Response to reviewers - RC2

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 us 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. While it is true that SH ET has not been targeted as much in the past, the simple focus on an understudied region does not necessarily constitute sufficient motivation. In this regard, it would be useful to understand whether there are some SH-specific features that make studying ET in this basin particularly interesting with respect to other basins. Or do the authors expect to find the same behavior as in the North Pacific and the North Atlantic? I encourage the authors to emphasize these aspects in the Introduction and in the discussion of the results, highlighting specific SH aspects more explicitly (e.g., the first coming to mind is the fact that the SH jet is generally more zonal than over the Northern Hemisphere).

Thanks for the suggestion. We have enhanced the perspective in the Introduction (L90-L93) and Section 3.3.2 (L329-342) of the revised manuscript.

2. Related to the previous point, I have the impression that the results of this study are not properly contextualized with respect to the previous literature that specifically focused on the downstream impact of ET. For instance, the presence of different "pathways" or "flavours" of flow amplification downstream of ET had been discussed by Riboldi et al. (2019) for the western North Pacific basin and by Brannan and Chagnon (2020) for the North Atlantic, which are not cited here: do the C2/C3 subsets discussed in this paper correspond to those previous categorizations, or do they highlight something different and/or peculiar to the SH? I am thinking in particular to the results of Brannan and Chagnon (2020), who noticed that in some cases North Atlantic ET can correspond to flow de-amplification – are there potentially similarities with cluster C3, with the wave propagation interrupted by wave breaking? This contextualization would deserve, in my opinion, a dedicated paragraph of discussion and maybe more space in the Introduction.

Thanks for suggesting the relevant studies. We have now written a new paragraph to discuss different downstream responses owing to phasing between the transitioning TCs and propagating Rossby wave packets. This can be found in L69-L84 in the revised manuscript.

3. A Rossby wave breaking diagnostic could be applied to better understand why Rossby wave propagation is halted in C3 with respect to C2, and to put into context the results obtained from the PV and Ke budgets.

The Rossby wave breaking dataset by Barnes et al. (2025) is applied to assess the Rossby wave breaking (RWB) downstream of C2 and C3 ET events. The methodology and results

are presented below, and they have also been incorporated into the revised manuscript (Section 2.6 and Section 3.3.5 respectively).

RWB is defined as the region in which a single longitude line intersects the -2-PVU contour at least three times. The RWB zone is then defined as the box bounded by the western-most and eastern-most longitude and northern-most and southern-most latitude of the overturned contour. The wave breaking events are further categorised into anticyclonic wave breaking (AWB) and cyclonic wave breaking (CWB), through comparing the eastern-most and western-most boundaries of the overturned contour. The RWB dataset provides two-dimensional binary masks on individual isentropic surfaces, with values of 1 indicating the occurrence of RWB. We combine 6-hourly RWB masks on five isentropic levels (330, 335, 340, 345, and 350 K), identifying RWB if wave breaking is found on at least one of these surfaces. The RWB frequency is calculated by averaging the binary RWB fields over ET events, and the RWB frequency anomaly is then computed as the difference between observed composite frequency and a background frequency from a Monte Carlo approach. We generate one thousand random composites, and each contains the same number of events as the observed composite. For each event, a random date is selected by randomly choosing a year from 1979–2021 and a month and day within a 14-day window centred on the corresponding ET time. The background RWB frequency is the mean of these one thousand random composites. Statistical significance is tested using the 5th and 95th percentiles of the Monte Carlo distribution, with anomalies considered significant if they are below the 5th percentile or over the 95th percentile.

As shown in Fig. R1, 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. The positive AWB frequency anomalies in C2 appear to be more transient than those in C3 (Fig. R1a–e). In contrast, the enhanced AWB activity in C3 is more persistent and covers a broader area, with a more pronounced poleward extension (Fig. R1f–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).

