We sincerely thank the reviewers for their constructive comments and rigorous evaluation of our manuscript. Their insights have significantly improved the robustness of our dynamic and thermodynamic analyses. Below, we provide a point-by-point response detailing how we have addressed each concern in the revised manuscript.

RC1: 'Comment on egusphere-2026-1446', Anonymous Referee #1, 12 May 2026

The study investigates the tropical transition of Hurricane Ophelia, focusing on the secondary circulation evolution. While I agree there is a gap in process-based studies of tropical transition at convection-allowing resolution, I have major concerns about the analysis and interpretation that lead me to recommend the study be rejected for publication. My most major concerns are summarized below. I hope the authors find these comments constructive to improve their investigation and presentation of results.

Choice of tropical-transition period. The simulation is initialized on 00 UTC 9 Oct 2017, six hours before Ophelia was designated "tropical" in IBTrACS. It is curious that the authors define the tropical transition period between ~13 UTC 9 Oct 2017 through ~13 UTC 10 Oct 2017. First, there isn't any objective reasoning or justification as to why this period was chosen.

We thank the reviewer for highlighting the need for an objective definition of the tropical transition (TT) period. In the revised manuscript, we have introduced an objective methodology to define the exact timing of the TT in the simulation using Cyclone Phase Space (CPS) diagrams, following the framework of Hart (2003). The TT period is now objectively defined as the ± 12 h window during which the cyclone crosses the $-V_T^U = 0$ axis in the Core Depth diagram, marking the exact structural transition from a cold upper core to a warm upper core extending through the full tropospheric Depth. To further validate this timing and address the concern and uncertainty that our analysis captures mere intensification, we incorporated the potential vorticity (PV) metrics of Bentley et al. (2016) shown in the new Fig. A3.

Second, given the evidence presented, I would argue that much of this period is when Ophelia is tropical and what is investigated is more tropical cyclone intensification, rather than the true tropical transition period itself. Thus, what the authors have mostly discovered is the strengthening of the secondary circulation associated with intensification, which is not a novel finding.

We thank the reviewer for this important point. The new simulation starts on 07 OCT 2017 at 00:00 UTC, whereas the old version started on 09 OCT 2017 at 00:00 UTC. By starting 48 hours earlier, the new version captures the "pre-transition baroclinic stage" as an active cold-core low, due to the old simulation initialization time started too late and missed the previous transition processes. By extending the timeline backwards, the new version reveals a distinct "structural relaxation" or spindown phase *prior* to the TT. The manuscript currently highlights that $S(t)$ reaches a minimum before convection takes over, proving the transition is highly non-linear and distinct from standard tropical cyclone intensification.

Baroclinic support versus convective processes. In several places in the manuscript, the authors argue that there is a transition from baroclinic to convective support/processes. I agree that, conceptually, this is the crux of the tropical-transition process, but I do not think the authors present any convincing evidence of this transition. Specifically, how is "baroclinic support" quantified in this framework, and how is it tied to S(t)? Wouldn't S(t) increase for any cyclone with a thermally direct circulation, tropical or non-tropical, that is undergoing cyclogenesis or intensifying?

Rather than the secondary circulation as the central metric, I think a PV framework would be a more promising avenue. The authors touch upon the relevance of PV redistribution themselves. I recommend looking at Bentley et al. (2016)[https://doi.org/10.1175/MWR-D-15-0251.1]. One idea could be to adapt their framework to analyze the high-resolution model simulation of Ophelia.

Thank you for these points to improve the quality of the manuscript. We agree that the transition from baroclinic to convective support is the crux of the TT process. While our idealization of a nearly axisymmetric cyclone inherently differs from a fundamentally asymmetric baroclinic cyclone, the combination of our secondary circulation metrics with the suggested PV framework robustly captures this transition. Following your recommendation, we adapted the Bentley et al. (2016) PV framework to analyze the high-resolution simulation (Fig.A3). Our secondary circulation framework complements this PV analysis by explicitly resolving the diabatic and mechanical forcings contributing to those PV perturbations in a highly localized, storm-centered framework. Additionally, we have added dry potential temperature (θ) contours to the cross-sections in Fig. 5. These dry isentropes visually confirm the shift from an asymmetric cold-core system to a symmetric warm-core structure, providing clear thermodynamic evidence of the transition independent of S(t).

