Response to reviewers - RC1

Revision on "*Downstream development of extratropical transition of tropical cyclones over the Southern Hemisphere*"

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First, we would like to thank the editor and reviewers for their constructive comments and suggestions, which have helped to improve the logic, flow, analysis, and results' interpretation within the revised manuscript. The resubmitted manuscript has been thoroughly revised according to the reviewers' comments.

In this document, point-to-point responses to reviewers' comments are listed. Note that:

- Reviewers' comments are shown in black
  Authors' responses are shown in red

- The indicated line numbers refer to those in the clean version of the revised manuscript

Major comments

1. Logical structure and guidance

I find that the motivation of this work is very clear and the structure is nicely presented in the introduction. However, while reading Section 3, I found myself lost in the interpretation of the results and the accompanying discussion. In my view, this is at least in part because Section 3 is somewhat a list of results without sufficiently clarifying what should be inferred from each result. For example, the relationship between the PV budget and the Ke framework is not sufficiently clear (see also Other Comment 2). Another issue is that maps for C2 and C3 are generally similar, and it is thus not straightforward to identify which signals should be focused on. Therefore, the authors should revise the manuscript to guide readers more clearly on what should be interpreted from which analysis. In my understanding, one of the most important arguments of this study is:

For C3 ET, the stronger upper-level divergent wind around TCs than in C2 acts to modify the shape of western flank of the ridge just downstream of ET to be more meridional, particularly through the nonlinear term (and an additional contribution of the interaction term). The more prominent upper-level divergent wind is related to the stronger mid-level updraft that is (somehow) associated primarily with upper-level forcing, with a secondary contribution of low-level forcing. The more meridional shape of the western flank of the ridge leads to the poleward propagation of the system, suppressing its downstream development compared to C2, together with the weaker westerly wind speed. Is this correct? Please correct me if I misunderstand. Anyway, clarifying the most important components and their connections would largely improve the readability of the study. Related to the above, Sections 4 and 5 could substantially benefit from shortening by focusing more on the important arguments and logical flow. Specifically, in the current form, Section 4 (Discussion) is too lengthy and contains much of the summary of the results, not just discussions. In my view, for example, most of the part in L391-404 could be removed without loss of important information.

As also suggested by RC2 (major comment 5), we have restructured the Results, Discussion, and Conclusion sections as follows:

a. In the Results section, we integrate the discussions immediately after results are presented, to enhance the logical flow and highlight the physical mechanisms revealed by the diagnostics

b. We have merged the Discussion and Conclusion into a single section with two subsections:
   (1) Main finding: briefly lists the main results found in the current study
   (2) Discussion: Discusses potential limitations, including the lack of an explicit assessment of diabatic processes, which may play an important role during ET

These changes are reflected in the revised manuscript

2. Dynamical interpretations

This manuscript presents multiple lines of analyses based on different frameworks. However, the dynamical interpretations of these analyses are relatively thin, in particular the connection between the results based on different frameworks, leaving questions of the reason behind them. Specific comments on this issue are shown below as “other comments”.

With the restriction presented above, we believe the dynamical interpretations have now been strengthened by interpreting and discussing results immediately after they are presented.

3. Schematic figure

Related to Comments A and B, it is not straightforward for readers to follow the overall argument and identify the core findings related to the dynamics and differences between the two clusters, as the two clusters are seemingly similar in many ways. Therefore, I would strongly recommend adding a schematic figure to summarize the findings obtained by this study. It would largely help clarify the most important components and their connections (as commented in Comment A).

Thank you for this very helpful suggestion. Two schematics have been added to the revised manuscript to illustrate the key features of the two ET downstream pathways. These are shown here below.