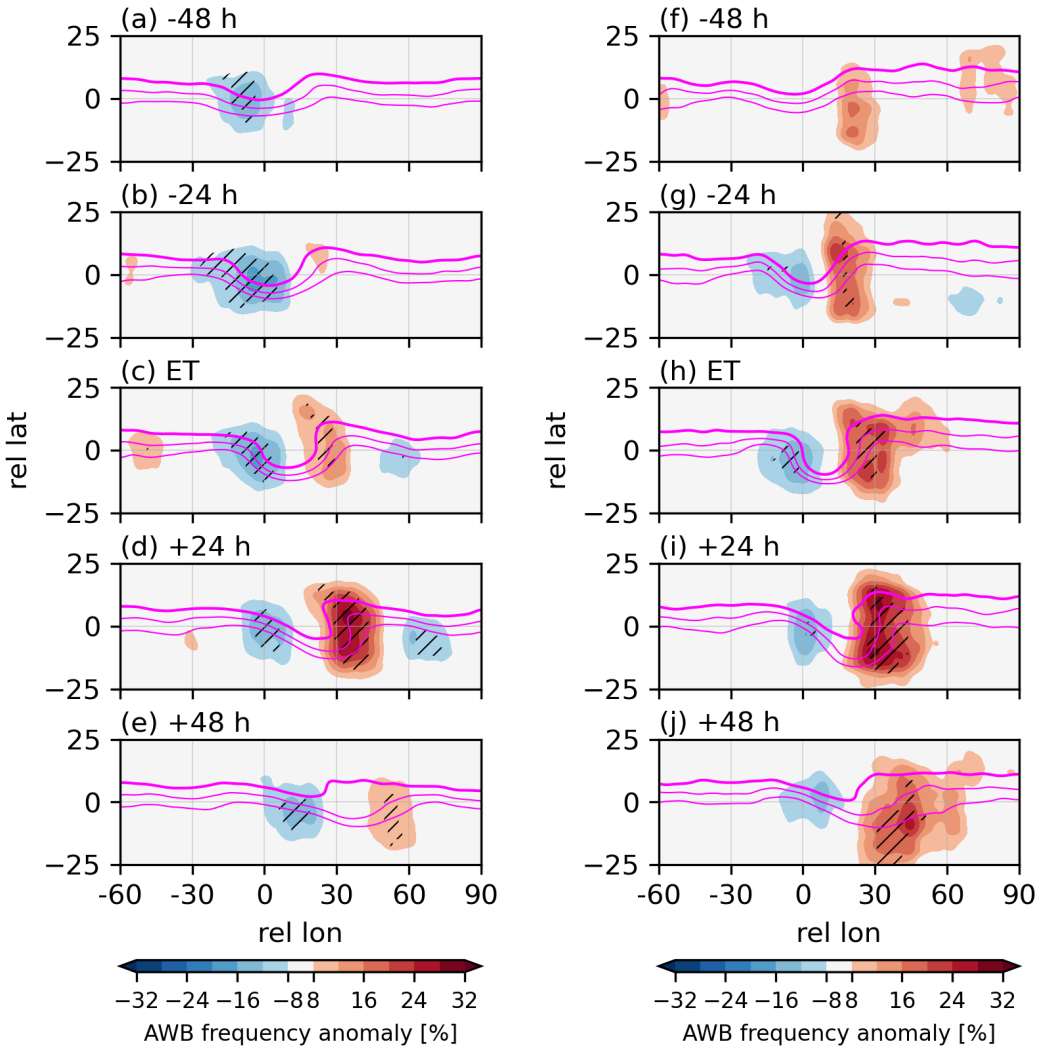


Figure R1. Lead-lag composites of AWB frequency anomaly (shading) and 350-K PV (magenta contours at -2, -3, and -4 PVU) for (left) C2 and (right) C3 ET events. Statistically significant anomalies are hatched.

4. In principle, one should be able to read and understand the analysis without having to go back and read Jin et al. (2026), too. For instance, in the manuscript there is no explanation of the adopted definition to define ET events (i.e., based on Hart's cyclone phase space) or of the rationale driving the cluster analysis. I would suggest discussing those aspects specifically in Sect. 3.1 or, even better, in a separate methodology paragraph. I also do not understand why seasonality of different clusters is the discussed there in Sect 3.1, as it is not further mentioned or addressed in the remaining parts of the paper.

