Replies to the reviewers of the manuscript "Convective activity within a tropical cyclone undergoing extratropical transition over a warmer ocean" submitted to Weather and Climate Dynamics (Preprint egusphere-2026-2701).

Dear Editor and Reviewers:

We would like to thank you for your time and effort in our manuscript. We really appreciate the detail of your reading, as your comments have been very useful in improving the paper. We have tried to address all your questions. Please, find below our replies to each one.

In this response, for clarity, the reviewers' comments are shown in red, while our responses to each one of them are provided in black. Sentences taken directly from the revised manuscript are shown in italics. Figures/tables mentioned in this response correspond to the revised version of the manuscript. Additionally, some figures included in this response are not in the revised manuscript or its supplementary material, which will be referenced with the letter R (Fig. R1 for example). Please note that some figures appear twice throughout this document, as they are included in our responses to both referees, to facilitate the independent assessment of the corresponding comments.

Reviewer #1:

General comments.

1) The authors add +2°C to SST but keep the atmospheric initial and boundary conditions unchanged. In a real warmer climate, the atmosphere would also be warmer and more humid, and the vertical stability, humidity profile, tropopause height, pressure field, and vertical wind shear could change. With only SST changed, the model starts with a strong ocean atmosphere imbalance. This can artificially increase surface fluxes, CAPE, and convection. This design is acceptable for an idealized SST sensitivity experiment. But it is not enough to claim that the results represent a future climate response. I would either add a consistent atmospheric perturbation following a Pseudo Global Warming method, including temperature, humidity, pressure, and lateral boundary changes, or reframe the whole study as a pure SST sensitivity experiment. In that case, it would be better to remove or weaken all sentences that present the results as a future climate projection. The abstract, discussion, and conclusion must clearly say that only SST was changed and that the results show a conditional sensitivity, not a full future scenario.

We thank the reviewer for this comment and agree that changing SSTs alone in the simulations does not account for a fully consistent future climate scenario. We also appreciate the reviewer's point that this setup may introduce an initial imbalance between the atmosphere and the ocean and that the interpretation of the results should therefore be carefully framed. Taking these considerations into account, we decided to keep this study as a pure SST sensitivity experiment and consider the Pseudo Global Warming approach in future research. Accordingly, the manuscript has been revised throughout to clearly state that the results correspond to a sensitivity experiment based on increased SSTs. In addition, any statements that could be interpreted as future climate projections have been removed to avoid potential misunderstandings. The introduction now says explicitly in lines 74-77:

"*The results presented in this study should not be interpreted as a complete future climate projection, since the atmosphere is not perturbed in the simulations. Instead, they represent an idealized sensitivity experiment that contributes to improve the understanding of the potential influence of warmer oceans on TC characteristics*."

Furthermore, to minimize the influence of the initial ocean-atmosphere imbalance, the spin up period during which this is most pronounced was excluded from the analysis of the convective activity.

2) The study uses one control run and one +2°C run. This is not enough to support strong statements about future cyclone risk, storm severity, or impacts over western Europe. The warmer simulation also changes the storm track and the timing of the interaction with the upper trough. Therefore, changes in wind and rainfall are not only due to thermodynamics. They also come from different track, different shear, different dry air interaction, and different phase of the cyclone. I think the best solution is to add more simulations. For example, +1°C, +3°C, different initial times, or a small ensemble with perturbed initial conditions. If this is not possible, the authors must clearly say that the results are based on one deterministic sensitivity experiment

and should not be generalized. The authors should also compare the simulations by cyclone phase, not only by clock time. For example: tropical phase, start of ET, middle of ET, end of ET. They should show SST below the storm, vertical wind shear, dry air position, surface fluxes, and upper level trough interaction along both tracks.

We agree that the lack of simulations was one the weak points of the manuscript. Thus, following your suggestions, the study has now been modified to add more simulations to the study. In the reviewed manuscript, these are introduced in the lines 92-97:

"*A total of six simulations of Tropical Storm Delta were performed, divided into two sets: a control group (CSIM) and a +2 °C SST group (+2SIM). Each set consists of three member simulations initialized on 26 November 2005 at 00:00, 06:00 and 12:00 UTC. All simulations have a duration of 97 h and are conducted over the same domain (12º–35º N, 48º–8º W), centred over the northeastern Atlantic. The difference between the two groups is the +2 ºC SST perturbation added to the initial and boundary conditions in the abovementioned domain in the simulations from +2SIM. CSIM is driven by initial and boundary conditions from ERA5 reanalysis*".

We decided to add more simulations to the experiment, in order to make its results more robust. The study still focuses on the change produced by the +2 °C SST perturbation in the group average, although the individual members are also plotted in the Figures to add consistency and show their variance. The PDFs from Fig. 5 and Fig. 6 include the three simulations in one distribution for each group.