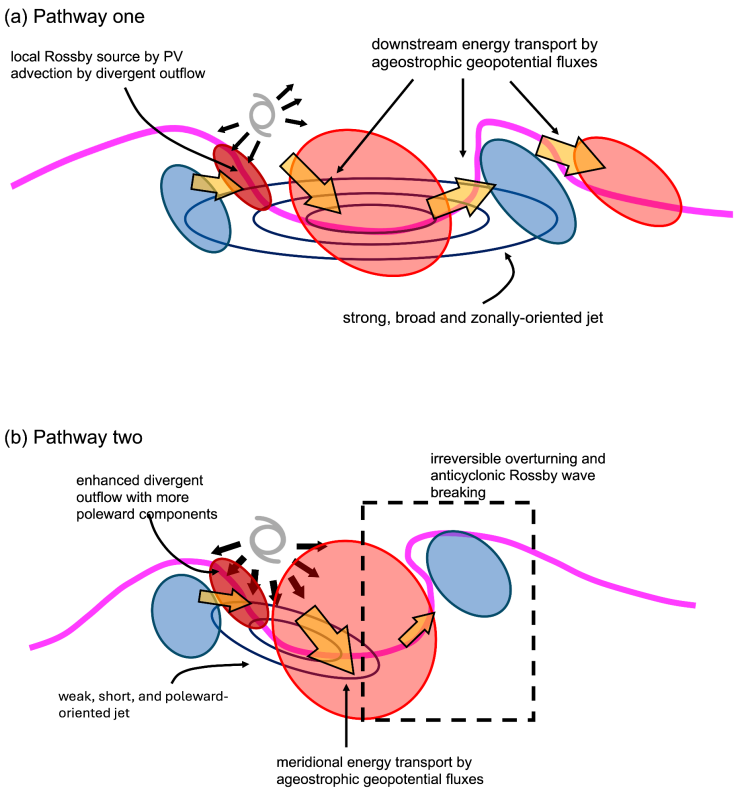


Figure R1. Schematics for two ET downstream pathways, inferred from C2 and C3 ET events in the Southern Hemisphere. The TC symbol indicates the position of the transitioning cyclone. The magenta line marks the dynamic tropopause. Light blue and red ellipses mark the Rossby wave packet, with blue for cyclonic PV anomaly and red for anticyclonic PV anomaly. Blue contours denote the jet stream. Black arrows are divergent outflow associated with the transitioning TC, and the dark red ellipse marks the anticyclonic PV advection by the divergent outflow. Yellow arrows indicate the eddy kinetic energy redistribution by ageostrophic fluxes. The black dashed box highlights the zone of wave breaking.

4. Regarding ET clustering

> The present study utilizes a particular clustering of ET events into four clusters. As this clustering is an important basis for this work, it needs to be explained in more detail, so that readers do not necessarily need to refer to previous papers. The most crucial question is what criteria were used for the clustering (e.g., track shape, intensity, duration, translation speed, life cycle, geographical location, etc.). For example, there seems to be a large difference in the typical intensity of TCs in each of the ET clusters (as mentioned in the later part of the manuscript). Is this related to the configuration (and motivation) of the clustering? I also wonder if there is a discernible difference in the typical longitude of each cluster. Such clarification would help readers figure out why this study focuses on C2 and C3 only and separately.

As also suggested by RC2, a clarification of ET definition and clustering method has now been added as a separate subsection in the Data and Methods (Section 2.2 in the revised manuscript), and it is briefly presented here.

Clustering technique

As in Jin et al. (2026), K-means clustering is performed based on 2226 x 2226 km cyclone-centred fields of both 500-hPa geopotential height (Z500) and MSLP at ET time for 506 detected ET events in the Southern Hemisphere during the period 1979-2021. The reason to include Z500 and MSLP is to consider the interaction between TCs and their upstream trough. Z500 has been used to determine ET time, as a TC becomes an open wave in the upper-levels (Demirci et al. 2007). Before clustering, the Z500 and MSLP fields are standardised separately. We determine the optimal cluster number according to the silhouette score and Davies-Bouldin index. We found that choosing four clusters maximised the silhouette score and minimised the Davies-Bouldin score, making it an optimal choice. With the clustering approach, the four ET clusters are identified, and they are associated with different synoptic configurations and TC characteristics. C2 and C3 are characterised by the scenario that a strong TC undergoes rapid transition at higher latitudes, embedded in largely amplified midlatitude flow. In contrast, C1 and C4 are associated with weaker TCs and less pronounced transition and midlatitude flow amplification.

Longitudinal differences

C1and C4 appear to be evenly distributed over the Southern Indian Ocean, the Australian region, and over the South Pacific, with two C1 outliers near South America. C2 and C3 tend to be more frequent over the South Africa sector of the Southern Indian Ocean, and the New Zealand sector of the South Pacific, whereas they are infrequent around Australia

(Figure R2). Detailed discussion is now provided in the revised manuscript in response to major comment 6.

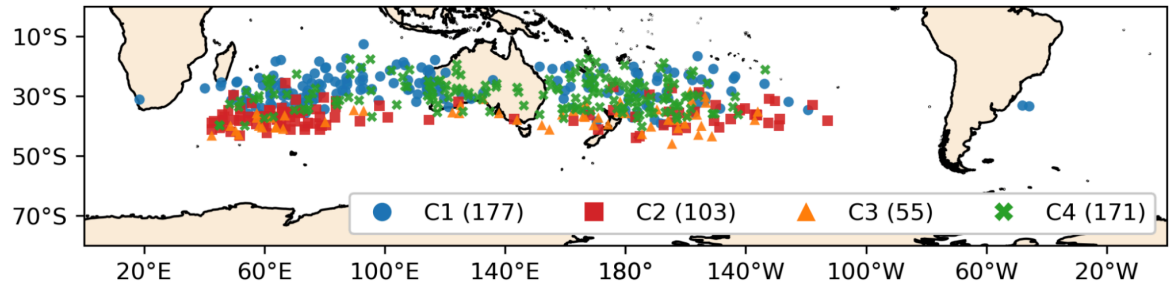


Figure R2. Geographic locations of TCs at ET time for clusters 1, 2, 3, and 4. Numbers in parentheses denote the number of ET events in each cluster. Figure 9 in Jin et al. (2026).