As also suggested by RC1, a clarification of the ET definition and clustering method has been added as a separate subsection in the revised Data and Methods. This can be found in Section 2.2 of the revised manuscript.

5. The paper has several descriptive sections in which the novel results get quite diluted (I am thinking in particular to Sect. 3.3.2, 3.3.3 and 3.3.4): the authors should try to emphasize more what is learnt from each analysis, rather than merely describing the features that are visible in them and their difference. A way to do so could be to conclude each paragraph in those sections with a short statement about the relevance/what has been learnt in the previous lines.

Many thanks for the suggestion. As also suggested by RC1 (major comment 1 and 2), 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. Merged 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.

Minor comments

1. Line 8: what are these “nonlinearities”? Could it be specified here?

Nonlinearities refer to the interaction between different PV features, namely the PV-induced flow advecting other PV features. Changed to "supplemented by the anticyclonic PV tendency arising from the interactions between PV anomalies” in L8 of the revised manuscript.

2. Lines 28-31: a relevant citation here is Pohorsky et al. (2019), that specifically focused on heavy precipitation events downstream of North Atlantic ET events related to upper-level flow amplification.

Citation added.

3. Line 36-48: Would it be possible to highlight in which aspects the eddy kinetic energy perspective of ET differs with respect to the eddy kinetic energy analysis of classic baroclinic development?

The differences are highlighted in L44-L50 of the revised manuscript. The key difference from the conventional baroclinic development is that the ageostrophic geopotential flux convergence near the first downstream ridge is influenced not only by ageostrophic flow associated with midlatitude troughs and ridges, but also by the divergent outflow associated with TC-midlatitude flow interactions (Quinting and Jones, 2016; Keller et al., 2019).

4. Line 64: what is meant here with “cyclonic PV advection”? Shouldn’t it be “anticyclonic” (as at line 78)?

Yes, it should be anticyclonic (or negative in NH) PV advection by the divergent wind. This has now been changed in the revised manuscript.

5. Sec. 2.3: please specify already here (rather than only at the end of the manuscript) that the diabatic term is not included, why it is so and in which regard this is supposed to affect the results.

The diabatic contribution is not directly addressed in the PV budget here, as we assume that PV is conserved on an isentropic surface in the absence of diabatic heating and friction. However, diabatic processes can be directly important for ET downstream development. For instance, the ascending moist and warm air stream ahead of the transitioning cyclone (developing warm conveyor belt) can largely modify PV and therefore

contribute to downstream ridge development (e.g., Grams et al. 2011). Nonetheless, previous studies have linked the PV advection by divergent outflow (I_χ) to latent heat release within the transitioning cyclone (Keller et al. 2019). Therefore, I_χ can be a diagnostic of the indirect influence of diabatic processes on ET downstream development, but it may not reflect the full diabatic influences.

Changes are made in L172-L178 in the revised manuscript.

6. Sect. 2.4: is the Ke budget closed? How large are the residuals?

The Ke budget is not closed. Figure R2 shows the lead-lag composites of residuals, defined as the Ke tendency minus the sum of the budget terms, for C2 and C3 ET events respectively. The residuals show a notable dipole around the transitioning cyclone, with positive values to the south and southeast and negative values to its north and northeast. Notably, the area of positive residuals is largely consistent with the regions of ascent (Figs. 4h and 5h in the manuscript). This suggests that the residuals may be influenced by latent heating and convection within the ascending airstream south/southeast of the cyclone, potentially linked to the warm conveyor belt. As also mentioned by Pinheiro et al. (2022, 2024) in their studies of cut-off lows, the exiting residuals suggest that some aspects of the ET downstream development may not be explicitly considered in the Ke framework, or they are not properly represented in reanalysis datasets, particularly in the region of strong diabatic activity.

Furthermore, the box-averaged Ke budget terms show smaller residuals within the area around the transitioning cyclone (Fig. R3), suggesting a better closure in the regional mean. Nonetheless, C3 features relatively larger residuals than C2, presumably suggesting a greater influence of diabatic processes in C3 downstream development.

This has been added in L202-L205 of the revised manuscript.