Uneven presentation of findings. There are numerous instances in the manuscript where I have trouble following the authors or am skeptical of the results.

A general criticism is that there are multiple instances of unsubstantiated claims where no evidence is provided or findings stated before evidence is presented, which results in confusion.

Budget closures are not assessed (e.g., Figs. 6 and 8) by showing the residual between the sum of forcing terms and actual tendencies. I appreciate budgets calculated from model output are challenging to close, especially thermodynamic ones, but it's important to be transparent here. From my eye, it seems like the residual could be large, which implies there is uncertainty in interpreting individual terms.

We thank the reviewer for this helpful recommendation. We have thoroughly reviewed the manuscript to ensure all claims are substantiated by evidence prior to interpretation. We have added a new supplementary figure (Fig. A4) that now fully represents the budget closure for Fig. 6. This new figure displays both the absolute magnitudes and the fractional contributions of all terms, explicitly including the residual term. Moreover, in Fig. 8, we have included the residual term for transparency, allowing readers to account for differences in the computations. We have also added a discussion of the numerical residuals to the revised manuscript.

Something also seems off in the budget presented in Fig. 6. Why wouldn't the frictional term continue to increase in magnitude as the TC intensifies? Early in the defined TT period, the thermal forcing (red line) never gets larger than the blue line (dynamical forcing), and the increase in the frictional term seems important for the bump up in S(t). Why?

We have corrected the caption for Fig.6 to specify that it depicts the negative of the frictional term, which clarifies the graphical representation. The initial bump in S(t) at the beginning of the transition represents a transient response to non-symmetric convective bursts. The key finding remains that the centrifugal and thermal forcings balance and rise at nearly the same rate once TT begins. To fully address the reviewer's concern and provide a clearer picture of the budget dynamics, we have added a new supplementary figure (Fig. A4) in the revised manuscript. This new figure explicitly shows both the absolute magnitudes of all budget terms and their fractional contributions to the total budget, importantly including the residual term.

Some of the visualizations and variable choices are either confusing or not well explained/justified. Some examples:

- 7 is confusing to interpret. Going back to my first comment, I feel just shows intensification.
- In Fig. 11a, 14-km height is above the outflow layer of the TC (see Fig. 5).
- CAPE is not well tied to the diagnostic framework, so seems to come out of nowhere in Fig. 11c.

Thanks for your suggestions. We agree with the reviewer that 14 km was too high and above the primary outflow layer. Based on the maximum wind divergence observed in the new cross-sections (Figs. 5d, f), we have lowered the analysis level for the radial wind in Fig.11a to 12 km. (Lines 352–356)
We have better integrated CAPE into our diagnostic framework, retrieving directly from the model output. We clarify that the progressive inward erosion of CAPE is consistent with eyewall convection operating under conditions of near moist neutrality (Emanuel, 1986), capturing the shift from a broad baroclinically-driven unstable environment to a concentrated, thermally-driven inner core. (Lines 374–377)

RC2: 'Comment on egusphere-2026-1446', Anonymous Referee #2, 13 May 2026 reply

This study explores the dynamics of the secondary circulation associated with tropical transition (TT). While the analyses used in this study appear to be more suitable for diagnosing the dynamics of axisymmetric tropical cyclones than that of asymmetric baroclinic systems, they show some interesting dynamical differences among the stages before, during, and after TT. In particular, the analyses show changes in the intensification rate, the radius of maximum wind, and the structures and dynamics of the secondary circulation. However, I have several concerns regarding the current manuscript as shown below, and I am not confident that the interpretations and conclusions presented in the manuscript are appropriate. Therefore, I will be able to review this manuscript once the following issues particularly raised in my comments from 1 to 4 are addressed sufficiently. At this time, I recommend major revision of this manuscript.

Thank you for your time and effort in this insightful revision in our manuscript. Your comments have been very useful in improving the paper.

Specific comments

1. It is unclear how well the WRF-ARW simulation agrees with the observations and the best track analysis. For example, are the differences in intensification rate between the stages in Fig. 4 found in the best track? The authors should also provide a more detailed explanation of the degree to which the timing and structures of cloud pattern are similar between the observation and the simulation (Figs. 3, 5).

