

# Response to Reviewers

CLDY-D-25-00402: Revisiting The Rare Transition of a South Atlantic Cyclone to Tropical Storm Akará: Energy Cycle and Stratosphere-Troposphere Interaction

## Response to Reviewer 1

### Overview

The manuscript is a very welcome complete study of a subtropical cyclone that underwent tropical transition over the South Atlantic Ocean. Its main importance relies on - for the first time, as far as I am concerned - aggregating different analysis tools (such as CPS, Hovmoeller diagrams and Lorenz Energy Cycle) to construct a broader interpretation of physical processes in these complex cyclonic systems. Also, it draws attention to stratospheric intrusions, a mechanism that was not, until now, much considered in subtropical and tropical cyclogenesis over the South Atlantic. The text is very well written, objective and clear. The results and discussions are sound. Thus, I recommend the acceptance of this article. I have just minor comments to improve the document.

### Comments

1. Introduction: Please provide some words about Energetics+cyclones - was the Lorenz Energetic Cycle applied to cyclones' studies before? Which type? What was the objective? In what region(s)? Was LEC used jointly with other tools, or just the Energetics analysis was provided? By doing this you highlight that this work brings - or does not - a really new analysis for tropical transitions (or for South Atlantic, if it had already been done elsewhere).

R: Thank you for the suggestion. We added new paragraphs in the Introduction (L98-110) summarizing prior applications of the Lorenz Energy Cycle (LEC) to cyclone studies, and for heat and vorticity budgets. We also revised the closing paragraph of the Introduction (L143-145) to state that cyclone-centered diagnostics that integrate LEC with phase-averaged heat and vorticity budgets remain scarce for the South Atlantic, thereby clarifying the contribution and novelty of the present study.

2. Line 131: Change "available currently" to "currently available".

R: Thanks for catching that out. We corrected it in the new version (L159).

3. Line 158: Change "are" to "as".

R: Thanks for catching that out. We corrected it in the new version (L186).

4. Line 182: In justifying the domain choice, you can also point out that this domain may be suitable to South Atlantic subtropical cyclones in general, as Gozzo et al (2014) found in the climatology that most of the systems had radius around 500 km.

R: Great suggestion, we revised L210-215 for incorporate it.

5. Figure 2: Maybe the intensification phase color could be changed to not blend into the background SST field?

R: Thank you for the suggestion. We retained the intensification color to preserve a consistent phase palette across Figs. 2-4. To improve legibility over the SST background, we added a contrasting outline to the markers (and increased the marker edge width), which prevents blending. We hope the updated figure is clearer.

6. Line 324: Hart (2003) discussed the problem of data resolution in representing warm cores on the CPS. It was empirically shown that  $1^\circ$  resolution or finer could resolve the upper warm core. So why do these  $0.25^\circ$  resolution data are not able to show a warm core in CPS even when it clearly shows an anomalously warm core in the vertical cross-section? No need to change anything in the article, but a point to reflect on.

R: Thank you for the remark. We believe that the apparent mismatch arises from the diagnostics rather than data spacing. The CPS warm-core metrics are azimuthal means of geopotential-height gradients evaluated over 500 km semicircles and are therefore sensitive to residual baroclinicity and asymmetry within that radius, as well as to the vertical-derivative nature of the thermal-wind measures. By contrast, the vertical cross-section in our Figure 3 follows Reboita et al. (2020), computing a zonal temperature anomaly as the difference between means in a  $2^\circ \times 10^\circ$  box and a surrounding  $2^\circ \times 30^\circ$  box centered on the storm, which enhances the local warm signal and is less affected by azimuthal averaging.

7. Line 324: Change "indicates an symmetric" to "indicates a symmetric".

R: Thanks for catching that out. We corrected it in the new version (L361).

8. Line 395: The increased low level mass convergence may result from the stronger ascent in the presence of the mid level cold cutoff low. The temperature profile is conducive to a convectively unstable environment, enhancing upward motion and convection. This idea is supported by numerous studies of subtropical cyclones (e.g. Reboita et al. 2021). You may consider a comment on this here.