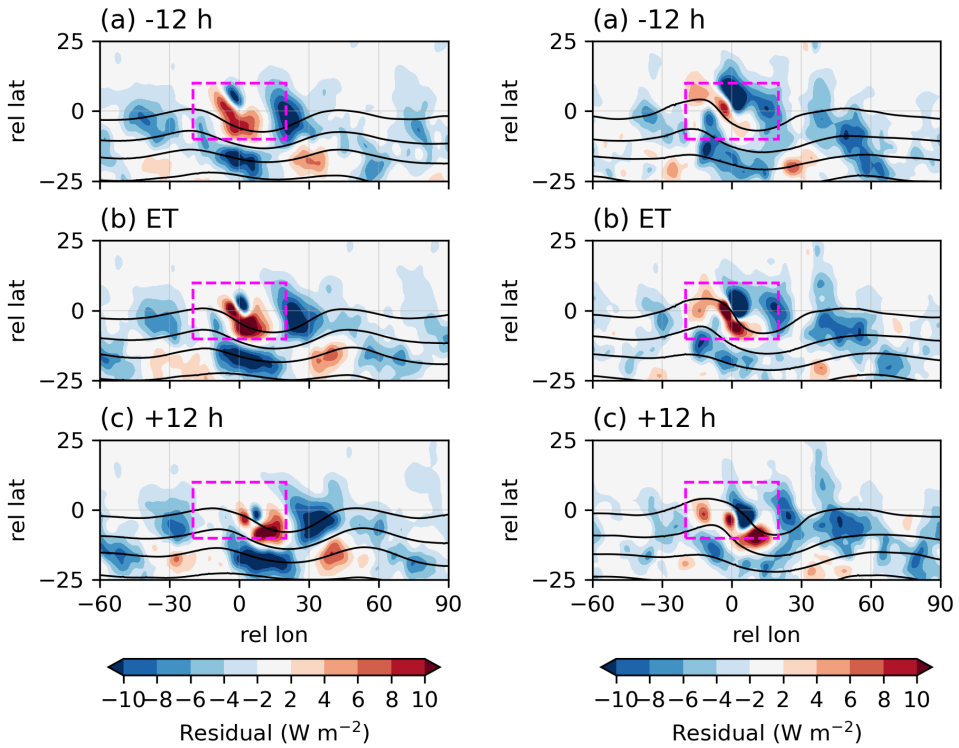


Figure R2. Lead-lag composites of the residuals in the Ke budget (shading) and 300-hPa geopotential (contours) for (left) C2 and (right) C3 ET events. The dashed magenta boxes indicate the area used to calculate the temporal evolution of the Ke budget terms.