First, to validate the cyclone's trajectory and intensification, we have included a direct comparison against the National Hurricane Center best track dataset. We added a new Fig.(Fig. S1) showing the track agreement, where the simulation successfully captures Ophelia's quasi-stationary cyclonic looping. Indeed, we have computed the Cyclone Phase Space (Hart, 2003) and the Bentley et al. (2016) Potential Vorticity (PV) framework (Fig.A2, A3) to objectively anchor the simulation's structural transition to established metrics, which resolves discrepancies between best track analysis timing and internal model physics.

2. The authors should explain the analyses in more detail. Regarding section 2.2, it would be easier to understand if the authors elaborated on the physical meanings of R and H where they first appear. In addition, when this analysis is applied to the output of the simulation, what values are used for R and H? Are horizontal winds at z=0 directly obtained from the model output? Are the assumptions regarding wind and pressure at the boundaries reasonable in the model output?

Thank you for these points to clarify what means R and H. We have elaborated on the physical definitions of our budget variables in Section 2.2. Specifically, we clarify that R denotes the maximum radius of the system and H denotes the system height bounding the integration volume.

3. While this study focuses on TT processes, the roles of baroclinicity and a cut-off low are not clear. If the analyses of the secondary circulation indicate any influences of the cut-off low, it would be beneficial to discuss that point. As the authors mention baroclinicity many times, I also recommend adding an analysis of baroclinicity that demonstrates the differences among the stages. The secondary circulation is not directly linked to baroclinicity itself, although it can be affected by baroclinicity.

Thank you for this point in improving the paper. The new version introduces the Bentley et al. (2016) PV framework (new Fig.A3) to explicitly quantify these forcings. It tracks **PV3**, which explicitly isolates the upper-tropospheric PV anomaly associated with the overarching cut-off low/trough, proving it dominates the pre-transition phase. It tracks **PV1**, which directly quantifies lower-tropospheric baroclinicity (near-surface potential temperature anomaly gradients). The analysis proves that PV1 remains near zero, confirming the absence of strong low-level frontal thermal asymmetry and highlighting that the baroclinic forcing was primarily aloft. To further analyze baroclinicity across the stages, the new version incorporates CPS diagrams (new Fig.A2). This plots the lower-tropospheric thermal asymmetry parameter (B), demonstrating that the system maintained a persistently symmetric, non-frontal structure in the lower levels from early in its lifecycle, while simultaneously tracking the upper-level transition from a cold core to a warm core.

In the old manuscript, the pre-transition behavior of $S(t)$ was largely ignored because the simulation started too late. By extending the simulation 48 hours earlier, the new version captures an initial prominent peak in $S(t)$. The text now explicitly links this to the cut-off low, interpreting this early secondary circulation peak as "a period of active baroclinic development, where the cold-core system likely extracts available potential energy via interactions with a transient upper-level trough and strong temperature gradients". Indeed, the new version adds dry potential temperature (θ) contours to the cross-sections in Fig.5. These isentropes allow for a direct visual analysis of the atmospheric baroclinicity, clearly showing the initial highly-sheared cold core (driven by the cut-off low) bowing inward and warming as the secondary circulation redistributes diabatic heat.

4. It would be easier to read if the description of what the Fig.shows were distinguished from the explanation of how it can be interpreted. I will elaborate on this point later in relation to Figs. 2, 3, and 5.

Thanks for this point. We have separated the descriptions of the Figs. from their dynamic interpretations, particularly in Section 3.1.1.

5. Line 109-110: The Coriolis force is also included. Done

6. Equation (12): I think the third equation should be divided by 2π.

Thank you for your observation. However, we think it should be multiplied by 2pi:

$$\langle(\ldots)\rangle = \frac{1}{2\pi}\int_0^{2\pi}(\ldots)d\phi$$

$$\frac{2}{r_e^2 H}\int_0^{r_e}\int_0^{H}\left(\int_0^{2\pi}\tilde{\theta}\,d\phi\right)rdrdz = \frac{2\cdot 2\pi}{r_e^2 H}\int_0^{r_e}\int_0^{H}\langle\tilde{\theta}\rangle\, rdrdz$$

7. Fig.1: What does θ denote?

Thank you for this point to clarify in the manuscript. The symbol in Fig. 1 is $\dot{\theta}$, which denotes the material derivative of potential temperature with respect to time ($d\theta/dt$). This represents the diabatic heating rate occurring within the eyewall. It is determined previous to the Fig. 1, in the mathematical derivation.