R: Thanks for the suggestion, we included this discussion on L440-445.

9. Figure 7: Consider change the MSLP contours to black, for better visualization in these fields.

R: As requested by another reviewer, the former Fig. 7 has been incorporated into the new Figs. 5, 6, 11, 12, and 13. We updated the MSLP contours to gray to improve contrast and readability against the background fields.

10. Line 448: Change "cool" to "cooler".

R: Thanks for catching that out. We corrected it in the new version (L505).

11. Line 452: Subsidence shows up in Figure 11 as warming. If I get it right, the significant signal occurs only on Feb 18-19. I do not see a significant signal on Feb 17, and near 10 hPa (could you mean 100 hPa?). I suggest correct the text to highlight the warming signal on Feb 18-19, or - if I got it wrong - describe/show more clearly the subsidence near 10 hPa on Feb 17.

R: You are absolutely right, thanks for catching that out. We corrected it in the new version (L510-518).

12. Line 488: In "approximately 300 hPa", are you referring to the warming nucleus during the mature phase, on Feb 20, correct? If so I think the propagation should be "to approximately 200 hPa".

R: Thanks for catching that out. We corrected it in the new version (L565-566).

13. Line 488: I suggest to remove the word "Consequently". As it is, it seems that the insufficient surface fluxes are a consequence of the stratospheric intrusion, which is obviously not the case.

R: Thanks for the suggestion. We altered it in the new version (L565-570).

14. Line 539: Is it the BAe term? In the mature phase its value was 0.26, and now in the decaying phase it is -0.27. It seems to have become negative, contrary to what the text says.

R: We meant BAz term. We corrected it into the new vewsion (L617-618).

15. Line 543: Refer to Figure 11.

R: Thanks for the suggestion. We altered it in the new version (L62).

16. Line 553: Change "typically" to "traditionally".

R: We altered it in the new version (L632).

# Response to Reviewer 2

## Overview

This study presents a detailed dynamical and thermodynamical analysis of a rare case of a subtropical cyclone that transitions into a tropical storm, building on its previous documentation by Reboita et al (2024). I think that this is an important analysis that sheds some light into the underlying process that characterized this rare event. I think that such a study is appropriate for Climate Dynamics.

I however have some comments that warrants major revisions to the paper and I do hope that they will be taken in the constructive spirit in which they are suggested.

First, I would encourage the authors to consider English editing, and I say this as a Second Language English speaker myself as a reviewer.

R: We appreciate the reviewer's assessment of the manuscript's relevance and potential for publication. Regarding language, we have undertaken a thorough English revision of the entire text, incorporating the specific corrections suggested by the reviewers and performing additional proofreading for grammar and clarity. We believe these changes have improved readability and precision.

## Major comments

1. As interesting this paper may be, I found it rather hard to follow. My confusion is caused by the fact that the sequence in which the figures are presented does not always follow the flow of the discussion. To illustrate the point being made in this comment: In Section 3.1 for instance, the authors consider Figures 4, then 3, 5a, 6b (without saying anything about 6a and what it means for 6b and how it links to 5a), 7a and 7b and then jumps to 9 and then back to 8a and jumps again to 11. I hope that the authors can see how confusing this is for one as a reader. The paper really needs to be reorganized and so this comment constitutes major revision.

As a reader I would urge the authors to present the figures in the order in which the sequence of events are discussed. May I suggest two options

Option 1: If the authors prefer keeping the plots as they are then I suggest that the figures be discussed in their entirety from start to finish and then the next one etc. this means that the paper will have to be divided according to the figures or a combination of figures rather than as it is done presently (in terms of stage of development of the cyclone), so that the stages of the events will have to be embedded in the text in each section. Then the authors would have to consider a Discussion section that will tie everything together and that may then be followed by a Conclusion section

Option 2: Divide the text, as it stands, but then bring all the panels that correspond to the various stages of development in one place. For instance, all panels that correspond to the Incipient Stage (Section 3.1) from 5, 6, 7, 8, 9 (see next comment about the diagnostics used here) and 10 could be brought together somehow as either a combination of plots or something that are presented in a consistent manner in which the analysis is presented.