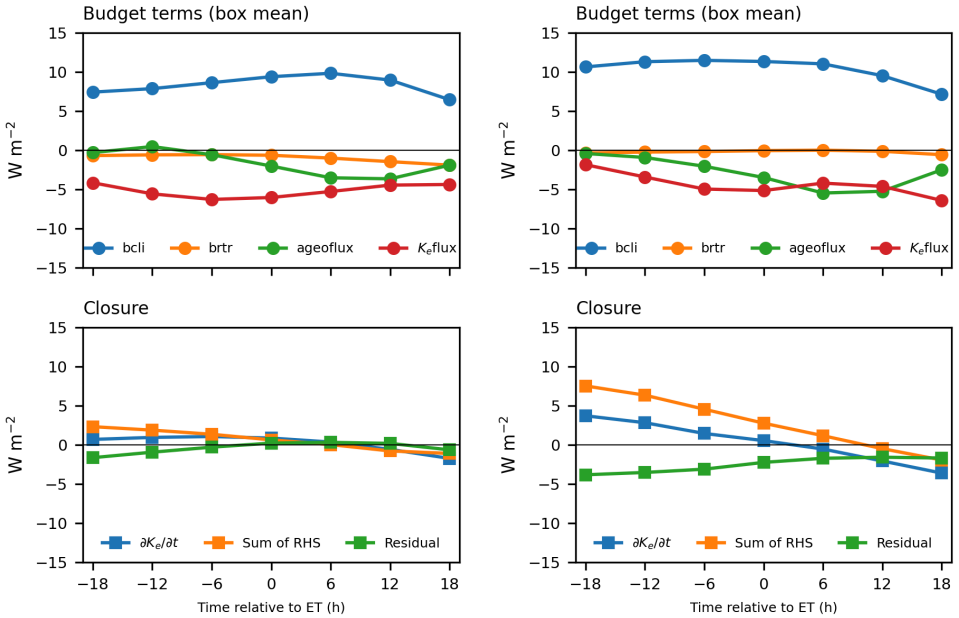


Figure R3. Temporal evolution of Ke terms for (left) C2 and (right) C3 ET events. Individual terms are spatially averaged in the magenta dashed box shown in Fig. R2.

7. Fig. 6f-j: would it be possible to show here, rather than repeating the divergent wind in the second column? It would match better the description of the PV advection pattern by the eddy-related nondivergent wind shown in those sub-plots and discussed at lines 257-268. Would it be also possible to show and highlight the location of the PV tower, which is discussed several times in Sect 3.3.3?

We agree. The non-divergent wind is now plotted on the second column of Fig. 6 in the revised manuscript.

Regarding the PV tower, the yellow dot in each panel of Fig. 6 indicates the location of the transitioning cyclone centre, providing a horizontal reference for the PV tower discussed in Sect. 3.3.3. Nonetheless, we agree that vertical cross sections could better illustrate the three-dimensional PV structure. However, we would like to avoid increasing the complexity of the figures in the main manuscript. The vertical cross sections at ET time can be found in our previous paper (Fig. 12 in Jin et al. 2026).

8. Section 3.3.4: This whole section might sound rather descriptive. I believe the manuscript would profit here from a more explicit acknowledgment of the novel scientific insights gained from such a detailed analysis of the Ke composites, also with respect to Quinting and Jones (2016) who used the same approach.

Section 3.3.4 has now been largely revised to include more dynamic interpretation.

9. Line 316: maybe I missed it, but how is this “strong ET” subset defined?

“Strong” ET refers to a subset of C2 and C3 ET events, because they have more pronounced signatures of downstream development (stronger downstream anticyclonic PV anomalies and further downstream cyclonic PV anomalies), compared with C1 and C3 (Fig. 2 in the manuscript). The wording “strong” is confusing here, so we changed it to “a subset of C2 and C3 ET events over the southwestern Indian Ocean” in the revised manuscript.

10. Line 317: it seems like C2 dominates the subset of “strong ET”. What is the reason behind merging it together with the fewer C3 cases? How would the composites of Fig. 10 look like if C2/C3 cases were to be considered separately?

The original logic was to see the general (or combined) influences of “strong” ET in the southwest Indian Ocean (SWIO) on the Australian weather. Nonetheless, as also suggested by RC1 (major comment 5), it is more rational to compare the downstream influences of C2 and C3 ET separately, given their different downstream pathways (Figs. 6–9 in the manuscript). Their corresponding composites of the 350-K PV, PV anomaly and MSLP anomaly are shown in Figure R4. The subsets of C2 and C3 are generally consistent with

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) appear to be dominated by C2, given its larger sample size (63 events).

Regarding their influences on Australian weather, SWIO C2 ET results in significantly increased frequency of warm days over the Australian region, with an eastward propagation from the west to the east of the country (Fig. R5e–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. These patterns are broadly consistent with those of the combined SWIO C2 and C3 ET composite (Fig. 11 in the manuscript), 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. R6e–h). Therefore, it is difficult to build a clear relationship between SWIO C3 ET and Australian weather, presumably 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. R6c–d).

