparatively small and its pattern not ostensibly linked to that of the vorticity, and (ii) that these characteristics, whilst not justifying the adoption of a barotropic model, do not run counter to its adoption.

Fig. 11b is not consistent with the condition Z_III = nC < 0.
Figure has been reconfigured.

Fig. 13: The authors still have not quoted C or r_c/n for these curves. Please do so.
Values now inserted in the Figure legend.

'Ready Reckoner:' Fig. 15 is at least somewhat comprehensible now, but this whole section remains hard to follow, and I am not convinced that the method for determining the most unstable zonal wave number is any simpler than just explicitly calculating m*, especially given all of the caveats and the fact that both Fig. 14 and 15 assume b=0.5. It would really help if the approach were illustrated the particular parameter values explored in the non-linear calculations.

(i) To help clarify these two issues we have split sub-section 3.2.3 into two sub-sections. In the new sub-section 3.2.3 we have first set out its objective and contrasted that objective to the material in the previous sub-section, and secondly streamlined the remaining text to emphasize the main results. The new sub-section 3.2.4 can be viewed as providing a ready answer to the question: "What is the wavenumber of the most unstable mode?"

(ii) Selection of $b = 0.5$:

The parameter setting $b = 0.5$ was adopted as a typically realized value. In practice b would normally lie in the range, say, $0.3 < b < 0.60$ correspond to jets with locations from 18^0 to 36^0 from the pole. The value of Eqs (10) is that the changes to Figs. (14, 15) can be readily inferred (or calculated).