Clearly, Fig 2, 3, 4 and 11 cannot be separated but the issues treated could be considered as overarching and presented as leading issues before the separation into the various sections.

Personally, I would prefer Option 1 because it would be easier to deal with this issue or the authors may find another way of dealing with this, but as things stand, I do think this is ideal.

R: We appreciate the detailed guidance on figure flow. We adopted Option 2 to preserve the phase-based, chronological narrative of the cyclone's evolution while improving readability. Concretely, we reorganized the figure set so that, within each lifecycle stage subsection, the relevant snapshots appear together: the former Figs. 5–7 are now grouped into composites (new Figs. 5, 6, 11, 12, 13, and 14) showing, respectively, (a) GOES-16 Ch. 13 brightness temperature with 1000-hPa winds, (b) SST–T2m with MSLP, and (c) 500-hPa geopotential height with (the newly added) 200-hPa PVU. We did not embed the heat, vorticity, and LEC figures into the snapshot composites because those diagnostics are phase-averaged (not instantaneous) and often span multiple snapshots within a stage. Instead, we reference them consistently during each stage subsection and added explicit cross-references to guide the reader. We also have revised so that each Figure is referred in the order they appear on the text for avoiding confusion and made sure that each Figure is properly referred on the text. We hope this restructuring clarifies the flow and makes the analysis easier to follow.

2. The second major comment has to do with the energetics framework used in this study. I realize that the analysis toolbox used here was developed by the lead author (this is quite commendable). However, I went and look at the JOSS paper that documents this software and I see no explanation of the LEC derivation issues. The authors may challenge this if they will, but, as useful as the LEC Toolkit may be in certain circumstances, I do not think that it is appropriate for use in weather system dynamics. It is not clear to me how the basic state was defined. If for instance you define a time symmetric basic state flow (as in Murakami 2011, Murakami et al 2011) so that you obtain a lat-lon distribution of the energy terms and conversion processes. The box plot that results are much more involved as it makes explicit the interaction component of kinetic energy and it is more complicated than Figure 1 (and the interpretations shown in Figure 9). What makes Fig 1 simple and easy to interpret is the fact that a zonally averaged basic state flow that assumes cyclicity from 0 to 360 longitude degrees, which allows certain terms to vanish when we take the zonal average. Even if one were to

consider a single wave component of an idealized baroclinic wave, cyclicity would still be a critical factor to come up with is a simple box plot such as presented in Figure 1 (see Holton's Introduction to Dynamic Meteorology and other texts that treat the two-layer model energetics). So how is cyclicity considered in this study so that the considered here so that the LEC is applicable to this case?

I am of the opinion that the LEC is not an appropriate energy frame work to use for weather systems because the cyclicity removes the ageostrophic geopotential flux divergence completely that accounts for downstream energy transfer, particularly as a COL is involved aloft. I invite the authors to dispute this if they will and suggest that they please consider the Orlanski and Katzfey (1991) framework, and a 31 day mean basic state. However, they will lose the box plot but will gain much deeper insights into the energetics of the systems. For these reasons, I am of the opinion that the analysis presented in Lines 367 - 385, 428 - 437, 533 - 543, as well as the use of Figure 9 and associated conclusions should be revisited using a more appropriate framework that makes the correct assumptions for the problem at hand.

The issues raised here constitute a major comment as this is central to the paper as suggested by the title.

R: We appreciate the reviewer's careful evaluation of the energetics framework. First, to avoid any misunderstanding: the first author developed the software implementation (the paper on the JOSS), not a new theory. Our analysis does not introduce a new LEC formulation; rather, it applies the limited-area Lorenz Energy Cycle following standard derivations used in cyclone studies (e.g., Brennan & Vincent, 1980; Michaelides, 1987) within a semi-Lagrangian, storm-following domain (Michaelides et al., 1999). The complete working equations, the eddy-mean decomposition, and the treatment of boundary terms employed here are documented in our EarthArXiv preprint (which has also been submitted to Climate Dynamics and already passed the first round of reviews), now cited explicitly in the Methods (lines L207-209) to avoid expanding the manuscript. In this paper we apply the established LEC equations and the semi-Lagrangian framework as documented in that preprint.