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

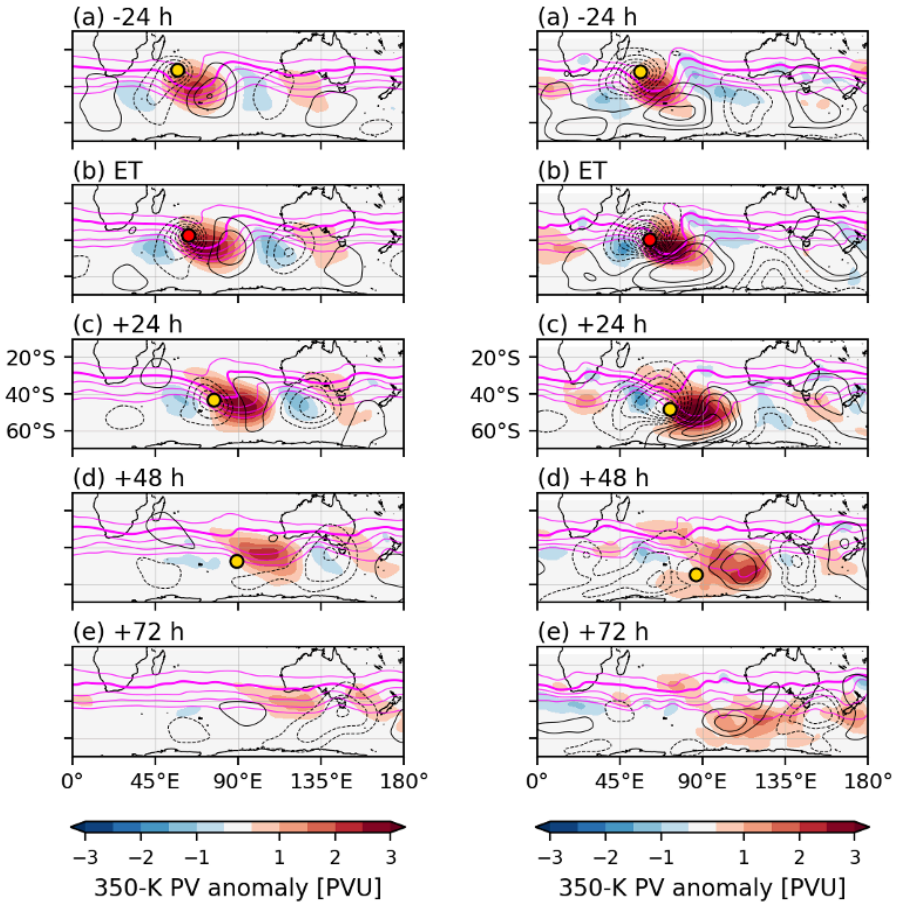


Figure R4. Composites of 350-K PV, PV anomaly, and MSLP anomaly for (left) C2 and (right) C3 ET events in the southwest Indian Ocean. 350-K PV anomaly is shaded as per the colourbar, and MSLP anomaly is marked by black contours (every 2 hPa starting from ±2 hPa; solid for positive and dashed for negative 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.

