

Response to Reviewers' Comments

Ji-Hee Yoo and Hye-Yeong Chun

Department of Atmospheric Sciences, Yonsei University, Seoul, Korea

17 July, 2025

Dear Editor and Reviewers,

Thank you very much for your constructive comments and suggestions on this paper. We carefully revised the manuscript to address all comments and made every effort to improve its clarity and scientific rigor. In particular:

I. We discussed the potential dependency between two instability-driven in-situ PW2 growth mechanisms—nonlinear wave–wave interactions and over-reflection—and their differing temporal contributions to PW2 amplification.

II. We supplemented our analysis with two additional reanalysis datasets, to support the robustness of our results involving the mesosphere, where reanalysis reliability is limited due to sparse observation.

III. We incorporated a discussion of the findings from O'Neill et al. (2017), whose analysis of SSW02 underscores the importance of nonlinear PV advection and vortex–vortex interactions in vortex splitting, beyond the classical wave–mean flow framework adopted in this study.

We respond to each reviewer's comments in the following paragraphs. The original comments are shown in blue, and our responses are given in black.

We hope that the revised manuscript meets the journal's standards and will be considered for publication in *Atmospheric Chemistry and Physics*.

Sincerely,

Hye-Yeong Chun

Reviewer #1's Comments

General comment:

In this paper, the authors examine the role of in situ excited planetary waves (PWs) for the major sudden stratospheric warming in the Southern Hemisphere 2002. It demonstrates that westward propagating PWs with zonal wave number 2 (PW2) were amplified due to the barotropic-baroclinic instability in the stratosphere and split the polar vortex. The instability was formed by the breaking of zonal wave number 1 PWs and depositing westward momentum. Furthermore, the authors suggest that the over-reflection of upward propagating PW2 contributed to split the polar vortex.

The paper is well-written and is highly relevant to EGU sphere. While some minor revisions are needed, I recommend the paper for publication, pending a few minor revisions.

Thank you very much for carefully reviewing the manuscript and providing valuable comments on the manuscript. The comments and suggestions are very helpful, and we made best effort to address them during the revision process to improve the manuscript.

Minor Comments:

1. Sec 3.5, L288: ... can be traced back ...

It is hard to identify from Figure 7. Please specify more clearly.

Thank you for the helpful comment. As you pointed out, the upward development process described in the original sentence was not sufficiently clear based on Fig. 7 alone. To address this, we examine the geopotential height perturbations (GHP) at 100, 10, and 2 hPa (Fig. R1.1), which approximately correspond to the isentropic surfaces of Ertel's potential vorticity (EPV) shown in Fig. 7.

As shown in Fig. R1.1 below, at 100 hPa, a clear zonal wavenumber (ZWN) 2 pattern appears in GHP from 22 September. At 10 hPa, the primary cyclone becomes pinched into a peanut-like structure on 23 September, and by 24 September, it splits into two cyclones. At 2 hPa, these processes occur with a one-day delay, as the pinch forms on 24 September and the separation becomes distinct from 25 September onward, suggesting that the vortex split initiates in the lower stratosphere and subsequently develops upward. This progression is consistent with the vertical evolution of PW2 amplitude (Fig. 2b), where lower stratospheric enhancement precedes that of the upper stratosphere, and with the westward tilt of the phase of PW2 with height up to 3 hPa (Fig. 2c).

We included this description in the revised manuscript (L303–306), with Fig. R1.1 added to the supplementary material (Fig. S3) for further clarification.

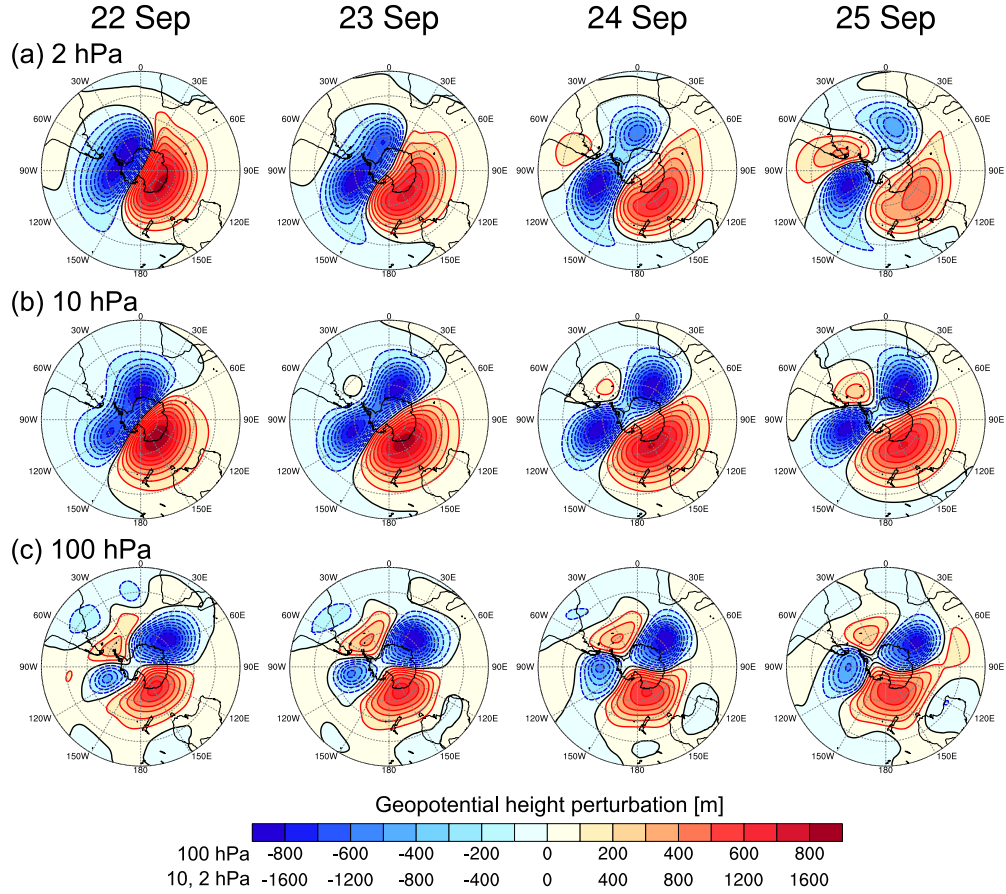


Figure R1.1. Polar stereographic series of GHP at (a) 2 hPa, (b) 10 hPa, and (c) 100 hPa from 22 to 25 September 2002.

2. Sec 3.5, from L286: Two paragraphs describe the possibility of over-reflection, which amplified the incident PW2 from troposphere. But main component of incident PW2 was EPW2, although amplified PW2 was mainly WPW2, as shown in Fig.4. It is OK? Please clarify.

Thank you for bringing this point to our attention. We originally thought that although incident PW2 was predominantly eastward-propagating ones (EPW2) at 100 hPa, the small portion of westward-propagating PW2 (WPW2) were amplified by instability as their critical levels reside in the instability region. However, during the revision process, we found that EPW2 hardly reach the upper stratosphere, as they encounter background winds increasingly unfavorable for upward propagation—where westerlies transition to easterlies with height (Fig. 3), according to the Charney and Drazin (1961) criterion $[0 < u - C_x < u_c = \beta / (k^2 + l^2 + \frac{f_0}{4H^2N^2})]$. In contrast, although their amplitude is relatively small, WPW2 can penetrate above the zero-wind line and reach the instability region, suggesting their contribution to the prevalent 1 hPa WPW2. However, WPW2 at 1 hPa is not a simple continuation of the WPW2 propagating upward from below but rather a product of instability-driven amplification. This is supported by a distinct positive EPFD at 1 hPa above the instability region, indicative of local wave amplification.

Although your comment is directed to Section 3.5, we think this clarification fits more naturally in Section 3.3 where the generation mechanisms are first discussed. We accordingly revised both sections to clarify this point (L211–222 in Sec 3.3 and L328–330 in Sec 3.5).

3. Sec 3.5, L320: Although ..., over-reflection appears to play an increasingly dominant role in amplifying PW2 as the onset approached.

What makes the authors say so? What is the evidence?

Thank you for bringing up this important point. While both mechanisms involve instability, the processes through which they operate differ somewhat. The nonlinear wave–wave interaction involves PW1 dissipation that generates and amplifies smaller-scale perturbations through instability, contributing to PW2 amplification. In case of over-reflection, unstable PW2 growth arises when incident PWs can tunnel through an evanescent layer and reach their critical level located within an unstable region.

The nonlinear wave–wave interaction process—evident in the development of easterlies resulting from PW1 dissipation (Fig. 6b), the associated onset of instability (Fig. 3b), and the emergence of an additional cyclone in the tailing region of the parent cyclone (Fig. 7a)—was already active by 22 September and persisted through 25 September. In contrast, as shown in Fig. 3a and b, over-reflection was unlikely until 23 September, as upward-propagating PW2 did not reach their critical levels within the unstable region, which remained confined to high latitudes (poleward of 60°S). From 24 September onward, however, the easterlies extended downward and equatorward, coinciding with intensified upward-propagating PW2—as indicated by the enhanced EP-flux—such that these waves reached the turning level at the instability lower boundary and over-reflection occurred (Fig. 8). This suggests that wave–wave interactions appear to dominate the early phase of wave generation, whereas over-reflection likely contributed to the later phase. Determining which mechanism played the dominant role requires a quantitative comparison, which remains beyond the scope of this study. Accordingly, we revised the relative sentences throughout the manuscript (L19–20, L343–352, L376–378).

4. Sec 4, L345: ... than tropospheric forcing

This study shows two mechanism as described in Abstract: 1) PW1 break made instability which amplified PW2; 2) over-reflection of PW2. The latter is also accompanied by stratospheric instability and positive EPFD. Is the latter classified as tropospheric forcing? These two mechanisms are completely independent phenomena? Please help readers to have clear images. Comments 3) and 4) are true for Abstract as well.