5. Influence on the Australian region

> It is nice to show the downstream influence of ET on the Australian region. However, I wonder why the authors combine C2 and C3 to investigate their downstream influence, given that the characteristics of their downstream development are distinct (as argued in the prior parts of the manuscript). To focus on the difference between C2 and C3 would fit the flow of this work much better.

It is more rational to compare the downstream influences of C2 and C3 ET separately, as they are featured by different downstream pathways (Figs. 6–9 in the manuscript), although the subsetting produces a relatively small sample size of C3 (16 events). Their corresponding composites of 350-K PV, PV anomaly and MSLP anomaly are shown in Figure R3. The subsets of C2 and C3 are generally consistent to the full sets of C2 and C3 (Fig. 3 in the manuscript), with more zonally-propagating wave packets in C2, and a more pronounced and poleward-propagating anticyclonic PV anomaly in C3. The composites of combined SWIO C2 and C3 ET events (Fig. 10 a–e in the manuscript) appears to be dominated by C2, given its larger sample size (63 events).

Regarding their influences on Australian weather, SWIO C2 ET results in a significantly increased frequency of warm days over the Australian region, with an eastward propagation from the west to the east of the country (Fig. R4e–h). This progression of the warm day frequency is spatially and temporally consistent with the eastward movement of anticyclonic PV anomalies on the 350-K isentropic surface. Meanwhile, the frequency of heavy rainfall days increases over southeastern Australia at +48 and +72 h, presumably associated with the front extending from the eastward-moving surface low (Fig. R4a–d). These patterns are broadly consistent with those of the combined C2 and C3 ET composite (Fig. 11 in the manuscript), again indicating that the combined patterns are largely shaped by C2. In contrast, the downstream weather impacts associated with SWIO C3 ET exhibit less spatial and temporal coherence (Fig. R5). Therefore, it is difficult to build a consistent relationship

between SWIO C3 ET and Australian weather, owing to a more localised downstream development of C3. Nonetheless, the enhanced heavy-rainfall frequency at +72 h appears to be associated with an eastward-propagating surface low linked to the upstream ET event (Fig. R5c–d).

We have revised Section 3.4 in the revised manuscript, to incorporate the new results and their interpretation.