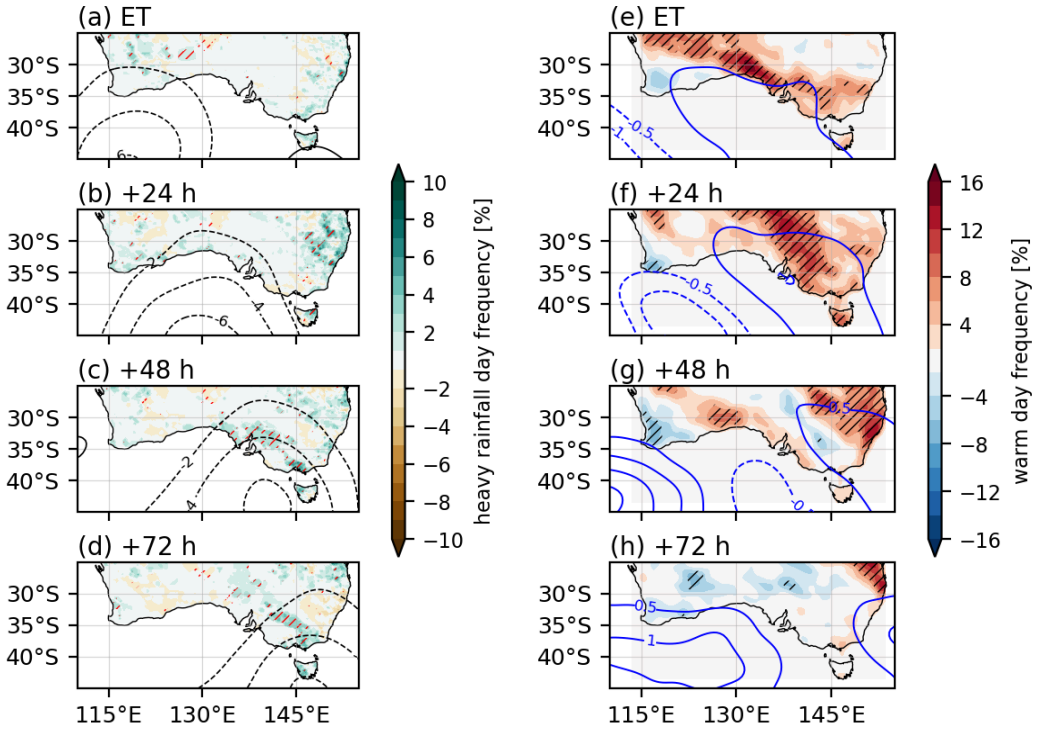


Figure R5. Frequency anomalies of (left) heavy rainfall days and (right) warm days over southern Australia (south of 25S) during and after SWIO C2 ET events. The frequency anomalies are shaded as per the colorbar. Statistically significant anomalies are hatched in red and grey, respectively. MSLP anomaly is 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).

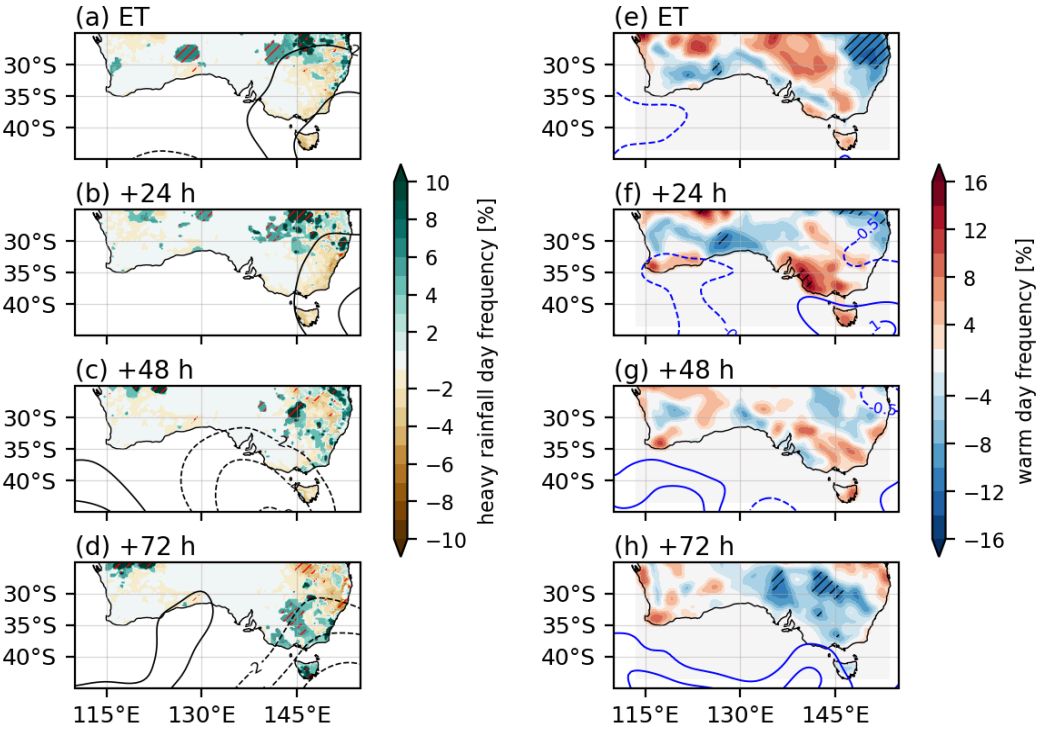


Figure R6. As in Figure R5, but for SWIO C3 ET events.

11. Lines 356-359: this statement is quite general and does not sound so novel. Would it be possible to continue instead on the line of the previous sentence, and keep discussing the differences between C2 and C3 downstream impacts from the Ke perspective?