Thank you for bringing up this important point. As you noted, the over-reflection mechanism involves both upward-propagating tropospheric waves and stratospheric instability. Therefore, it is not appropriate to classify over-reflection strictly as either tropospheric or stratospheric forcing. As the original sentence in L352 is revised following Comment #36 by Reviewer 2, the expression that previously described over-reflection as being induced solely by stratospheric instability is deleted.

As explained in our response to your Comment 3, the two mechanisms operate through distinct pathways, though they share a common mediator—instability. However, our results suggest that they are not entirely independent. Specifically, the nonlinear wave–wave interaction initiated by PW1 breaking can induce stratospheric instability, which in turn provides favorable conditions for over-reflection if upward-propagating PW is present. Thus, while the two processes are dynamically distinct, the occurrence of one

may conditionally facilitate the other. To confirm our speculation, further investigation using dedicated modeling studies is required. Thus, we included our speculation in Section 4 (L371–376), but omit it from the Abstract.

WORDING

1. Eq.(4) , L101 : $X' \cos \phi$

It is corrected in the revised manuscript (L108).

2. Section 3.2, L143: above 3 hPa

It is modified in the revised manuscript (L148).

3. Figure3(c): the contour interval of EPFD (red contours) is different from 3(b) and Fig.8

Thank you for pointing this out. We modified the contour interval of EPFD in Fig. 3c to match that of Fig. 3b and Fig. 8 for consistency.

4. Section 3.3, L206: “aligning with the range of easterlies present in the instability region”

Show the contour interval or contour labels of u in Fig.3

Thank you for the helpful suggestion. Contour labels for the zonal-mean zonal wind are added to Fig. 3 for clarity.

5. Section 3.4, L236: An abrupt development of easterlies *were preceded by* ?

Thank you for pointing this out. The inaccurate expression is corrected in the revised manuscript (L248).

Reviewer #2's Comments

General Comments:

This is a well-written case study of the 2002 stratospheric sudden warming event in the southern hemisphere, referred to in the paper as SSW02. Given its extremely unusual nature, SSW02 has attracted attention in the literature for many years. The core contribution of the present study is to examine the role of in situ-excited planetary wave 2 in causing SSW02, as distinct from zonal wavenumber 2 waves propagating up from the troposphere.

I think this study merits prompt publication, albeit after what are likely to be major revisions. The single largest concern I have about the current version of the work is that the conclusions lean on the upper stratospheric and lower mesospheric fields of a single reanalysis product (MERRA-2). But how reliable are those, really? A recent study [1] of an NH SSW event found that MERRA-2 had problems above ~60 km and the only satellite instrument used by MERRA-2 above that altitude (Aura MLS) didn't launch until 2004. It's one thing to argue that the GCM underlying a reanalysis should be able to spread information from lower altitude upward, but the arguments made in the present paper involve apparent downward propagation of both waves and the zonal jet from higher altitudes. This would seem to create a greater need for high-altitude data.

Unfortunately I am not clear on how good the observing system for this part of the atmosphere was in September 2002. A recent whole-atmosphere reanalysis [2] starts in 2004 and used 6 satellite instruments, only one of which (TIMED SABER, not used in MERRA-2) was active in 2002. A technical report about the MERRA-2 observing system [3] warns of large biases in the lower mesosphere of the model, although it also suggests that the reanalysis state at those altitudes should be influenced by (if nothing else) channel 14 of the AMSU-A which was carried on three NOAA satellites active in 2002. In contrast, the ERA5 description paper [4] refers to the mesosphere as “observation-free” and warns of associated artifacts in the ERA5 product. However, ERA5 is also assimilating AMSU-A channel 14.

Within the framework of the present manuscript, I think the best way to address this issue is to repeat at least some of the major analyses using a different reanalysis product to see how robust the results are. It would be fine to skip the statistical significance calculations (about which more below), in which case the amount of new data that needs to be downloaded will not be large. It's not obvious to me which reanalysis should be used for the comparison—ERA5 is actually available in two versions (5 and 5.1) for the early 2000s for reasons laid out in [4] and an associated ECMWF technical report [5]. (Note that the technical report includes some figures showing SSW02 in several different ECMWF reanalyses.) Perhaps one of the Japanese reanalyses would be appropriate instead. If the two reanalyses agree, great! If not, I think the study is still worth publishing as a demonstration of the limits of existing reanalysis for studies of this event and as a prompt for future research.

Thank you for thoroughly reviewing our manuscript and offering insightful comments. Your suggestions were highly constructive and have guided us in improving the clarity and quality of the revised manuscript.

Your major comment raised a crucial question regarding the reliability of the reanalysis datasets in representing the upper stratosphere and lower mesosphere, where observational constraints are limited. We fully agree that this is a valid concern, especially since the key mechanisms in our study involve downward propagation of both zonal wind and planetary waves from that altitude. The JAWARA dataset (Koshin et al. 2025) would be the most suitable for this purpose, but it is only available from 2004. Therefore, to provide support the validity of our results, we supplemented our analysis using two additional reanalysis datasets: ERA5.1 (Simmons et al., 2020) and the Japanese Reanalysis for Three Quarters of a Century (JRA-3Q; Kosaka et al., 2024). ERA5.1 is provided on 137 hybrid sigma–pressure levels up to 0.01 hPa (~80 km), with a horizontal resolution of 0.25° and 1-hourly temporal resolution. JRA-3Q is provided on a Gaussian grid with a latitude resolution of approximately 0.56° and 43 vertical levels spanning from 1000 to 0.1 hPa, with 6-hourly resolution. All results are based on daily averages.

Figures R2.1 to R2.5 show intercomparisons conducted for the analysis in this study involving the lower mesosphere. While the zonal-mean fields and PW amplitude are broadly similar (Fig. R2.1–2.3), ERA5.1 with higher spatial resolution exhibits finer-scale structures in variables involving horizontal and vertical derivatives, such as wave fluxes and $\bar{q}_y$ (Fig. R2.4–2.5). In addition, as the upper limit of ERA5.1 extends to 0.01 hPa, unlike the other two reanalysis datasets limited to 0.1 hPa, wave fluxes remain strong even at 0.1 hPa (Fig. R2.6). However, the key features and conclusions of our study remain consistent across the datasets.

Although none of these three datasets assimilate TIMED SABER observations, and therefore cannot fully guarantee an accurate representation of the mesosphere, the overall agreement among them suggests that our results reflect actual phenomena at those altitudes not artifacts of any single dataset. Three representative figures from the comparison (Fig. R2.1, 2.3, and 2.4) are included in the supplementary material with data specification (Text S2 and Fig. S11–13) and the related discussion is provided in Section 4 (L409–420).

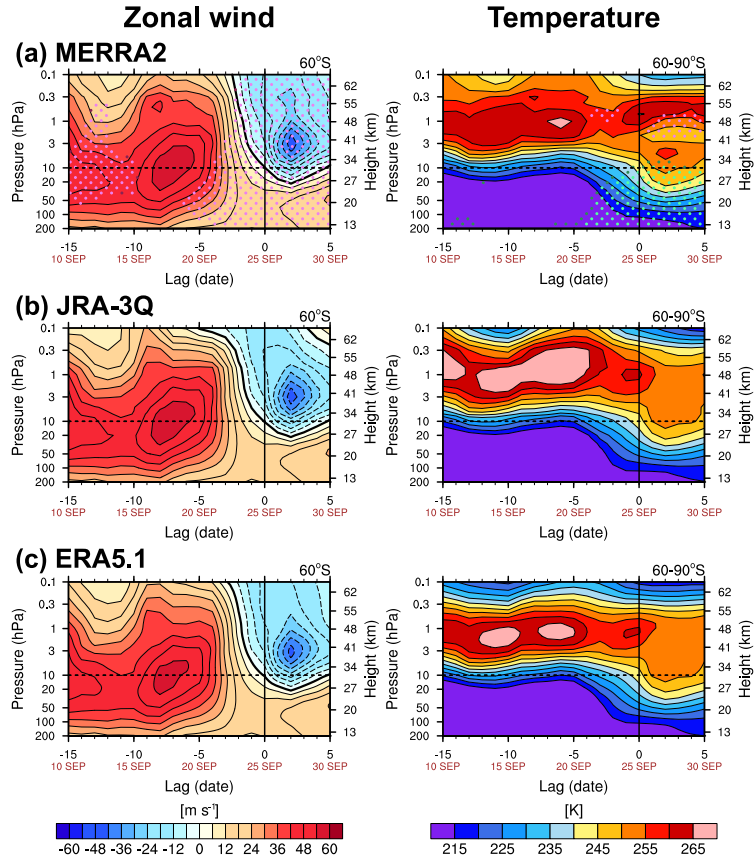


Figure R2.1. Time–height cross sections of the zonal-mean zonal wind at 60°S (left) and polar cap temperature averaged over 60–90°S (right) revealed in (a) MERRA-2, (b) JRA-3Q, and (c) ERA5.1.

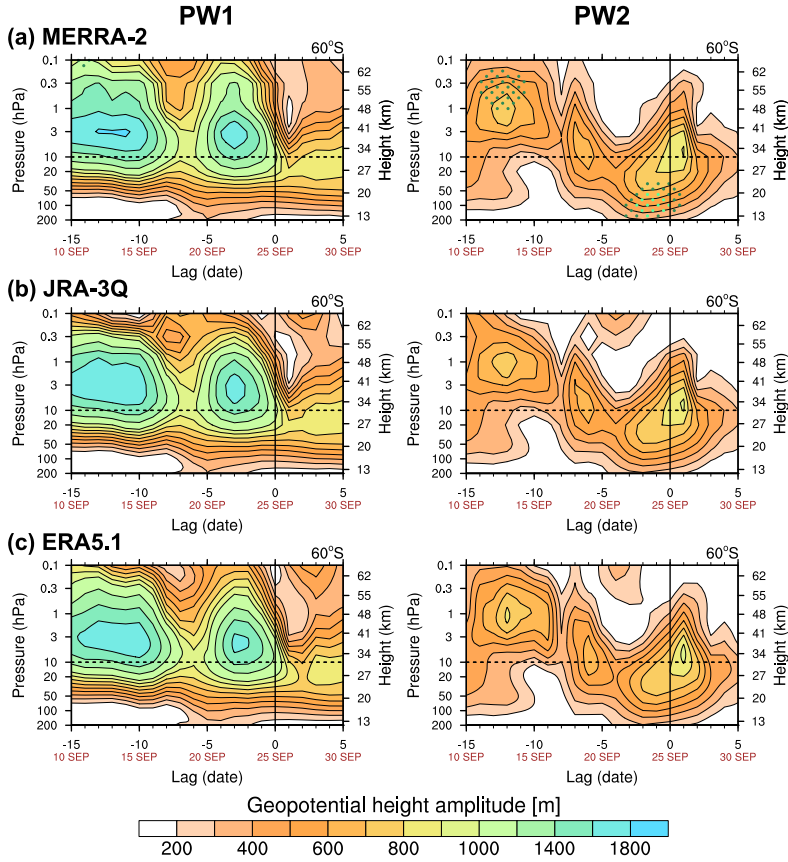


Figure R2.2. Time–height cross sections of GHP amplitude of PWs with ZWN1 (PW1, left) and 2 (PW2, right) at 60°S revealed in (a) MERRA-2, (b) JRA-3Q, and (c) ERA5.1.