Regarding the basic state and "cyclicity," we do not assume a zonally periodic 0–360° framework. In the limited-area LEC used here, the eddy/mean separation is defined by longitudinal averaging over the moving domain, and non-periodicity is handled explicitly through lateral energy-flux and pressure-work (geopotential) boundary terms. These terms, their magnitudes, and implications are computed and discussed in the preprint. We also comment there on a possible relationship between these boundary terms and downstream development; this remains speculative, and we explicitly suggest further investigation. We now clarify these points in the Methods (lines L729-742) and direct readers to the preprint for the full mathematical expressions.

On suitability, we acknowledge the limitations of the limited-area LEC, particularly its limitations on representing the downstream development. However, this formulation has

been employed in numerous cyclone studies across basins and regimes (see Introduction lines L98-110), and in our case it yields physically coherent results consistent with the literature and with our other diagnostics. We have added text in the Discussion (lines L729-742) explicitly stating this limitation and noting that alternative frameworks (e.g., Orlanski & Katzfey, 1991–type energetics with a time-mean basic state) provide complementary insight into downstream development.

3. The third major issue that I, as a reviewer of the manuscript, is that the STI issue has been inadequately addressed and some of the suggestion made about this matter are not clearly shown by the diagnostics. For instance in ;line 450 to 451 the authors make the bold statement that the vertical motion presented in Figure 11 is evidence of stratospheric intrusion. I do not clearly see how that would be the case if the tropopause is not even defined. How do the authors quantitatively know this? So I would like to make the suggestion that you consider PV diagnostics to assess this. I also think that this intrusion can be addressed as early as in Figure 6 by the inclusion of PV = -2 PVU at the 350K or 330 K isentropic surface, for instance.

Figure 8 should include PV = -2 PVU and PV anomalies to try and illustrate the intrusions that the authors claim they see. This constitute a major comment because this is issue is highlighted as a focus together with the energetics in the title.

R: Thank you for the insightful suggestion. We acknowledge that a dynamic-tropopause depiction was missing. We have now added PV overlays to the reorganized figure set: PV contours are included at 200 hPa in the new Figs. 5, 6, 11, 12, and 13. In addition, we provide a new Supplementary Fig. S1 showing the cyclone's vertical structure with PV contours (-2 to -4 PVU) and vertical velocity (shaded), which makes the stratospheric intrusions explicit in space and time. We also revised the text (lines L510-518) to clarify that the vertical-motion signal is consistent with, and now corroborated by, the PV diagnostics rather than inferred solely from  $\omega$ .

## Minor comments

4. Please fix the latex code in the abstract to  $28^{\circ}\text{C}$

R: Thanks for catching that out. We corrected it in the new version (L21).

5. Lines 302 - 312: Whilst I take the point that the authors make about the fact that Reboita et al (2024) has detailed this case but may the authors please include the dates that defined the transitions, as shown in Figure 4 and also include these dates somehow in Figure 3. This will help the reader a great deal.

R: Thank you for the suggestion. We added the explicit transition dates in the lines L342-345 to mark the cyclone phases. In Figure 3, the x-axis shared by both panels already displays all analysis timestamps (dates and UTC hours), enabling readers to follow the transitions directly.

6. Please fix degree Celcius in the Fig 3 caption



R: Thanks for catching that out. We corrected it in the new version.

7. Lines 331 - 333: How does the CPS indicate this, please elaborate.

R: We have added L373-376 elaborating (briefly) on how CPS parameters indicate the systems thermal structure and symmetry.

8. Line 354 and Figure 8: Please be consistent in labels of your terms in the vorticity equations and reference to them in the text and in the figure that represent them (in Figure 8 whilst we are talking about Figure 8 (and figure 10) please explain in the caption how these terms were averaged so that the reader does not have to go back to the methodology section to try and figure that out). So which of the terms in the vorticity equation is the ZD terms (I mean I know what it is but what if your reader is really familiar with dynamic meteorology concepts, they will not know which one is the stretching term). Please try to be consistent with that so that your paper is easier to read.