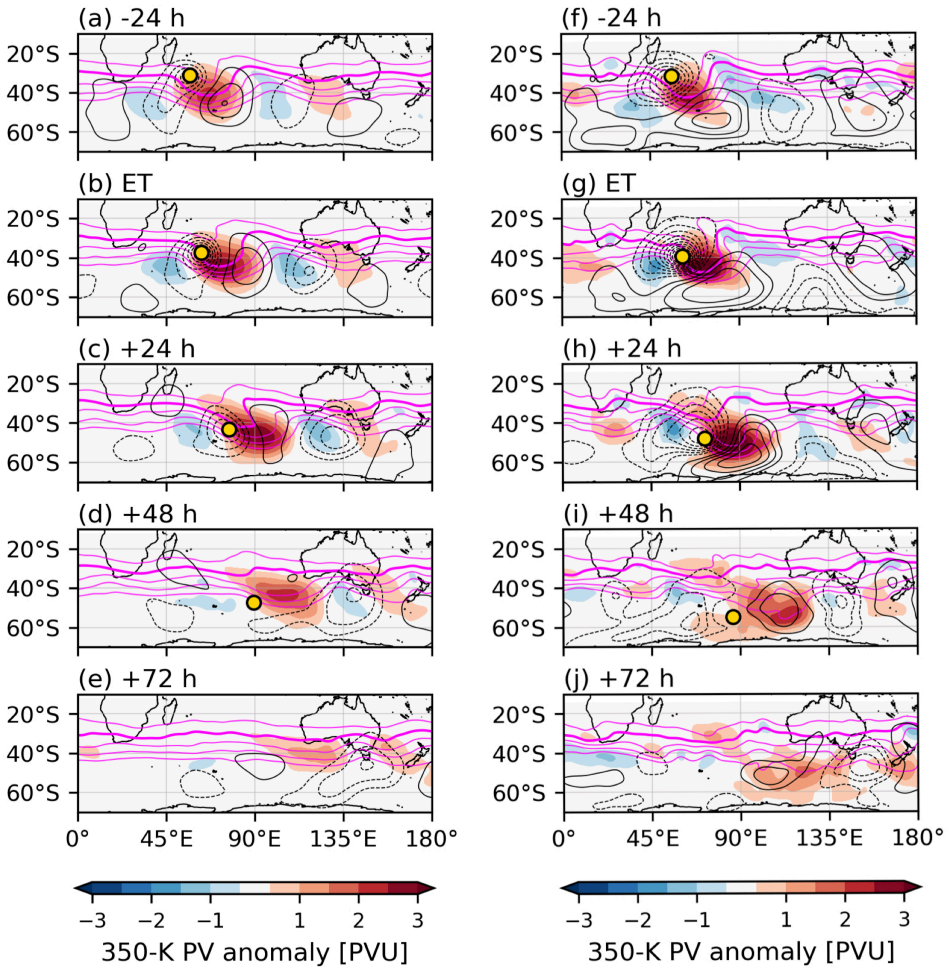


Figure R3. Composites of 350-K PV, PV anomaly, and MSLP anomaly for (left) C2 and (right) C3 ET events in the southwestern Indian Ocean. The 350-K PV anomaly is shaded as per the colourbar, and the MSLP anomaly is marked by black contours (every 2 hPa starting from ±2 hPa; solid for positive and dashed for negative values). 350-K PV is indicated by magenta contours (every 1 PVU starting at -1 PVU), and a thick line highlights the -2 PVU contour. Yellow dots mark the cyclone position, and a red dot marks the cyclone position at ET.

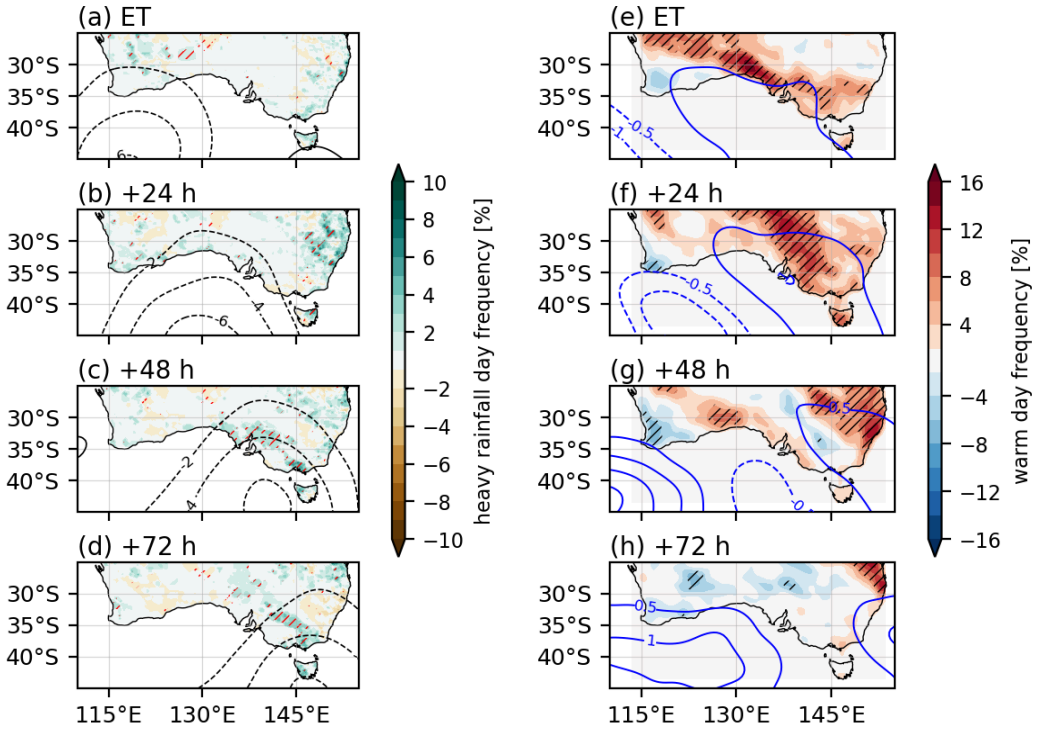


Figure R4. Frequency anomalies of (left) heavy rainfall days and (right) warm days during and after SWIO C2 ET events over southern Australia (south of 25S). The frequency anomalies are shaded as per the colorbar. Statistically significant anomalies are hatched in red and grey, respectively. MSLP anomalies are marked by black contours (every 2 hPa starting from ±2 hPa; solid for positive and dashed for negative values), and the 350-K PV anomaly is marked by blue contours (every 0.5 PVU starting from ±0.5 PVU; solid for positive and dashed for negative values).