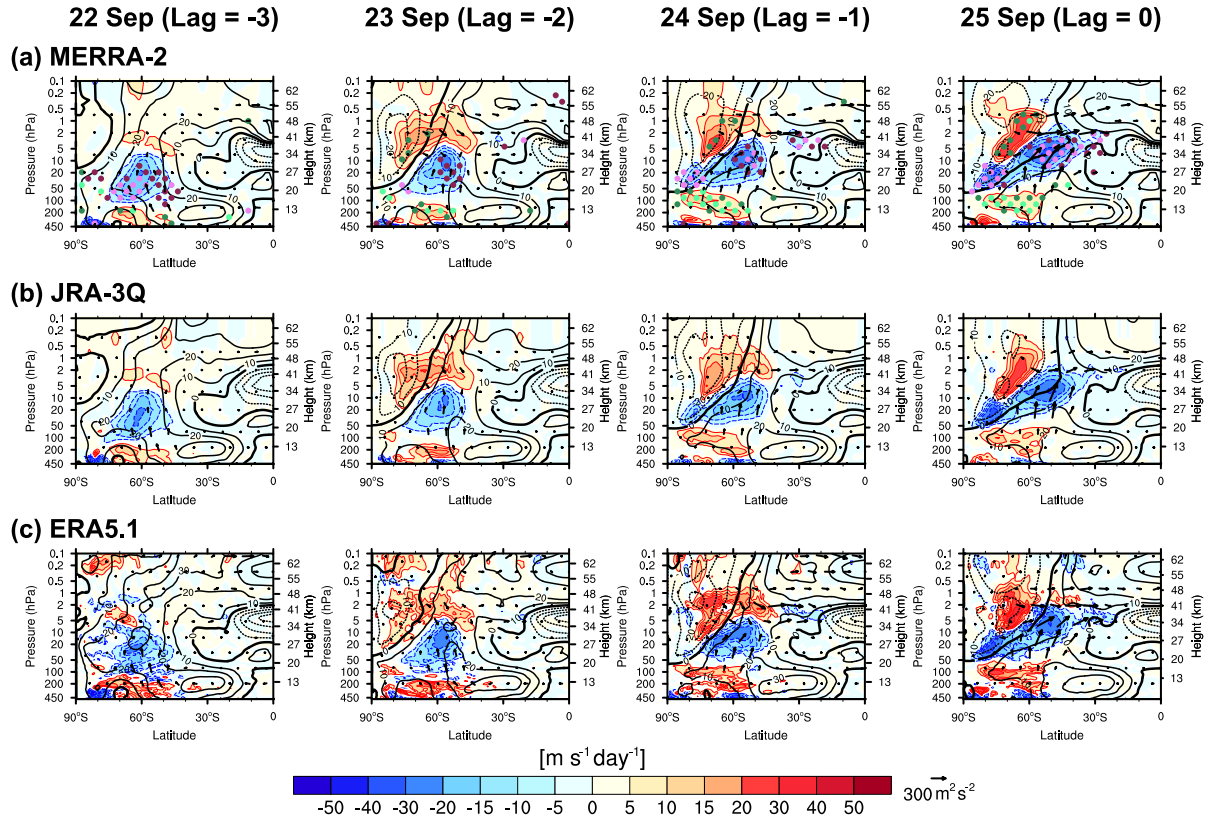


Figure R2.3. Latitude–height cross sections of the EP-flux (vectors) overlaid on EPFD (shading) for PW2 from 22 to 25 September 2002 revealed in (a) MERRA-2, (b) JRA-3Q, and (c) ERA5.1.

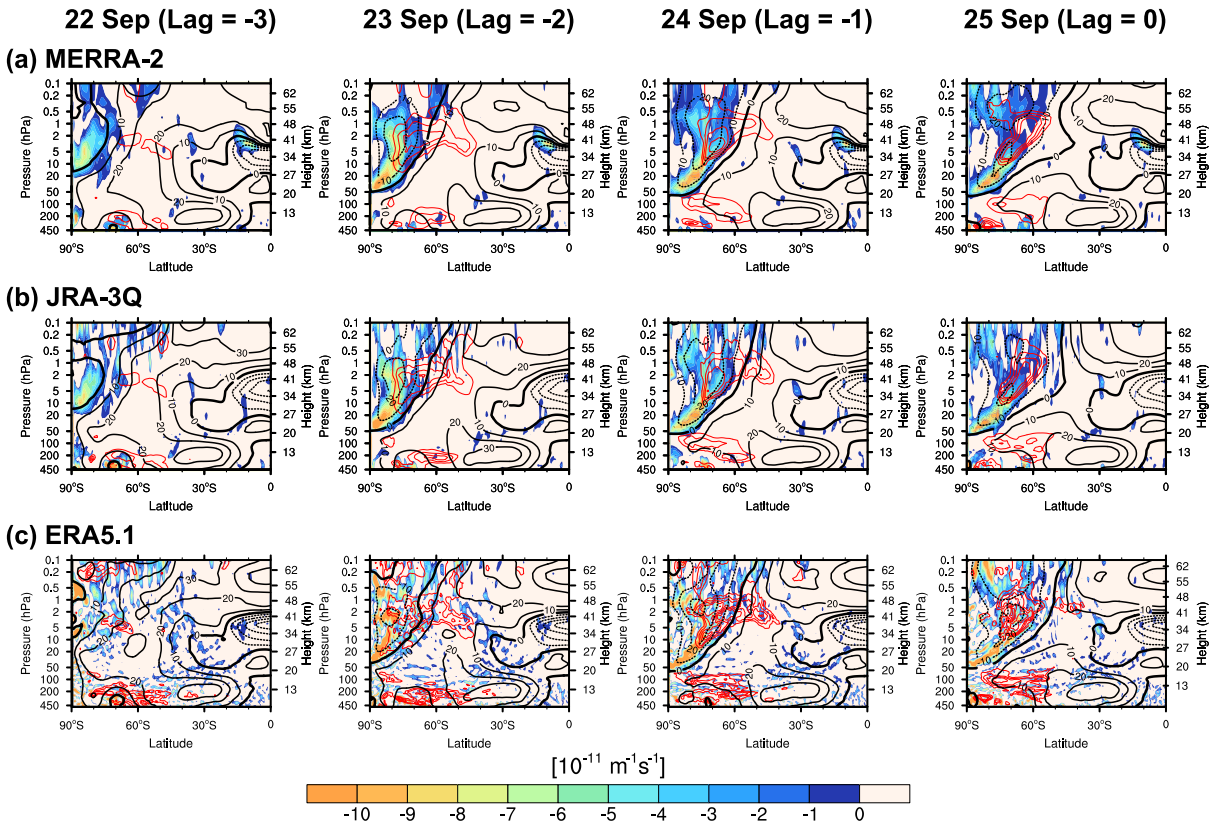
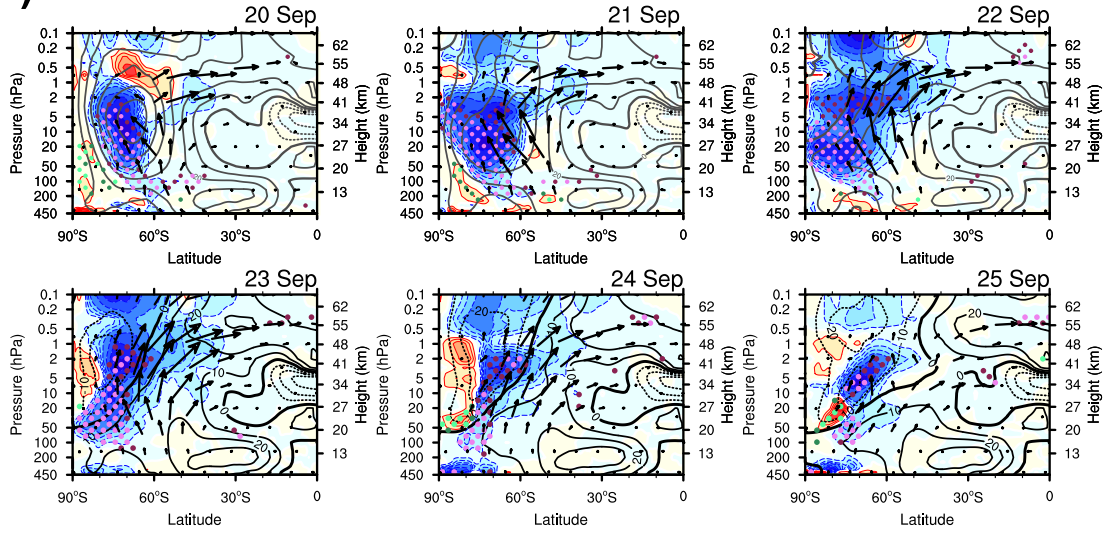
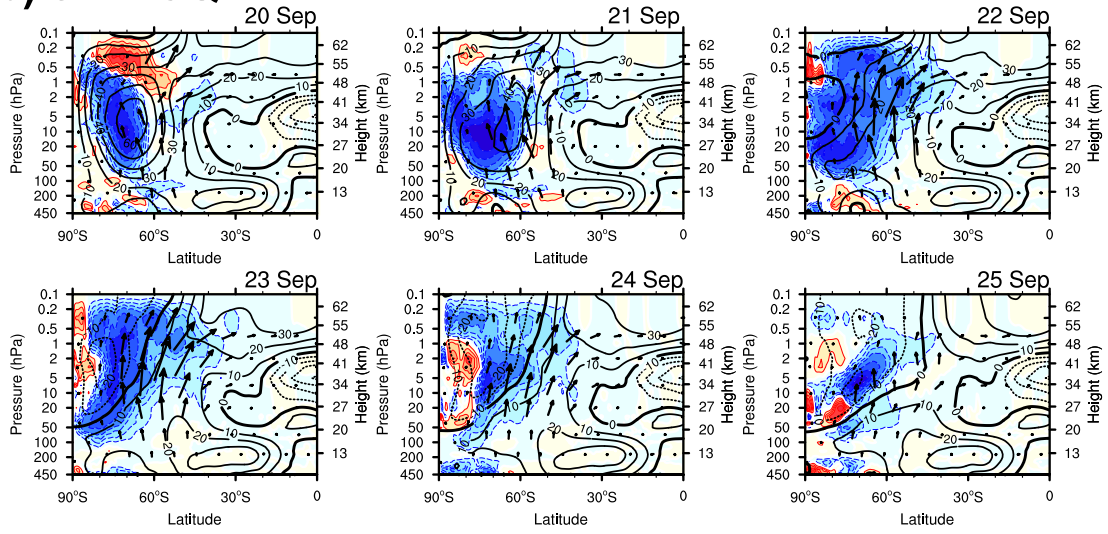