R: Thank you. We standardized the notation across the manuscript and figures.

9. Figure 7: the contours are simply not visible, please use black or something.

R: In the reorganized Figs. 5, 6, 11, 12, and 13, we updated the contours to a high-contrast gray and increased their line weight to improve visibility against the background fields.

10. Figure 11: How were the Hovmöller plots produced, please include that in the caption.

R: Thanks for the suggestion, we have included that on the caption (Figure 10 in the new version).

## Response to Reviewer 3

### Overview

This study analyzes the life cycle of Cyclone Akara in the South Atlantic in February 2022. Akara was the third documented tropical cyclone in this region, and formed through a tropical transition process. The authors describe the life-cycle phases and cyclone type by utilizing objective methods, and examine its dynamics and thermodynamics quantitatively based on energy budget, vorticity budget and heat budget analyses. I appreciate that this case is important for understanding tropical and subtropical cyclones in this region, and that the quantitative analyses help in interpreting its formation mechanisms. However, I have a concern about the energy budget analysis (LEC) in this study. Whereas the energy budget analysis provides a clear picture of cyclone processes, the analysis is sometimes difficult to apply to real cases. I think that the authors should explain its applicability to this case more carefully and interpret its results from the perspective of cyclone structures in more detail. I will explain this



point in detail in my comment #1. I also make several minor comments on the analyses and explanations. Therefore, I recommend major revision of this manuscript, although I think it is worth publishing in Climate Dynamics.

## Major comment

1. I think that the energy budget analysis around a cyclone is sometimes more difficult to examine than vorticity and heat budget analyses because it requires determining mean and eddy fields. The size and orientation (not necessarily zonal) of the domain should be determined so that the eddy and mean fields appropriately represent the cyclone and its environment, respectively. This study examines a 5-degree square domain centered on the cyclone's central position. This domain size appears to cover only a part of the cyclone structure (e.g., Fig. 5), which may be insufficient for energy budget analysis though sufficient for vorticity and heat budget analyses. I understand that it is generally difficult to determine the domain of the energy budget analysis because the surrounding conditions are not simple in the real atmosphere as mentioned in the manuscript. Even so, the result of the analysis should be validated and discussed more carefully.

For example, the analysis shows that the conversion from eddy APE to eddy kinetic energy is dominant in intensification and mature phase of the cyclone. This result is interesting because a tropical cyclone is generally considered to develop through diabatic heating. This conversion is attributed to the barotropic conversion in horizontal wind shear between the southwesterly flow from the post-frontal high and the northeasterly flow from the SASH in section 3.2. My concern is that these flows appear to be associated with the cyclone structure rather than the above-mentioned synoptic systems. In addition, if the horizontal flow in the shear are oriented in the northeast-southwest direction, is it appropriate to calculate the mean field in the zonal direction? Therefore, I recommend discussing the structures of zonal-mean and eddy fields that are responsible for the barotropic conversion.

In summary, the authors should validate the design of the energy budget analysis more carefully and interpret the results in more detail. If necessary, the authors may improve the design of the analysis.

R: We thank the reviewer for the careful assessment and constructive suggestions.

Regarding domain selection, our intent was to isolate the cyclone's structure and suppress contamination from neighboring synoptic systems (as stated on L210-221). A larger domain would have aliased the frontal region and continental convection (strongly affected by local thermodynamics and topography) into the energetics, especially during the early stage when the cutoff low was not yet coupled to the surface vortex. As discussed in the manuscript, Akará progressively detaches from the frontal structure; a larger domain would therefore bias the later-stage energetics toward baroclinic

conversions associated with the front rather than the cyclone itself. Moreover, as pointed by another reviewer, the chosen  $5^\circ \times 5^\circ$  extent is consistent with the typical scale of South Atlantic subtropical cyclones (radii near 500 km reported by Gozzo et al., 2014), which supports its suitability for this case (L212-214). We agree that a systematic sensitivity analysis to domain size is valuable; we note this as a direction for future work.