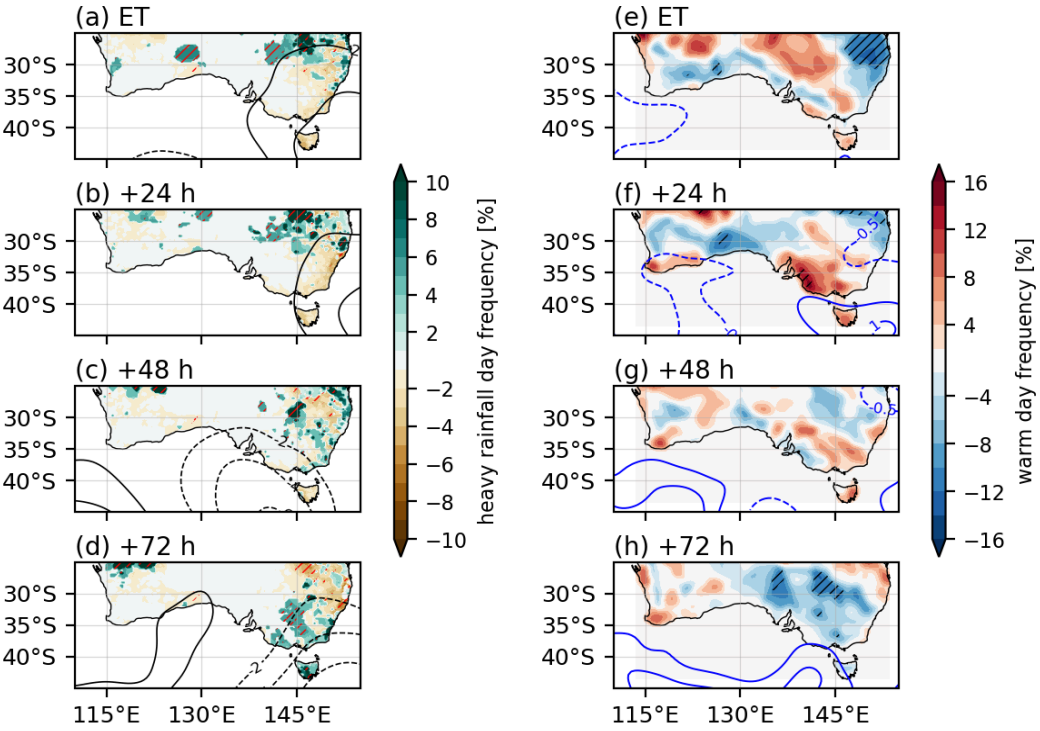


Figure R5. As in Figure R4, but for SWIO C3 ET events.

6. Regionality

In the present study, the discussion on regionality of ET event dynamics is limited. It may be true that the Southern Hemisphere is more zonally symmetric than the Northern Hemisphere, but the effect of landmass and zonal asymmetry in mean states (such as jet streams) can have an impact on ET event dynamics differently in different basins. If the four clusters reflect the difference in the typical longitudinal sector of ET events, such a zonal asymmetry in basic states is likely to play an important role. In addition, it would also be beneficial to add a discussion on the influence of oceanic fronts (especially in the Southern Indian Ocean) on ET event dynamics. I suspect that considering the regionality of the clusters would help elucidate the importance of the pre-existing midlatitude flow for the ET characteristics.

Thank you for your suggestions. We have enhanced the discussion on regionality in the revised manuscript. This includes:

a. a new paragraph in the Introduction to discuss the regional differences
b. a paragraph to illustrate the limitations in investigating regionality. Specifically, C2 and C3 show less case-to-case longitudinal variability over the southwest Indian Ocean but larger variability in the South Pacific; this would motivate future research

The current study focuses more on the large-scale atmospheric dynamics, and analysis and discussion of contributions from oceanic processes are beyond the scope of this manuscript, but left for future research.

Minor comments

1. Introduction: It would be helpful to clarify the focus of this study on the downstream influence of ET on the Australian region (L95) and its importance before Section 2.

We clarify that our focus on the downstream influence of ET over Australia is motivated by Quinting and Jones (2016), who showed that recurving TCs over the south Indian Ocean are associated with amplified Rossby waves downstream over Australia. As heatwaves in southeastern Australia have been linked to amplified Rossby wave patterns (Parker et al., 2013, 2014), understanding upstream ET events may be important for improving heatwave prediction in the Australian region.

The changes are made in the Introduction section in the revised manuscript (L85-90).

2. L36-68: I found that this part can benefit from focusing more on literature relevant for the present study. It would also be beneficial to add an explanation of the difference between the two frameworks (i.e., pros and cons, and why we need both in this study).

Thanks for the suggestion. We rewrote these two paragraphs to highlight the strengths and limitations of the EKE and PV frameworks. In particular, we now discuss how these two frameworks provide different perspectives on the physical processes governing ET downstream development.

The changes are made in the Introduction section in the revised manuscript (L44-50 and L51-68).

3. L149: available potential energy -> eddy available potential energy?

Changed

4. Figure 2: The caption says the contour interval is 1hPa. Is this correct? If this is correct, I wonder why anomalies particularly in C1 and C4 are so weak.