8. Caption of Fig. 1: It is somewhat strange to explain that arrows denote mixing ratio.

Thanks. The caption for Fig.1 has been corrected to state that green arrows denote "air-sea enthalpy fluxes" rather than "mixing ratio".

9. Lines 149-150: What does "catalyst" mean? It sounds as if a cut-ff low is unchanged during TT processes.

The term "catalyst" regarding the cut-off low has been replaced with the more physically accurate "forcing mechanism".

10. Lines 162-180: This paragraph is difficult to read because the description of Fig. 2 and the explanation of previous studies are mixed together.

It has been rewritten to clarify.

11. Lines 166-168: It is not clear which characteristics in this description can be identified in Fig. 2.

Thanks for this point. We have rewritten the paragraph to improve the readability and understanding.

12. Line 177: When is the TT time (t0), and how is it defined? Why do the authors use relative time for the observations and actual time for the simulation? This makes it difficult to compare the two.

We thank the reviewer for pointing out this confusing discrepancy. We agree that the temporal framing in the original manuscript made it difficult to compare the observed environment with the simulated physics. We have addressed this by strictly defining both timelines and explaining their relationship in the revised text.

For the synoptic and satellite analyses (Figs. 2 and 3), t_0 represents the observed time of tropical transition as documented in the HURDAT database. We used relative time ($t_0 - 24$h, t_0, $t_0 + 24$h) here to illustrate the large-scale atmospheric evolution framing the event, providing a general synoptic context independent of our model's specific timing. We have updated the text to explicitly state the actual UTC timestamp for t_0 to anchor these Figs. for the reader.

However, the high-resolution models can exhibit slight temporal offsets in structural evolution compared to observations due to internal spin-up and parameterizations. Therefore, forcing the simulation to perfectly match the HURDAT t_0 would be physically inconsistent. To solve this, we now objectively define the simulated TT period using CPS diagrams (Hart, 2003), along the PV diagnostics following Bentley et al. (2016). The simulated TT is defined as the ± 12h window surrounding the exact moment the modeled cyclone crosses the $-V_T^U = 0$ axis (transitioning from a cold to a warm upper core).

13. Lines 182-184: How can the authors conclude that the dominant perturbation energy source is a consequence of a baroclinic instability? While the cloud pattern in Fig. 3a may show a baroclinic signature, it is too strong to refer to the dominant energy source.

Thank you for this point. We removed the unsupported assertion that the dominant perturbation energy source was a direct consequence of baroclinic instability in Fig. 3a.

14. Lines 225-226: At which level is the cold core identified in Fig. 5b?

We thank the reviewer for pointing this out. We agree that identifying the cold core solely from the equivalent potential temperature (θ_e) field in the original manuscript was ambiguous, as θ_e is heavily influenced by moisture variations. To resolve this, we have added dry potential temperature (θ) contours (green lines) to the cross-sections in Fig. 5 in the revised manuscript. The cold core is located in the mid-to-upper troposphere, approximately between 4 km and 10 km altitude. In the new Fig. 5b, this cold core is now clearly identifiable by the downward bowing (depression) of the dry isentropes across this columna, the classic thermodynamic signature of a cold-core cut-off low. We have updated the text in Section 3.1.2 to explicitly direct the reader to these new θ contours to observe the cold core structure.

15. Fig.6: This analysis is interesting. If the authors could confirm that the sum of the three terms is consistent with the time derivative of the secondary circulation, it would enhance the robustness of this analysis.

We thank the reviewer for this excellent suggestion. We completely agree that evaluating the closure of this budget enhances the robustness of the analysis.

To address this in the revised manuscript, we have explicitly calculated the residual (the difference between the actual time derivative of the secondary circulation and the sum of the centrifugal, thermal, and frictional forcing terms). We have introduced a new supplementary (Fig.A4) that displays both the absolute values of all these terms (Fig. A4a) and their fractional contributions to the total budget (Fig. A4b).