Concerning the dominance of barotropic conversions, we interpret this within the context of published evidence that barotropic instability contributes to the initial organization of tropical disturbances, e.g., ITCZ breakdown and African easterly waves (L704-719). It has also been identified in subtropical and transitioning systems using LEC-based diagnostics. We have expanded the Introduction to reference these applications (L98-210), thereby facilitating comparison and underscoring that LEC is an established framework for cyclone energetics in the South Atlantic and elsewhere.

On the question of mean/eddy definitions and flow orientation, the barotropic conversion term CK in our formulation comprises multiple subterms and the wind components are both considered as zonal deviations and as deviations from the area mean. We now cite our preprint (L207-209) where the full expressions and derivation are presented, and where the treatment of lateral fluxes and pressure–work terms is detailed. For completeness, the mathematical expression and discussion of the CK term, the subterms sensitivity to mean/eddy definitions, and conditions under which they vanish are also provided in the first author's Ph.D. thesis (available at: <https://www.teses.usp.br/teses/disponiveis/14/14133/tde-17102024-150703/en.php>). We include this reference for transparency and acknowledge that the reviewer's concern regarding flow orientation and mean/eddy partitions is completely valid. We added cross-reference to the pre-print in the manuscript to guide readers (L207-209).

## Minor comments

2. Line 21: there is a typo in parentheses.  
R: Thanks for pointing that out. We have fixed it on the revised version (L21).
3. Line 144 and other parts: I think "zonal-mean component" is a more precise expression than "zonal component."  
R: We have modified that in the new version (L172).
4. Line 158: The sentence including "is usually referred are" is not grammatical.  
R: Thanks for noticing it. We have corrected it to "is usually referred as" (L186)
5. Figure 1 and other parts: Make the upper/lower case letters in the LEC terms consistent throughout the manuscript; e.g. "CE" or "Ce."  
R: We have updated Figure 1 and standardized the notation across the manuscript.

6. Lines 206-207: More accurately, term (C) indicates vertical advection in addition to adiabatic expansion/compression.  
R: Thanks for the correction, we adjusted it into the new version (L239-241).
7. Equation (11): Is term (G) correct? The tilting term generally includes the horizontal gradient of vertical wind rather than the horizontal gradient of relative vorticity.  
R: Thanks for noticing that, it was a typo. We have corrected it in the revised version.
8. Equation (11): Replace "+" with "-" in the equation.  
R: Thanks for noticing that, it was a typo. We have corrected it in the revised version.
9. Equation (11): I think "h" should be added to the nabla in term (E) and (F).  
R: Thanks for noticing that. We have corrected it in the revised version.
10. Lines 304-306: This sentence is confusing. It says that the cyclone classification is based on Reboita et al. (2024). On the other hand, section 2.2.3 mention that the cyclone types are determined based on the Cyclone Phase Space. What is the relationship between the Cyclone Phase Space and Reboita's classification.  
R: Indeed, the original phrasing could be somewhat confusing. We have removed the reference to Reboita's classification to improve clarity (L337-338).
11. Lines 325-326: Where is the deep convection in the frontal region in Fig. 5a?  
R: In Fig. 5a, the deep convection within the frontal band is located near 25°S between 50°W and 40°W (east of the continent), as indicated by GOES-16 Channel-13 brightness temperatures locally below -30 °C in some parts and even below -50 °C in others. Although this cluster is on average weaker than the convection farther south (25–30°S, 40–35°W), the zonal averaging used for the temperature anomaly (computed over ocean longitudes only) emphasizes the warming associated with this frontal convection because there is little to no convection to its east. We have revised the text for clarity (L363-365).
12. Figure 3 bottom: What is the "zonal mean temperature deviation near the cyclone center" in the caption? Does it mean the temperature deviation at the cyclone center from the zonal mean (5 degrees)?  
R: We follow Reboita et al. (2021, 2024). At each time and level, we compute the mean temperature in a 2° lat × 10° lon box centered on the storm and subtract it from the mean in a 2° × 30° box centered on the same point. This "zonal temperature deviation near the cyclone center" is therefore not a deviation from a basinwide zonal mean. We have added this definition to the Fig. 3 caption for clarity.
13. Line 338-340: Why is the reduced stability by surface heat fluxes evidenced by the horizontal advection? The two processes does not seem to be related directly.  
R: The wording could imply a causal link that was not intended. Our point was to present two independent diagnostics: (i) reduced boundary-layer stability inferred from the