The MSLP contour interval is 1 hPa. C1 and C4 are associated with relatively weaker central pressure and less compact MSLP contours, compared to C2 and C3 (Figures 10c and 11a, d, j, and g in Jin et al. 2026). This difference arises from the K-means clustering that considers the 2226km × 2226km cyclone-centred fields of MSLP and Z500 at ET time.

5. The titles of subsections 3.2 and 3.3 are very similar. In my view, it would be beneficial to change the title of Section 3.3 to represent its content more effectively.

Agree. The title of subsection 3.3 has now been changed to *Two pathways of ET downstream development.*

6. Figure 5: It seems that the more prominent negative QGomega_top just downstream of the ridge in C3 (Fig. 5b) compared to C2 is a very important factor characterizing their different nature in the downstream development. Can the authors explain what makes this signal (e.g., upper-level vorticity advection by something)?

We assume the reviewer's comment refers to the pronounced $QG\omega_{top}$ downstream of the upper-level trough in Fig. 5b. The Q-vector form of the QG omega equation does not explicitly address the contributions from vorticity advection. As a result, we use the traditional form of the QG omega equation here:

$$\left(\nabla_p^2 + \frac{f_0^2}{\sigma}\frac{\partial^2}{\partial p^2}\right)\omega = -\frac{f_0}{\sigma}\frac{\partial}{\partial p}\left[-\mathbf{V}_g \cdot \nabla_p(\zeta_g + f)\right] - \frac{R_d}{\sigma p}\nabla_p^2\left(-\mathbf{V}_g \cdot \nabla_p T\right)$$

In this formula, QG vertical motion is forced by two terms: the vertical derivative of geostrophic absolute-vorticity advection (F_{vor}) and the horizontal Laplacian of geostrophic temperature advection (F_T). Positive values of the forcing terms favour QG ascent, whereas negative values favour QG descent.

Fig. R6 shows the C2 and C3 composites of geostrophic absolute-vorticity advection and F_{vor}, vertically averaged over the 250 hPa and 500 hPa. C3 exhibits a much stronger cyclonic vorticity advection and positive F_{vor} in the vicinity of the transitioning cyclone, compared with those of C2. In C3, the stronger vorticity advection may arise from stronger geostrophic wind associated with the upstream trough and stronger relative vorticity associated with a stronger TC (Fig. 3h in the manuscript). However, the enhanced QG ascent may not be explained by geostrophic absolute-vorticity advection alone, given large amplitudes of temperature-advection forcing (F_T shown in Fig. R7). This reflects the influence of TC-associated circulation on the surrounding temperature fields. Interestingly, F_T and F_{vor} has approximately opposite spatial patterns (Figs. R6 and R7), suggesting that two terms partially offset each other. Nevertheless, the combined forcing has positive values near and south of the transitioning cyclone (Fig. R8), spatially consistent with the $QG\omega_{top}$ in Fig. 5b of the manuscript.

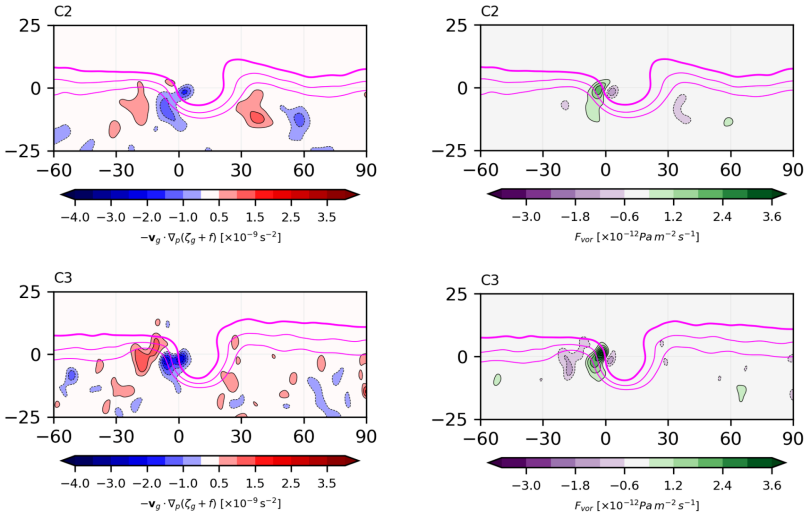


Figure R6. Composites of 250-500-hPa geostrophic absolute-vorticity advection and F_{vor} (shaded as per the colour bar) for (top) C2 and (bottom) C3. Magenta contours mark the 350-K PV (at -2, -3, and -4 PVU) with the thick contour indicating -2 PVU.