Figure R2.4. Negative $\bar{q}_y$ (colors) overlaid with the positive EPFD of PW2 (red contours) from 22 to 25 September 2002 revealed in (a) MERRA-2, (b) JRA-3Q, and (c) ERA5.1.

(a) MERRA-2



(b) JRA-3Q



(c) ERA5.1

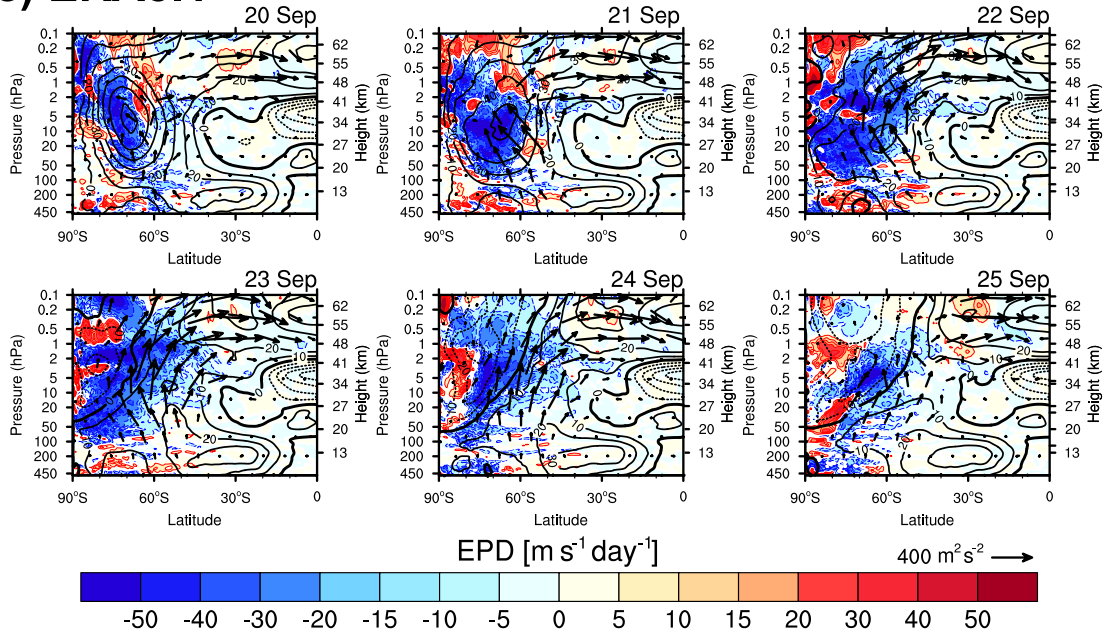


Figure R2.5. Latitude–height cross sections of EP-flux (vectors) overlaid on EPFD (shading) for PW1 from 20 to 25 September 2002 revealed in (a) MERRA-2, (b) JRA-3Q, and (c) ERA5.1.

Specific Comments By Line Number:

1. 24-25 One of your references (Newman and Nash 2005) attracted a (2014!) comment claiming that there was an SH SSW in 1972 and citing some related works—do those works need to be cited and/or your claim modified?

We have reviewed the (2014!) comment on Newman and Nash (2005). The studies cited there by Sehra (1975, 1976, 1979) reported a sharp stratospheric warming based on rocket soundings at Molodezhnaya Station (67°40'S, 45°51'E). However, in our manuscript, "major sudden stratospheric warming" (SSW) refers to events that satisfy the WMO/IQSY (1964) criterion, which requires both a significant polar stratospheric temperature increase and a reversal of the zonal-mean zonal wind from westerlies to easterlies at 60°N and 10 hPa, though the application of this definition to the Southern Hemisphere remains debated.

Since temperature observations from a single station are insufficient to confirm the occurrence of a major SSW, we examined this possibility for August–September 1972, when Sehra reported the warming, using JRA-3Q data. The zonal-mean zonal wind at 60°S and 10 hPa (Fig. R2.6) showed no wind reversal, indicating no major SSW occurred. Thus, we retain the original description in our manuscript.

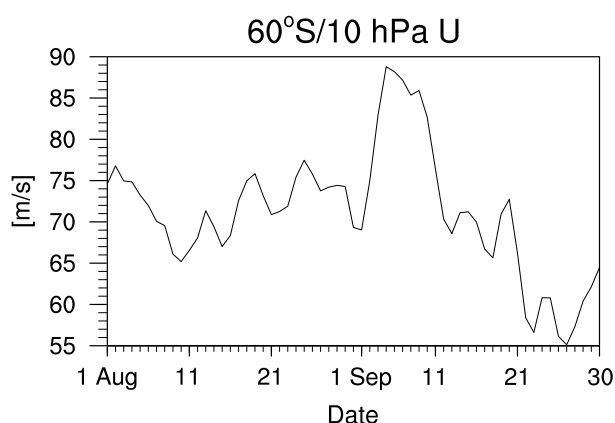


Figure R2.6. Time series of zonal-mean zonal wind at 60°S and 10 hPa during August–September 1972.

2. 26 “breakdown of the polar vortex during midwinter”→the event took place on 25 September, but to me at least SH winter means JJA. So I would classify the event as happening in spring.

We modified “midwinter” to “spring” as suggested (L30).

3. 27 I don’t quite understand what is meant by “sparse mountainous”.

The original expression was intended to convey the reduced mountainous land cover. We revised that to “less mountainous terrain” for clarity (L31).

4. 78 The MERRA-2 model top is actually at 0.01 hPa (not 0.1 hPa), although I gather you are using the pressure level data that ends at 0.1 hPa.

You are right. We modified the original sentence in the revised manuscript (L81) for clarity.

5. 78-79 Sentence reads as though MERRA-2 ends at the end of 2023, which is not correct. What you mean is that you're using the 44 years 1980-2023 to calculate the characteristic magnitude of variability used to contextualize the size of SSW02.

Thank you for the comment. We revised the original sentence to avoid potential misunderstanding (L83).

6. 84 Make sure to double-check this equation to make sure it has correct factors of $\cos(\text{latitude})$

Thank you for the comment. We carefully double-checked the EP-flux formulation, and confirmed that all factors of $\cos(\text{latitude})$ are consistent with the standard spherical-coordinate expression provided in Andrews et al. (1987, Eq. 3.5.3).

7. 94 This equation and subsequent discussions of BT-BC instability involve derivatives with respect to “ y ”—this would make sense for theoretical analyses on the QG beta plane, but you're analyzing real data on the sphere. You should either explain what you mean by “ y ” or just rewrite this equation, references to q_y , etc. in the physically correct spherical geometry.

Thank you for pointing this out. We rewrote Eq. (3) in the spherical geometry (L98). However, we retain the compact notation $\bar{q}_y$ throughout the manuscript, with the clarification that $y = a\phi$ (L102).

8. 96-97 This is an allusion to the way in which the flow is satisfying the Charney-Stern-Pedlosky criterion for instability, right? Maybe you should say that directly. It might also be interesting to point out that this isn't necessarily the most common way to satisfy the CSP criterion—see discussion in [6].

You are correct. The reversal of $\bar{q}_y$ sign corresponds to one of the Charney-Stern-Pedlosky (CSP) necessary conditions for instability, which requires that at least one of the following criteria be satisfied: (i) Q_y changes sign within the domain, (ii) Q_y has the opposite sign to U_z at the upper boundary, (iii) Q_y and U_z at the lower boundary have the same sign, or (iv) U_z has the same sign at both boundaries when $Q_y = 0$ (Vallis, 2017). Here, U and Q are the zonal-mean zonal wind and potential vorticity, respectively. We state this in the revised manuscript without detailing all four criteria.

In addition, we include a brief note that, under typical midlatitude conditions, instability criterion is normally satisfied through (iii), where both Q_y and U_z at the lower boundary are positive (L103–105).

9. 101 The $\cos(\text{latitude})$ superscript appears to be a typo.

It is corrected in the revised manuscript (L108).

10. 101-107 These equations use both f and f_0 —the generalization of QG theory to the sphere is not trivial, and (assuming these equations are all written correctly) you should clearly state what you have chosen for f_0 and (in my opinion) say a few words to remind readers about why you are using both f and f_0 .

Thank you for your careful observation. You are correct — the appearance of f_0 in the equations was a typo. We corrected the equations to consistently use f in these equations (L98, L108–109).