16. Lines 249-250: I think that the small S(t) is not necessarily related to baroclinicity.

Thank you for this point, we agree with the reviewer. A small $S(t)$ indicates a weak symmetric overturning circulation and is not, by itself, a direct metric or proxy for baroclinicity. We no longer infer baroclinic influence from the state of $S(t)$. Instead, we now explicitly quantify baroclinicity using the objective Potential Vorticity (PV) framework of Bentley et al. (2016) presented in the new Fig.A3. This allows us to track upper-level baroclinic forcing (PV3) and lower-level thermal gradients (PV1) independently of the secondary circulation. By removing the explicit link between small $S(t)$ and baroclinicity, and relying instead on the PV diagnostics, we believe the physical interpretation is now much more rigorous.

17. Fig.7: Although this Fig.may present interesting results, the authors should provide a more detailed explanation of the purpose of the analysis and calculation method. As the atmosphere is generally stratified, I expect that the maximum potential intensity at r=0 (vertical axis) occurs at the top of the analyzed domain. If so, at which level is the top? I would also like the authors to discuss what dynamics can change the potential temperature at the center of the cyclone. Adiabatic warming is a possible explanation, but it may disappear if the top of the analyzed domain is H according to the assumption.

We thank the reviewer for these insightful questions. We agree that the physical purpose of this analysis and the mechanisms driving the central temperature changes needed further elaboration. We have substantially expanded our discussion of Fig.7 in the revised manuscript to address these points.

We agree with the reviewer that due to atmospheric stratification, the absolute maximum potential temperature naturally occurs at the upper boundary. We added in the revised manuscript a statement (Section 2.2) that the top of our analyzed integration domain, H, is defined at the upper-tropospheric boundary (bounding the primary overturning circulation). We have added a dedicated physical discussion in Section 3 to clarify the adiabatic warming process. Even though our control volume is capped at H, this subsidence transports high-θ air downward from the stable upper troposphere/lower stratosphere into the mid-levels of the cyclone's core, progressively warming the column from the top down. To make this dynamic process visually apparent to the reader, we added the dry potential temperature (θ) contours to the cross-sections in Fig. 5. These contours clearly show the downward bowing of the isentropes at the center of the cyclone, confirming that forced adiabatic subsidence is the primary dynamic mechanism establishing the warm core.

18. Line 268: The peak maximum equivalent potential temperature in Fig. 7a appears to be about 350 K.

Thanks. Fixed.

19. Line 322: Where are Figs. A1 and A2 presented?

Those are in the Supplementary material (Appendix A).

20. Line 344-346: In relation to the calculation of eddy heat flux, how is the center of the cyclone determined?

Thanks for this point. In our analysis, the center of the cyclone ($r = 0$) at each time step is defined by the location of the minimum Sea Level Pressure (SLP). This tracking methodology is also consistent with the center-finding algorithm used to construct the objective CPS diagrams (new Fig.A2). We have updated the text in Section 2.2 (Methodology) to explicitly state this definition, ensuring readers understand exactly how the cylindrical coordinate system is anchored at every time step.

21. Lines 377-384: Fig.11a shows complex evolution of radial wind at z=14 km. Does the radial wind at this level really reflect deep convection? I'm asking for several reasons. Fig.5f apparently indicates that strong divergent zonal flow occurs below z=14 km after the TT period. Fig.10 shows a positive vertical advection (downdraft) around z= 12 km implying the updraft at z = 14 km may not be connected to the updraft of deep convection. In addition, because the large vertical advection at z=14 km may be partly attributed to the large stratification, is the vertical velocity itself large at this level? Thank for your observation, we really appreciate it.

We agree with the reviewer that 14 km was too high and above the primary outflow layer. Based on the maximum wind divergence observed in the new cross-sections (Figs. 5d, f), we have lowered the analysis level for the radial wind in Fig.11a to 12 km.

22. Fig.11: The gray shading becomes hard to see when it is overlaid on the color shading, particularly in Figs. 11a, 11b, 11c.

The formatting of Fig.11 was modified. We replaced the opaque gray shading with dashed horizontal lines to ensure the underlying contour data remains visible.

23. Lines 403-404: Which part of Fig. 8 indicate the TC eye warming rate?

Thanks for this point. It was a typo and the correct Fig.is Fig. 9.

Technical collections

24. Line 170: Replace “by (Hoskins et al., 1985)” with “by Hoskins et al. (1985)” Fixed.

25. Line 316: “anan” appears to be a typo. Fixed.