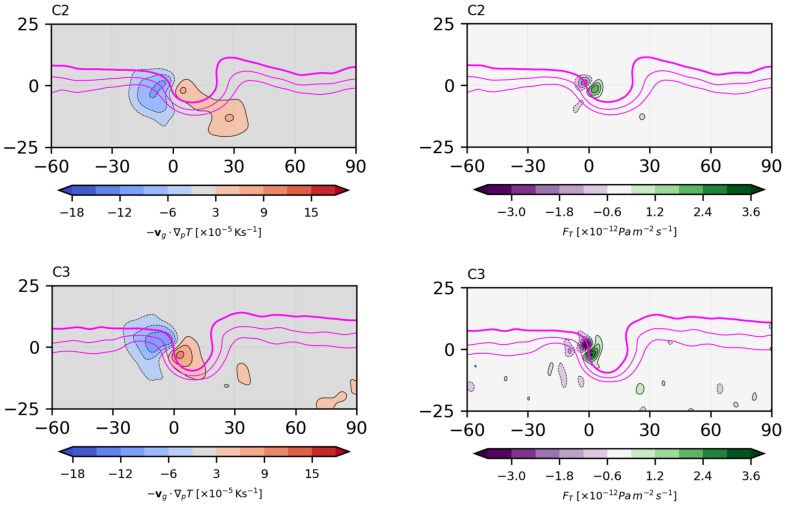


Figure R7. Composites of 250-500-hPa temperature advection and F_T (shaded as per the colour bar) for (top) C2 and (bottom) C3. Magenta contours mark the 350-K PV (at -2, -3, and -4 PVU) with the thick contour indicating -2 PVU.

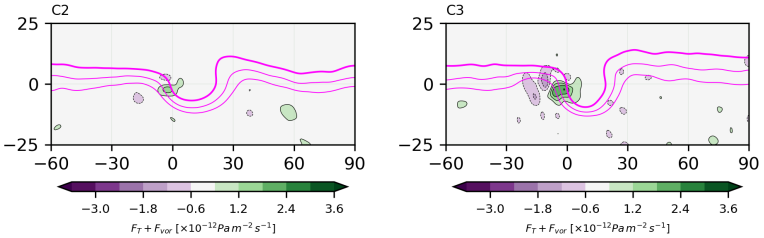


Figure R8. Composites of the 250-500-hPa combined forcing (F_T and F_{vor}; shading) for (left) C2 and (right) C3. Magenta contours mark the 350-K PV (at -2, -3, and -4 PVU) with the thick contour indicating -2 PVU.

7. L265-266: Can the authors explain why “this tendency pattern” is in phase with the propagation terms? I wonder how this is related to the weakening TC cyclonic PV tower (L266). This applies to L355-356. Please elaborate a little more on the mechanisms.

We re-phrased the sentence with more explanation of the mechanism in the revised manuscript (L365-L368) and here:

As $\vec{V}_{\chi}$ and likely the balanced circulation associated with TC cyclonic PV tower weaken, their associated anticyclonic PV tendencies also diminish by +24 hr (Fig. 6e and j). Therefore, the nonlinear tendency is dominated by the cyclonic tendency associated with upstream trough advection, broadly in phase with the linear propagation terms (Fig. 6j and o). This suggests that the downstream wave pattern becomes increasingly controlled by the linear Rossby wave propagation.

8. Figures 6 and 7: These ET events exhibit a clear signal of anticyclonic Rossby wave breaking (AWB) downstream (as touched on in the later part of the manuscript). Can the authors relate such AWB signature and the downstream influence around the Australian region, for example, by the climatological-mean AWB frequency distribution?

As also suggested by RC2 (major comment 3). The Rossby wave dataset by Barnes et al. (2025) is applied to assess the Rossby wave breaking (RWB) downstream of C2 and C3 ET events. The detailed methodology can be found in response to RC2’s major comment 3. The methodology and results have also been incorporated into the revised manuscript (Section 2.6 and Section 3.3.5) respectively.

We briefly present the results here. It is evident that C2 and C3 ET events are able to significantly enhance downstream AWB frequency, while suppressing AWB frequency upstream and in the vicinity of the transitioning cyclone (Fig. R9). The positive AWB frequency anomalies in C2 appear to be more transient than those in C3 (Fig. R9a–e). In contrast, the enhanced AWB activity in C3 is more persistent and covers a broader area, with a more pronounced poleward extension (Fig. R9f–j). The persistence of enhanced AWB activity, together with limited far-downstream development (Figs. 7 and 9 in the manuscript), suggests that the AWB may contribute to the decay of the downstream Rossby wave packet (Barnes et al. 2025) and potentially limit its zonal propagation through redistributing energy meridionally (Wirth et al. 2018). We also examined CWB frequency anomalies associated with ET events, but did not find any spatially coherent patterns of statistically significant anomalies (figure not shown).