11. 115 Just for clarity maybe you should state that k is the nondimensional zonal wavenumber.

Thank you for pointing this out. We clarified that k refers to the nondimensional zonal wavenumber (L120).

12. 122 “the PNJ weakened dramatically by more than 100 m/s”—is the weakening really *that* large? I’m not seeing it in the figure.

Thank you for bringing this to our attention. Upon re-examination, we found that the actual weakening was approximately 57 m/s. We corrected this in the revised manuscript (L128).

13. 124 “statistically significant at the 99% confidence level”—in my opinion “statistical significance” is an odd concept to apply to a case study of a single event that is well-known to be both a) real and b) unusual. In this context there is no “null hypothesis” being tested, you’re just trying to quantify how extreme SSW02 was. I think you should drop the language here and elsewhere in the paper about “statistical significance” and “confidence intervals” and just state that the various colored dots represent <0.5th/<2.5th/>97.5th/>99.5th percentile as appropriate.

Thank you for the helpful suggestion. As you recommended, we eliminated phrases such as statistical significance and confidence level throughout the manuscript, replacing them with percentile-based description to quantify the extremity of SSW02 in an objective manner (L130–131, L142, L154–155, L165–166, L168, L255–256, L258–259).

14. 126 Are we supposed to be able to see the claimed upward-propagating signal in the figure? I also don’t understand how this claim is supposed to related to the downward propagation of the easterly wind from the mesosphere (or even if the claims are supposed to be related).

Thank you for the comment. We interpreted the upward extension of anomalously strong zonal wind deceleration (pink dots) and temperature increase (green dots) as an “upward-propagating signal,” based on their evolution in the time–height cross-section (Fig. 1). However, we agree that the link between this signal and the downward propagation of easterly winds from the mesosphere is unclear. To avoid potential confusion, we removed the sentence in the revised manuscript.

15. Figure 2a This figure might be easier to understand if you also had a supplementary figure showing the full geopotential height field at 10 hPa on these same dates, instead of just the GHP. Figure 7 does sort of do this, but for somewhat different levels and times.

Figure R2.7 presents the full geopotential height field at 10 hPa for the same dates as shown in Fig. 2a. Since the spatial patterns closely resemble those in Fig. 2a, we decided not to include this figure in the supplementary material.

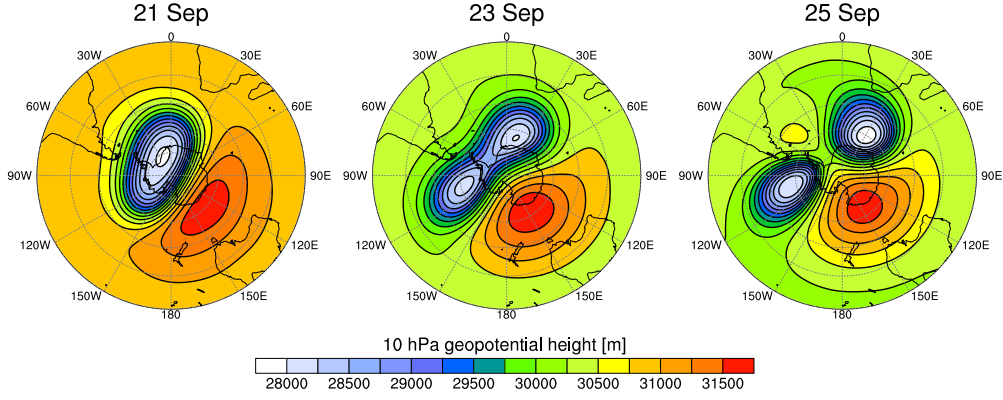


Figure R2.7. Polar stereographic series showing the geopotential height field at 10 hPa on 21, 23, and 25 September 2002.

16. Figure 3 This figure seems to be (in the preprint at least) a raster graphic. I think you should try to have this figure (and all others) published as vector graphics, to enable zooming in to view the many details.

Thank you for the suggestion. We converted all figures to vector graphic format in the revised manuscript.

17. 158-159 This sentence reads to me as claiming that there were easterlies in 70S-50S, <10 hpa on 22 September, which does not actually appear to be true.

Thank you for the helpful comment. As correctly noted by the reviewer, the easterlies did not yet occupy the region of positive EPFD on 22 September, but gradually descended and overlapped with it by 23 September. We revised the sentence accordingly in the manuscript (L164).

18. Figure 3a How is the orientation of the EP flux vectors selected in this and all other EP flux figures. This isn't a trivial issue [7].

Thank you for raising this point. The orientation and scaling of the EP-flux vectors follow the method described in Jucker (2020), using Eq. (18) and Table 2. We clarified this in the caption of Fig. 3 in the revised manuscript (L200–201).

19. 170-173 Seems plausible, but did you actually check plots of separate q_y subterms to confirm?

In response to the reviewer's question, we explicitly checked the individual subterms of $\bar{q}_y$:

$$\bar{q}_y = \frac{\partial \bar{q}}{a \partial \phi} = \frac{2\Omega \cos \phi}{a} - \frac{1}{a^2} \frac{\partial}{\partial \phi} \left[\frac{1}{\cos \phi} \frac{\partial (\bar{u} \cos \phi)}{\partial \phi} \right] - \frac{1}{\rho_0} \frac{\partial}{\partial z} \left(\rho_0 \frac{f^2}{N^2} \frac{\partial \bar{u}}{\partial z} \right). \quad (\text{R1})$$

24 Sep (Lag = -1)

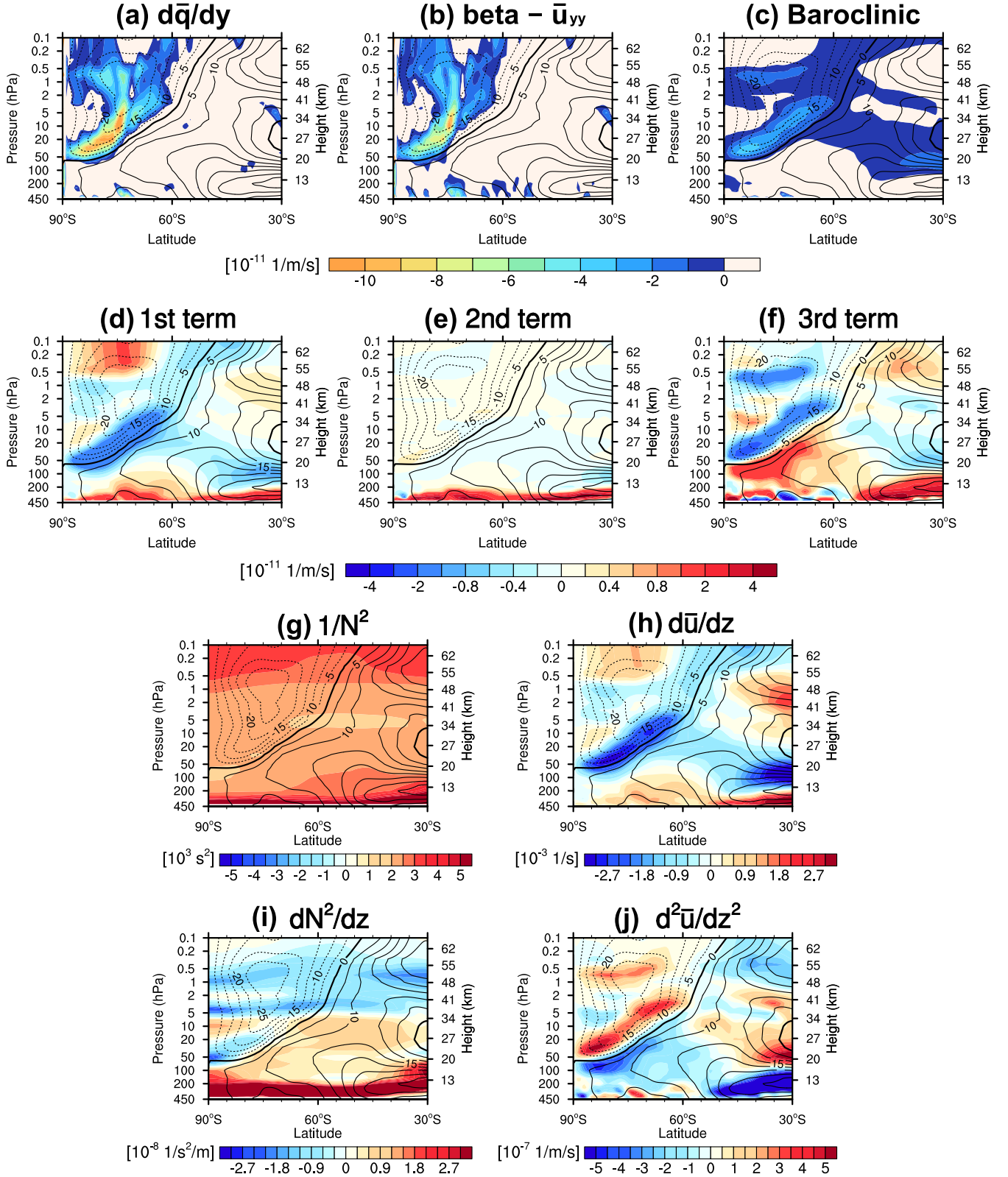


Figure R2.8. Latitude–height cross sections of (a–c) $\bar{q}_y$, the sum of first two terms (the meridional gradient of f and barotropic term), and the third term (baroclinic term) on the righthand side of Eq. (R1) and (d–f) the three terms on the right-hand side of Eq. (R2) divided by f^2 , (g–j) the inverse squared Brunt–Väisälä frequency $1/N^2$, the vertical shear of zonal wind $\bar{u}_z$, the vertical gradient of Brunt–Väisälä frequency dN^2/dz , and the vertical curvature of zonal wind $\bar{u}_{zz}$, on 24 September 2002. The black contours represent the zonal-mean zonal winds. The solid, dashed, and thick solid lines denote positive, negative, and zero wind, respectively.