We have revised the discussion to further explore the differences in downstream impacts between C2 and C3 from the Ke perspective. This can be found in the revised Section 3.3.4.

12. Lines 364-366: I am not fully following the reasoning behind the comparison here. Latent heat release is a different phenomenon than baroclinic conversion and is not part of the eddy kinetic energy framework discussed in the methodology. In terms of conversion of available potential energy into kinetic energy, latent heat release can even act in an opposite direction than baroclinic development (e.g., Federer et al. 2025). Could the authors provide more explanations and rephrase if needed?

The contribution of latent heat release is not part of the Ke framework. We infer this contribution by assuming that QG omega largely underestimates the vertical motion in the region of large latent heating release. As a result, the baroclinic conversion calculated using the QG omega is most likely much weaker than the baroclinic conversion calculated using the ERA5 vertical velocity, as shown by Quinting and Jones (2016) in their Fig. 13. Therefore, the Ke generation associated with baroclinic conversion is more dominated by the vertical motion associated with diabatic processes than the QG-driven ascent.

The argument is rephrased in L535-L539 of the revised manuscript.

13. Section 4 (Discussion): this section is rather long and could be re-organized thematically to give a more logical flow, because the topics follow each other without apparent order. My suggestion would be to “de-structure” this section, putting some of the discussed points in the corresponding Results sub-sections (alleviating the problem of the excessively descriptive style), and the rest in a novel Conclusion section (see next point).

Thank you for this helpful suggestion. We have restructured the Results, Discussion, and Conclusion sections. These revisions are detailed in our response to major comment 5.

14. Section 5 (Conclusions): The part dedicated to the Conclusions is rather small in this section, as it consists only of the first and the last paragraph and the rest is a discussion concerning limitations and of outlook. I would suggest merging the

Discussion and the Conclusion in a single section, possibly with sub-sections (e.g., “Main findings”, “Discussion (including limitations)”, “Outlook”, …).

Thank you for this helpful suggestion. We have restructured the Results, Discussion, and Conclusion sections. These revisions are detailed in our response to major comment 5.

Technical/Grammar/Typos

1. Line 24: “a” ridge-trough couplet?

Changed.

2. Lines 112-114: do the authors mean “features”?

Features indicate weather systems here, such as upper-level trough and lower-level front.

References

Barnes, M. A., Reeder, M. J., and Ndarana, T.: Rossby wave breaking morphologies on the Southern Hemisphere dynamical tropopause, J. Climate, 38, 4825–4844, https://doi.org/10.1175/JCLI-D-24-0461.1, 2025.

Jin, C., A. Ritchie, E., and J. Holbrook, N.: Synoptic climatology of extratropical transition of tropical cyclones over the Southern Hemisphere, Weather and Climate Dynamics, 7, 1285–1306, https://doi.org/10.5194/wcd-7-1285-2026, 2026.

Pinheiro, H. R., Hodges, K. I., Gan, M. A., Ferreira, S. H., and Andrade, K. M.: Contributions of downstream baroclinic development to strong Southern Hemisphere cut-off lows, Q. J. Roy. Meteor. Soc., 148, 214–232, https://doi.org/10.1002/qj.4201, 2022.

Pinheiro, H. R., Hodges, K. I., and Gan, M. A.: Deepening mechanisms of cut-off lows in the Southern Hemisphere and the role of jet streams: insights from eddy kinetic energy analysis, Weather and Climate Dynamics, 5, 881–894, https://doi.org/10.5194/wcd-5-881-2024, 2024.

Quinting, J. F. and Jones, S. C.: On the Impact of Tropical Cyclones on Rossby Wave Packets: A Climatological Perspective, Monthly Weather Review, 144, 2021–2048, https://doi.org/10.1175/MWR-D-14-00298.1, 2016.

Wirth, V., Riemer, M., Chang, E. K. M., and Martius, O.: Rossby Wave Packets on the Midlatitude Waveguide—A Review, Monthly Weather Review, 146, 1965 – 2001, https://doi.org/10.1175/MWR-D-16-0483.1, 2018.