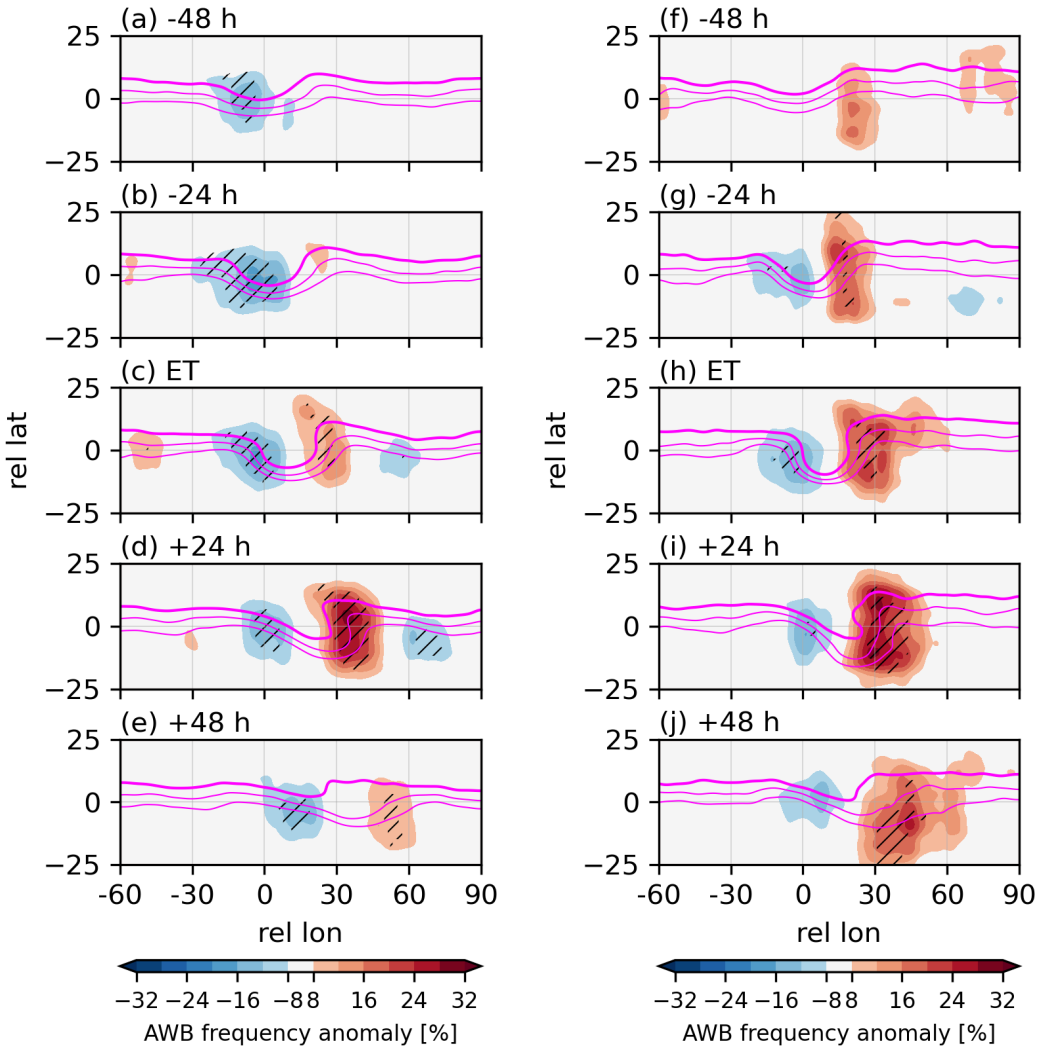


Figure R9. Lead-lag composites of AWB frequency anomaly (shading) and 350-K PV (magenta contours at -2, -3, and -4 PVU) for (left) C2 and (right) C3 ET events. Statistically significant anomalies are hatched.

9. L271: “60 and 90E” seems misleading, as this is relative longitude in reference to the ET. Please consider revising it.

Changed to 60 and 90 deg east of ET in L370-L371.

10. Figure 8: I guess that the shading indicates convergence, not divergence. Please check it. In addition, I wonder why the authors show selected contours of geopotential height at 500-hPa. Perhaps other variables would work better (e.g., wind speed to show jet streams).

Yes, the shading indicates convergence instead of divergence. Changes are made in the figure caption. We agree that 500-hPa geopotential height may be less effective to show the upper-level wave pattern. However, the jet stream (200-300-hPa wind speed) has already been shown in Figs. 4 and 5 in the manuscript. Therefore, we have plotted 300-hPa geopotential height to highlight the upper-level troughs and ridges.

11. L388: Figs -> Fig

Changed.

12. L419-420: There is an error around “Jin et al., 2026”.

Changed

13. Conditions for ET: While the authors mention that investigating conditions that determine ET characteristics (i.e., clusters) is future studies in Section 5, I would recommend adding some discussions on it. Obviously, one potentially important factor is a jet stream (as mentioned in L371), but other factors, such as preexisting wave-like anomalies (as seen in Fig. 3b), can also play a role.

Discussion of the factors determining the ET downstream pathways are now added.

References

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