Figure R2.8a–c present the latitude–height cross sections of $\bar{q}_y$, the sum of first two terms (the meridional gradient of f and barotropic term), and the third term (baroclinic term) on the righthand side of Eq. (R1) on 24 September—a representative case during the vortex destabilization period (22–25 September).

The strengthening easterlies induce a positive curvature of zonal wind (barotropic term) along their core, which dominates over the meridional gradient of f , leading to a negative sum of the two terms (Fig. R2.8b). Simultaneously, the baroclinic term becomes negative both below (50–5 hPa) and above (2–0.5 hPa) the easterly jet core in the polar stratosphere (60°–90°S, Fig. R2.8c).

To clarify the mechanisms driving the negative baroclinic term, we further expand it as follows (Yoo et al. 2023):

$$-\frac{1}{\rho_0} \left(\rho_0 \frac{f^2}{N^2} \bar{u}_z \right)_z = f^2 \left[\frac{1}{H} \frac{1}{N^2} \bar{u}_z + \frac{1}{N^4} \frac{dN^2}{dz} \bar{u}_z - \frac{1}{N^2} \bar{u}_{zz} \right]. \quad (\text{R2})$$

Figure R2.8d–f display the first, second, and third subterms of Eq. (R2), respectively, divided by f^2 . Both the first (Fig. R2.8d) and third terms (Fig. R2.8f) contribute to the negative baroclinic term within the developing easterlies over the polar stratosphere (Fig. R2.8c). Figure R2.8g–j present the distributions of four variables involved in the three subterms of Eq. (R2): the inverse squared Brunt–Väisälä frequency ($1/N^2$), the vertical shear of zonal wind ($\bar{u}_z$), the vertical gradient of Brunt–Väisälä frequency (dN^2/dz), and the vertical curvature of zonal wind ($\bar{u}_{zz}$), respectively. Under positive $1/N^2$ (Fig. R2.8g), the first subterm (Fig. R2.8d) becomes negative due to the easterlies descending from the lower mesosphere, which generate negative vertical shear ($\bar{u}_z$) along and below the jet core (Fig. R2.8h). The third subterm (Fig. R2.8f), showing distinct negative values above (2–0.5 hPa) and below (50–5 hPa) the easterly core, is attributed to strong positive vertical curvature ($\bar{u}_{zz}$) in these regions with positive $1/N^2$ (Fig. R2.8g, j).

In summary, the negative baroclinic term arises primarily from two processes: negative $\bar{u}_z$ beneath and near the easterly jet core, and enhanced positive $\bar{u}_{zz}$ both above and below the jet core. We included Figure R2.8 along with its detailed analysis in the supplementary material as Fig. S1 and Text S1, and are explicitly cited in the revised manuscript (L177–178).

20. 173 The overlap isn't exact, though—is it worth commenting on why that might be?

You are right—we changed the expression “largely” to “partially” (L178). As a possible explanation, we suggest that zonally localized instability could serve as a source of PW2 at lower latitudes (45–60°S), where positive EPFD appears without negative $\bar{q}_y$ (Fig. 3b in the original manuscript), as $\bar{q}_y$, derived from zonal-mean fields, may not capture such zonal asymmetries. Since this inference is based on the Ertel PV distribution in Fig. 7, we mentioned it after the description of Fig. 8 (L337–341).

21. 193-194 The caption should clarify that it's the zonal wavenumber 2 component of Z' that is being plotted in Figure 3c.

Thank you for pointing this out. We clarified in the revised manuscript that Fig. 3c shows the zonal wavenumber 2 component of Z' (L197).

22. Figure 4 The main text about and/or caption of this figure need much more discussion of how the phase speed spectra are computed—e.g., doesn't a phase speed spectrum need to be computed over some finite-width time window? What was that window and how was it selected, particularly given that you are attempting to interpret short-term temporal variations of the diagnosed phase speeds. If the analysis follows some previous paper sufficiently closely, you could just cite that paper instead.

In calculating the phase speed spectra of PW2, we followed Song et al. (2020) using an 11-day window. Prior to determining the window size, we computed the power spectral density (PSD) of the PW2 geopotential height amplitude at 1, 10, and 100 hPa over 45–75°S during September 2002 to identify the dominant periods (Fig. R2.9). The red dashed lines in this figure indicate the red-noise background based on a first-order Markov process.

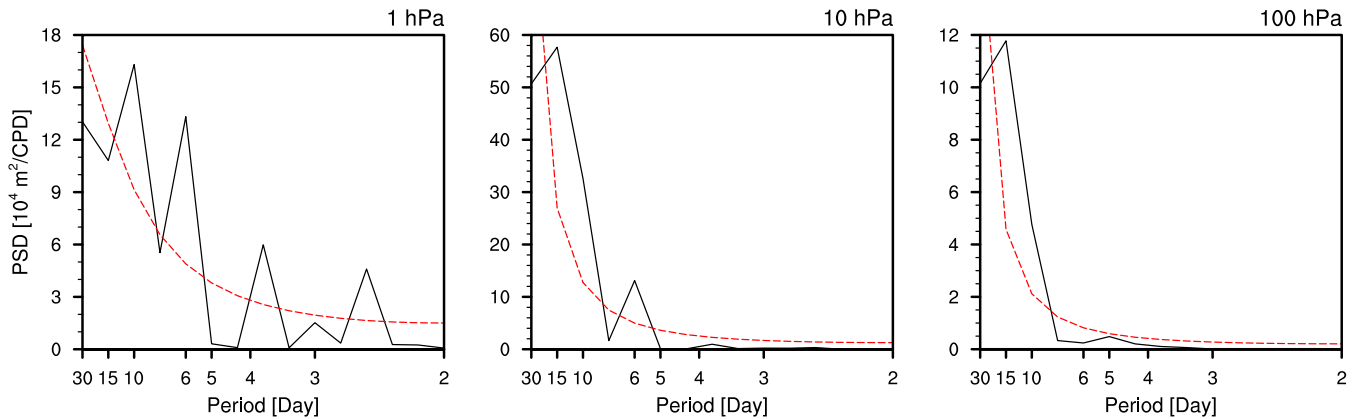


Figure R2.9. Power spectral density (PSD) of the GHP amplitude of PW2 during September 2002 at 1, 10, and 100 hPa.

At 100 hPa, a dominant peak is found at 15 days, while at 10 hPa, an additional peak is observed at 6 days. At 1 hPa, peaks are confined to periods shorter than 10 days, indicating enhanced high-frequency variability with increasing altitude. Although a longer window would better capture the dominant low-frequency, especially at 100 hPa, it may introduce uncertainties at higher altitudes due to the rapid changes in the mean fields during the evolution of the SSW02 event. Moreover, applying different window lengths at each level complicates direct vertical comparisons. Therefore, to balance the different temporal variability while ensuring consistency across vertical levels, we adopt an 11-day window as in Song et al. (2020), with sine and cosine tapering applied to the first and last 3 days, respectively, to reduce edge effects. The reference is provided in the caption of Fig. 4 in the revised manuscript (L205–207).

Meanwhile, in the process of revisiting this analysis, we found a minor error in the application of the time window. We corrected the calculation accordingly and updated Fig. 4 with the properly computed phase speed spectra of PW2. We confirm that this correction does not alter the central result.

23. 215 What “decreasing trend at 100 hPa”? (I don’t see it in the figure.)

Thank you for the comment. The “decreasing trend at 100 hPa” refers to the reduction in the westward phase speed range from approximately 40 m/s on 21 September to around 20 m/s on 25 September.

However, the dominant WPW2 at 100 hPa with amplitudes exceeding 100 m exhibit an increasing trend with time, consistent with the patterns observed at 1 and 10 hPa. Accordingly, we removed the sentence.

24. 222 How sure are you about the 21 September date, given the finite width of the contour interval? (In other words, might the jet structure you describe have been visible on 20 September if the upper left panel of Figure 5 were redrawn with a lot more contours?)

Figure R2.10 presents the zonal-mean zonal wind from 1 to 20 September, using a finer contour interval of 5 m/s. As the reviewer pointed out, the double-westerly jet-like configuration was already present prior to 21 September. Accordingly, we revised the corresponding sentence in the manuscript (L234–235).