SST–2m air temperature contrast and associated surface heat fluxes, and (ii) upward motion inferred from the vertical increase of cyclonic vorticity advection consistent with the quasi-geostrophic omega equation. We have revised the text to decouple these statements and avoid suggesting direct causality (L383–387).

14. Lines 354 and 360: It would be helpful if the descriptions of acronyms "ZD" and "FD" are moved to the explanation of equation (11) in section 2.2.2.

R: We have moved the definitions of ZD and FD to Section 2.2.2 alongside Eq. (11) (lines L255–257), as suggested. We also retained a brief reminder at their first occurrence in Section 3 to aid the reader (L399 and L345).

15. Figure 5: Is "wind barbel" in the caption a typo?

R: Thanks for noticing that, it was a typo. We have corrected it in the revised version and now, as requested by another reviewer, the Figure was split into Figures 5, 6, 11, 12, 13 and 14.

16. Figure 5: Explain what the green X marks indicate.

R: It is the system position at each time for each panel on the figure. We have clarified that in the caption. Actually, it is now a black X and appears on Figures 5, 6, 11, 12, 13 and 14 (see response to comment 15).

17. Figure 6: Give the unit of geopotential height.

R: Thanks noticing that this was missing, we added it into the new version - now Figures 5, 6, 11, 12, 13 and 14.

18. Line 452: 10 hPa is not shown in Figure 11. Is this 100 hPa?

R: You are absolutely right, thanks for catching that out. We corrected it in the new version (L512–513).

19. Line 452: It would be helpful to note the time when the subsidence reached down to approximately 200 hPa.

R: Thank you. We corrected a typo: the subsidence begins late on 18 February. We now also specify when it deepens, reaching approximately 250 hPa between 12–18 UTC on 19 February (lines L511–513). The earlier reference to ~200 hPa was imprecise; the revised text reports the diagnosed depth (~250 hPa) and the corresponding time window.

20. Lines 453–454: The authors mention that the process intensifies the mid-upper-level low, and increase divergence at the upper levels. From the perspective of geostrophic balance, how is the intensification of low linked to divergence?

R: We thank the reviewer for the insightful comment. From a quasigeostrophic (QG) perspective, a vertically aligned cutoff low with the surface cyclone does not, by itself, favor ascent at the cyclone center. After tropical transition, however, the flow departs from QG balance and the dynamics are dominated by nonlinear, ageostrophic circulations. In this regime, the cutoff contributes to reduced vertical wind shear, which

helps organize convection. While subsidence occurs in the tropical-cyclone eye, ascent is concentrated in the eyewall (Supplementary Fig. S1b,c). We have revised the text accordingly (lines L519-532) to clarify that the intensification–divergence linkage arises from ageostrophic outflow aloft over the eyewall, rather than from strictly geostrophic balance.

21. Figure 11: It would be helpful if the boundary between the red and blue shadings is zero.

R: We have updated the Figure (now, Figure 10).

22. Lines 584-589: The authors mention that stratospheric air intrusions intensified the mid-upper-level cyclonic circulation, providing dynamical support for convective activity. The latter part may indicate that the stratospheric air intrusion is linked to horizontal advection of vorticity, while it is also related to subsidence in section 3.2. What is the three-dimensional motion of the stratospheric air intrusion?

R: Thank you for raising this point. Our aim was not to resolve the full three-dimensional pathways of the intrusions. We have revised the text to interpret our diagnostics within a balanced PV framework: upper-level high-PV air crossing the tropopause supports surface pressure falls and low-level cyclonic spin-up via PV inversion; subsequent diabatic processes redistribute PV vertically, fostering vertical coherence and intensification (L666-680).