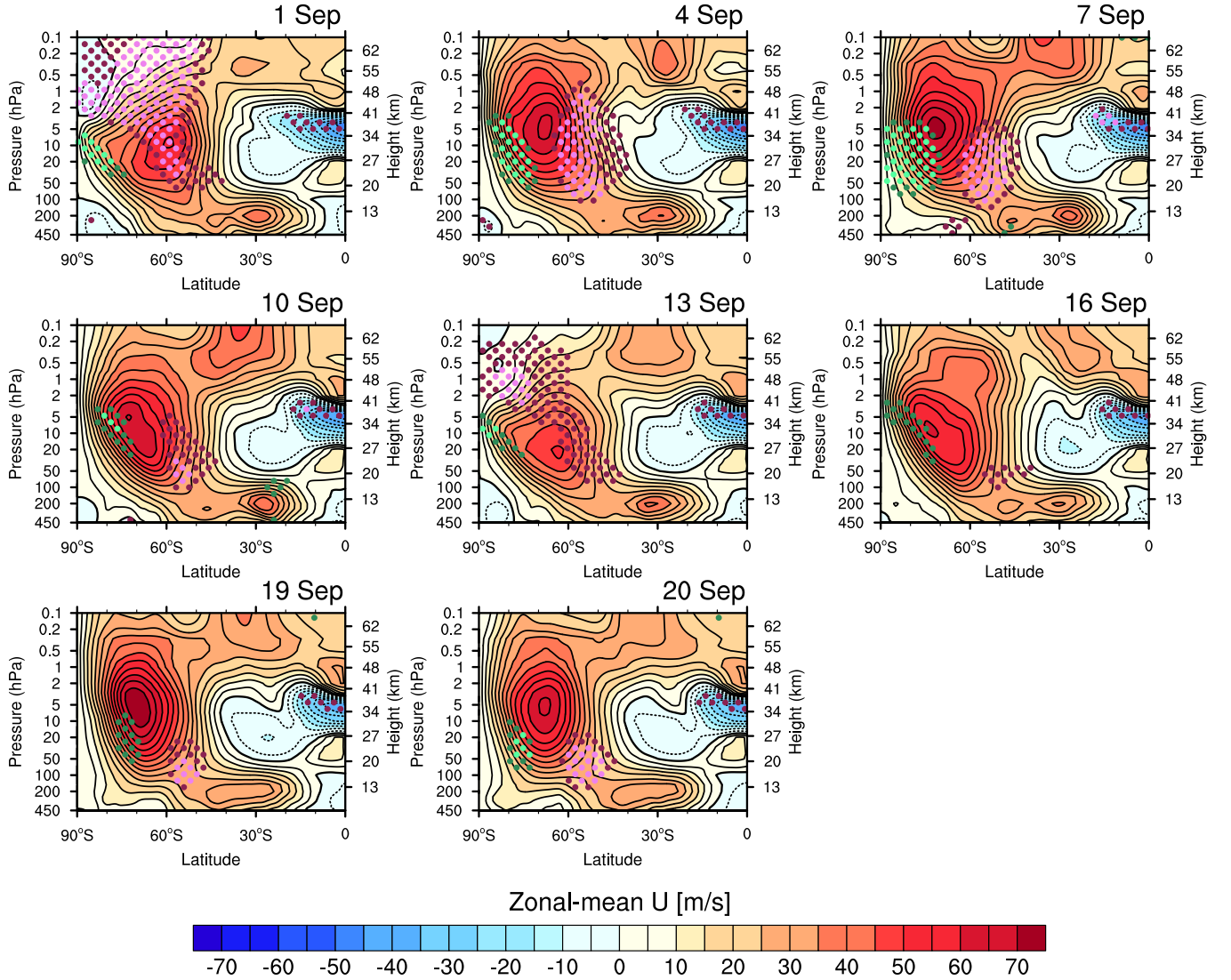


Figure R2.10. Latitude–height cross sections of zonal-mean zonal wind in the SH on selected dates between 1 and 20 September 2002, chosen to represent key stages in the evolution of the flow.

25. 226 Technically there was a very small region of easterlies on 21 September.

Thank you for pointing this out. We revised the date of the initial easterly appearance to 21 September (L238).

26. 242 I don't really understand what is meant by "negative PW forcings". Also maybe you should include the figure(s) you refer to as "not shown" in the supplementary material?

Thank you for the helpful comment. The term negative PW forcing refers to wave forcing necessary for reversing westerlies into easterlies. To avoid confusion, we modified the expression to explicitly describe its role in reversing the westerlies. Additionally, the comparison between total PW EPFD and PW1 EPFD, which was previously mentioned as "*not shown*," is now included in the supplementary material (Fig. S2) and cited accordingly in the revised manuscript (L255).

27. 246-247 I guess near the jet core the EPFD is at or below the 2.5th percentile value, but aren't more of the values actually below the 0.5th percentile?

Thank you for pointing out this. As you correctly noted, a substantial portion of the negative EPFD values falls below the 0.5th percentile near the jet core. We revised the corresponding sentence in the revised manuscript (L256).

28. 278-279 If the "additional cyclone" is what I think it is, might not its location be better described as 90E-180E?

Thank you for the comment. We revised the location description to 90°E–180° accordingly (L292).

29. 288-292 Are you sure about these claims? (I have a hard time seeing them in the figure.)

Thank you for your comment. This issue was also raised by Reviewer 1 (Comment #1). Please refer to our response there.

30. Figure 8 The figure panels contain a bunch of zeros that don't seem to be within (plotted) contour lines. What are they for? Maybe you should plot the associated zero contours?

Thank you for the helpful comment. The zeros correspond to zero EPFD. To avoid any confusion, we removed all line labels associated with EPFD and retained line labels only for the zonal-mean zonal wind in this figure. In addition, we added a clarification that the contour intervals for EPFD follow the color bar in Fig. 3a (L287).

31. 306 Clarify that "increases" means "increases towards zero", assuming that is what is meant.

"Increases" refers to a rise of C_x toward higher amplitude values. We modified the expression accordingly to avoid confusion (L324).

32. 310-311 Are we supposed to be able to see EP fluxes going upwards at the bottom of the $n^2 < 0$ region?

If an upward-propagating wave reaches the bottom of the evanescent region where $n^2 < 0$ and undergoes over-reflection, the reflected wave carries more energy than the incident wave. In such a case, the superposition of the upward and downward propagating wave components near that location results in a net downward EP-flux. That is why downward propagating waves are more dominant at the bottom of the $n^2 < 0$ region in Fig. 8.

33. 318-319 You're arguing that the possibility of instability is sensitive to the zonally resolved—not just zonal mean—qy, right? That doesn't necessarily seem impossible, but if you're thinking about the instability of zonal wavenumber 2 doesn't this wave have to "feel" the PV gradient over a pretty zonally extended region (perhaps even all longitudes) precisely because its wavelength is so long?

Thank you for this insightful comment. We agree ZWN 2 waves are likely amplified by instability acting over a broad longitudinal extent, given their large horizontal scale. As an attempt to explore this possibility, we examined the meridional gradient of EPV at 1500 K isentropic surface (shown in Fig. 7a) from 23 to 24 September (Fig. R2.11), when positive EPFD extends equatorward to 45°S beyond the region of negative $\bar{q}_y$ in Fig. 3b. In 45–75°S, the meridional gradient of EPV were negative in multiple longitudinal bands—broad sectors over the South Pacific and Atlantic, and smaller patches elsewhere—indicating spatially extensive but zonally intermittent instability-prone areas. Although not zonally continuous, these regions span a considerable longitudinal extent, suggesting that such instability could project onto the planetary-scale wave. Nevertheless, confirming whether such zonally varying instability can indeed amplify PW2 would require further investigation using idealized modeling. We revise the original expression (L337–341) and include Fig. R2.11 as Fig. S4 in the supplementary material.

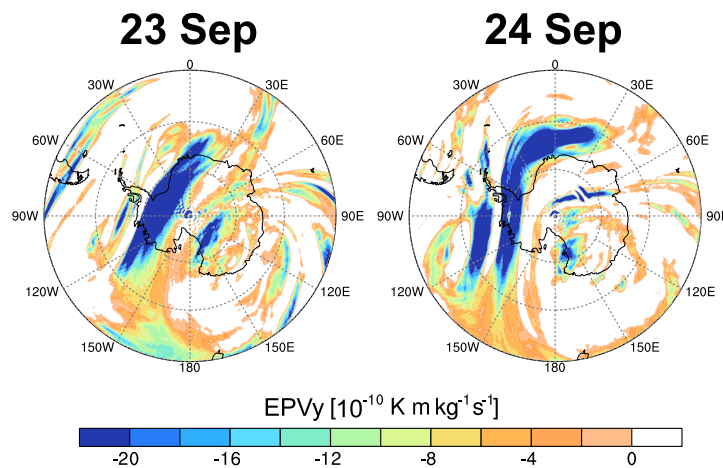


Figure R2.11. Meridional gradient of EPV at the 1500 K isentropic surface in 23–24 September 2002.

34. 320-322 Sorry, I don't understand what is substantiating this claim. (Maybe I just don't understand over-reflection well enough?)

Thank you for your comment. This question was also raised by Reviewer 1 (Comment #3). Please refer to our response there.

35. Figure S1/Table S1 Thank you for including this information, but I think it's sort of weird to refer to 6 pages of plots as a single "figure". I think they should just be renumbered as 6 different figures.

Thank you for the suggestion. As recommended by the reviewer, we renumber Fig. S1 as six separate figures (Fig. S5–S10, L389).

36. 352-354 Can you explain a bit more about how you are concluding that "in situ PW2 generation via instability may have played a more dominant role in approximately half of vortex-splitting SSW events than tropospheric wave forcing"?

Thank you for your insightful comment. Based on this study and our previous work (Yoo et al. 2023), we evaluated the occurrence of instability-induced PW2 across wave-2 vortex splitting events by establishing three indicative criteria—(1) PW1 dissipation (negative EPFD), (2) the emergence of instability (negative $\bar{q}_y$), and (3) PW2 generation (positive EPFD). However, more detailed case-by-case analyses are needed to assess the relative contribution of each process. Accordingly, we revised the relevant sentence and softened expressions elsewhere in the manuscript that previously described the role of this mechanism as being significant (L390–393).

37. 356-358, 367-369 Maybe you should add a line to your abstract saying that you discuss the implications of your (SSW02-specific) results for our understanding of SSW dynamics in general.

Thank you for the helpful suggestion. We revised the abstract to include the broader implications of our SSW02-specific findings for the general understanding of SSW dynamics (L20–22).

38. 364 What exactly do you mean by "not anomalous"? Surely the easterlies weren't exactly at their climatological mean values?

The term "not anomalous" in this context means that the easterlies did not qualify as a statistical anomaly. However, we agree that this expression is not essential in the current context and could lead to confusion. Therefore, we removed the phrase in the revised manuscript.

39. 369-370 I imagine this will be tough to do with reanalysis, given the sparse and time-dependent nature of the observing system at some of the relevant altitudes. If this really is a forced response to climate change, hopefully it can be modeled robustly.

Thank you for your constructive comment. We reflected your suggestion in the revised manuscript (L406).

Additional Thoughts on Future Work

Regarding the long-term possibility of (much more challenging) additional work, unfortunately there doesn't seem to be much satellite data not already included in MERRA-2 or ERA5. However MIPAS on Envisat and SMR on Odin may have usable data—indeed, a MIPAS-based study of SSW02 was published nearly two

decades ago [8]. (As explained in [2], the aforementioned SABER switches between observing 53S-83N and 83S-53N every 60 days and a quick look at the SABER data shows that a 53S-83N observing period began on 19 September 2002.) I am not sure if anyone has tried assimilating MIPAS or SMR data for this event.

Thank you for the thoughtful suggestion. We agree that further investigation of SSW02 using reanalyses incorporating satellite data such as MIPAS or SMR may offer valuable additional insights. Although this is beyond the scope of the present study, it could be a valuable direction for future research.

References

- [1] Liu et al. 2025, Dynamical Response of the Middle and Upper Atmosphere to the February 2018 Sudden Stratospheric Warming Revealed by MERRA-2 and SABER, JGR-Space Physics, <https://doi.org/10.1029/2024JA033528>
- [2] Koshin et al. 2025, The JAGUAR-DAS whole neutral atmosphere reanalysis: JAWARA, Prog. Earth Pla. Sci., <https://doi.org/10.1186/s40645-024-00674-3>
- [3] McCarty et al. 2016, MERRA-2 Input Observations: Summary and Assessment, GMAO Tech. Report, <https://ntrs.nasa.gov/citations/20160014544>
- [4] Hersbach et al. 2020, The ERA5 global reanalysis, QJRM, <https://doi.org/10.1002/qj.3803>
- [5] Simmons et al. 2020, Global stratospheric temperature bias and other stratospheric aspects of ERA5 and ERA5.1, ECMWF Tech. Report, <https://doi.org/10.21957/rcxqfmg0>
- [6] Vallis 2006, Atmospheric and Oceanic Fluid Dynamics: Fundamentals and Large-scale Circulation, Cambridge University Press, p. 265/equation 6.74, available via Google Books
- [7] Jucker 2021, Scaling of Eliassen-Palm flux vectors, Atmos. Sci. Lettrs., <https://doi.org/10.1002/asl.1020>
- [8] Wang et al. 2005, Longitudinal variations of temperature and ozone profiles observed by MIPAS during the Antarctic stratosphere sudden warming of 2002, JGR-Atmospheres, <https://doi.org/10.1029/2004JD005749>

Reviewer #3's Comments

General comment:

Review of “Role of in situ-excited planetary waves in polar vortex splitting during the 2002 Southern Hemisphere sudden stratospheric warming event”, authored by Ji-Hee Yoo and Hye-Yeong Chun.

This manuscript presents a study on the dynamics of the only major sudden warming that has been recorded on the Southern Hemisphere, using the reanalysis MERRA2. The authors thoroughly analyze the 5-day period before the central date of the event in terms of (linear) wave-mean flow interaction. They conclude that the breaking of planetary-scale wavenumber 1 in the polar stratosphere destabilized the flow and contributed to the generation of smaller-scale, wave-2 wave activity.

The figures are clear and the paper is well-written, and is relevant to ACP. I just have one concern that would only require adding a little bit of extra discussion.

This concern has to do with the applicability of linear wave propagation and wave-mean flow interaction theory to understand the dynamics of a highly distorted vortex as it is the case. I basically refer to the ideas and results by O’Neil and Pope (1988), who argued that the separation of the flow into a slowly-evolving, zonally symmetric component and a zonal harmonics might be overreaching during the final stages of the development of an SSW. There is a clear example in Figs. 6 and 7. Lines 243-244 argue that there is wave-1 focusing on high latitudes on 20-21 Sep “guided by the poleward-displaced vortex”. The authors refer to Fig 6a, where we see that the zonal-mean zonal wind on Sep 20 is poleward of 60°S and vertically aligned. However, the PV maps of Fig. 7 (or Z maps of Fig. 2) do not show a “poleward-displaced vortex”, but a cyclonic vortex severely displaced off the pole and elongated. O’Neill and Pope make the case that in such situations, non-linear PV advection and vortex-vortex interactions by inspection of Ertel’s PV maps might be a better suited framework to interpret the dynamics.

In this framework, it would be interesting to discuss the results of O’Neill et al (2017) on the same SSW in the Southern Hemisphere. They showed that the vortex split happened due to the interaction of a synoptic-scale cyclonic circulation in the upper troposphere barotropically aligned with one of the stratospheric vortex tips, and argued that the Eliassen-Palm fluxes cannot unequivocally be interpreted as indicating wave propagation (in this specific context of high non-zonal flows), since it is a non-local (zonally averaged) diagnostic.

We sincerely appreciate your careful review and thoughtful feedback. The comments provided were extremely helpful, and we have done our best to reflect them in the revised version to enhance the overall manuscript. In particular, in response to your major comment, we incorporated a discussion of the findings and implications of O’Neill et al. (2017) into the revised manuscript (L422–433).

Other comments:

1. Line 121. The reversal of zonal-mean winds (..) propagated downward → progressed down to 10 hPa

It is modified as suggested (L127).

2. Line 122. It is not apparent from visual inspection of Fig. 1 that the winds decelerated 100 m/s in one week. Please check.

Thank you for your comment. This question was also raised by Reviewer 2 (Comment #12). Upon re-examination, we found that the actual weakening was approximately 57 m/s. We corrected this in the revised manuscript (L128).

3. Line 125-126. What do the authors mean by “an upward-propagating signal from the troposphere”? The troposphere is not shown in Fig. 1, with the lower boundary at 200 hPa (i.e. lowermost stratosphere at high latitude). Besides, the polar warming seems to be confined above 100 hPa or so.

Thank you for your comment. This question was also raised by Reviewer 2 (Comment #14). We interpreted the upward extension of anomalously strong zonal wind deceleration (pink dots) and temperature increase (green dots) as an “upward-propagating signal,” based on their evolution in the time–height cross-section (Fig. 1). However, we agree that the link between this signal and the downward propagation of easterly winds from the mesosphere is unclear. To avoid potential confusion, we removed the sentence in the revised manuscript.

4. Lines 204-216. I find this description of Fig. 4 not clear enough. For example: “During (...) 22–25 September, PW2 at 1 hPa predominantly exhibited westward phase speeds of up to 30 m/s”. I guess a westward phase of 30 m/s refers to -30 m/s in the Fig. However, the amplitude of PW2 with -30 m/s on 22–25 Sep is quite small, it is larger at lower phase speeds.

The original sentence was intended to indicate that PW2 at 1 hPa predominantly exhibited westward phase speeds rather than eastward ones, and that the phase speed range extended up to -30 m/s. However, as you pointed out, PW amplitudes are relatively larger at lower westward phase speeds, particularly between -25 m/s and -5 m/s. Therefore, we modified the original expression to reflect the most dominant phase speed range rather than the upper bound (L213–214).

References:

O'Neill, A. and Pope, V.D. (1988), Simulations of linear and nonlinear disturbances in the stratosphere. *Q.J.R. Meteorol. Soc.*, 114: 1063-1110. <https://doi.org/10.1002/qj.49711448210>

O'Neill, A., Oatley, C.L., Charlton-Perez, A.J., Mitchell, D.M. and Jung, T. (2017), Vortex splitting on a planetary scale in the stratosphere by cyclogenesis on a subplanetary scale in the troposphere. *Q.J.R. Meteorol. Soc.*, 143: 691-705. <https://doi.org/10.1002/qj.2957>.

References

- Andrews, D. G., Holton, J. R., and Leovy, C. B.: Middle Atmosphere Dynamics. Academic Press, San Diego, CA, 489 pp., ISBN 9780120585762, 1987.
- Charney, J. G. and Drazin, P. G.: Propagation of planetary-scale disturbances from the lower into the upper atmosphere, *J. Geophys. Res.*, 66, 83–109, <https://doi.org/10.1029/JZ066i001p00083>, 1961.
- Jucker, M.: Scaling of Eliassen–Palm flux vectors, *Atmos. Sci. Lett.*, 22, e1020, <https://doi.org/10.1002/asl.1020>, 2021.
- Kosaka, Y., Kobayashi, S., Harada, Y., Kobayashi, C., Naoe, H., Yoshimoto, K., and Onogi, K.: The JRA-3Q reanalysis, *J. Meteorol. Soc. Jpn. Ser. II*, 102, 49–109, <https://doi.org/10.2151/jmsj.2024-004>, 2024.
- Koshin, H., Yamazaki, Y. H., Tomikawa, Y., Sato, K., and Miyoshi, Y.: The JAGUAR-DAS whole neutral atmosphere reanalysis: JAWARA, *Prog. Earth Planet. Sci.*, 12, 26, <https://doi.org/10.1186/s40645-024-00674-3>, 2025.
- O'Neill, A., Oatley, C. L., Charlton-Perez, A. J., Mitchell, D. M., and Jung, T.: Vortex splitting on a planetary scale in the stratosphere by cyclogenesis on a subplanetary scale in the troposphere, *Q. J. R. Meteorol. Soc.*, 143, 691–705, <https://doi.org/10.1002/qj.2957>, 2017.
- Sehra, P. S.: Upper atmospheric thermal structure in Antarctica, *Nature*, 254, 401–404, <https://doi.org/10.1038/254401a0>, 1975.
- Sehra, P. S.: Antarctic atmosphere: Temperature exploration and seasonal variations, *J. Geophys. Res.*, 81, 3715–3737, <https://doi.org/10.1029/JC081i021p03715>, 1976.
- Sehra, P. S.: Stratospheric circulation over Antarctica, *J. Meteor. Soc. Jpn.*, 54, 332–340, 1979.
- Simmons, A., Hersbach, H., Dee, D., and Berrisford, P.: Global stratospheric temperature bias and other stratospheric aspects of ERA5 and ERA5.1, ECMWF Tech. Rep., <https://doi.org/10.21957/rcxqfmg0>, 2020.
- Song, B. G., Chun, H. Y., and Song, I. S.: Role of gravity waves in a vortex-split sudden stratospheric warming in January 2009, *J. Atmos. Sci.*, 77, 3321–3342, <https://doi.org/10.1175/JAS-D-20-0039.1>, 2020.
- Vallis, G. K.: Atmospheric and Oceanic Fluid Dynamics, Cambridge University Press, Cambridge, 2017.
- WMO/IQSY: International Years of the Quiet Sun (IQSY) 1964–65. Alert messages with special references to stratwarms, WMO/IQSY Rep. No. 6, Secretariat of the World Meteorological Organization, Geneva, Switzerland, 1964.
- Yoo, J. H., Chun, H. Y., and Kang, M. J.: Vortex preconditioning of the 2021 sudden stratospheric warming: Barotropic–baroclinic instability associated with the double westerly jets, *Atmos. Chem. Phys.*, 23, 10869–10881, <https://doi.org/10.5194/acp-23-10869-2023>, 2023.