3) The paper says that warmer SST increases moisture, lowers LCL and LFC, increases CAPE, strengthens updrafts, increases latent heat release, and then deepens the cyclone. This chain is plausible, but the paper does not directly diagnose latent heating, diabatic heating, PV changes, or a heat budget. The claim that latent heat release drives the stronger cyclone is too strong without these diagnostics. The same is true for the claim that earlier ET is caused by a more intense and vertically extended cyclone. The paper needs to show the upper level flow, shear, and PV evolution more clearly. The authors can add direct diagnostics of diabatic heating or latent heating, vertical sections through the cyclone, surface fluxes, and potential vorticity fields. The warm seclusion idea is interesting, but it needs direct evidence. It should not be based only on CPS.

We understand the necessity of performing further analysis and add more diagnostics to justify the claims made in the original manuscript. In the revised manuscript, latent heat release is now directly studied with the same methodology as the other convective variables. It is introduced in lines 129-131: "*Latent heat release is analysed with the same proxy as in Jin et al. (2026), the convergence of integrated water vapour transport integrated between 1000 and 500 hPa in grid points close to saturation (relative humidity over 80%)*" and shown in Fig. 6c and Fig. 7.

Concerning the earlier ET and the warm seclusion, although we agree and it would be interesting to analyse more variables and include these diagnostics in the article, this study focuses on convective activity within the tropical cyclone. That is why we have decided to maintain the manuscript's ultimate objective and include all that new information in a future study, which, incidentally, is underway. Including these new diagnostics in a separate work would also allow to perform a detailed analysis of the complete structural change on the TC and the ET due to the oceanic warming, without making this manuscript excessively long. Therefore, some preliminary results from diagnostics regarding the earlier ET and the warm seclusion are included in this response document.

The ET diagnostics that are analysed, employing the same moving box as in the tracking, are:

-Lower tropospheric Eady Growth Rate (Hoskins & Valdes, 1990; Sarro & Evans, 2022) between 850 and 500 hPa.

-Vertical wind shear between 850 and 200 hPa.

-The 850 hPa frontogenesis through the Pettersen equation (Bluestein, 1993), calculated with the Metpy package (NSF Unidata, 2016).

-The upward motion due to dynamic forcing is calculated through the 200 hPa Trenberth approximation to the QG omega equation (Billingsley, 1998) and filtering the contribution of the cyclone as in López-Reyes et al. (2026).

These new diagnostics will be included in an underway manuscript. However, preliminary results are offered in this response in the Comment 13 and Fig. R1.

The warm seclusion is supported with the 850 hPa temperature and frontogenesis in one instant from the +2SIM group mean, in the Comment 15 and Fig. R2.

Billingsley, D. (1998). A review of QG theory—Part III: A different approach. Natl. Wea. Dig., 22, 3–10.
Bluestein, H. B. (1993). Synoptic-Dynamic Meteorology in Midlatitudes. Volume II: Observations and Theory of Weather Systems. Oxford University Press.
Hoskins, B. J., & Valdes, P. J. (1990). On the Existence of Storm-Tracks. Journal of the Atmospheric Sciences, 47(15), 1854–1864. https://doi.org/10.1175/1520-0469(1990)047<1854:OTEOST>2.0.CO;2
López-Reyes, M., Martín, M. L., Calvo-Sancho, C., & González-Alemán, J. J. (2026). Dynamic forcing behind Hurricane Lidia's rapid intensification. Weather and Climate Dynamics, 7(1), 523–545. https://doi.org/10.5194/wcd-7-523-2026
NSF Unidata. (2016). MetPy (1.7.1). UCAR/NSF Unidata. https://doi.org/https://doi.org/10.5065/D6WW7G29
Sarro, G., & Evans, C. (2022). An Updated Investigation of Post-Transformation Intensity, Structural, and Duration Extremes for Extratropically Transitioning North Atlantic Tropical Cyclones. Monthly Weather Review, 150(11), 2911–2933. https://doi.org/10.1175/MWR-D-22-0088.1

Specific comments.

Abstract

4) The abstract should clearly state that only SST was changed. It should not sound like a full future climate simulation. The sentence saying that Delta becomes a hurricane must clarify the wind averaging issue. The Saffir Simpson scale uses 1 minute wind, while the model gives 10 minute wind. The final sentence about future climates in western Europe should be weaker. I would say that the experiment suggests a possible sensitivity to warmer SSTs, not that it proves a future response.

As it is explained later in this response, the Saffir-Simpson wind thresholds have now been corrected to correspond to 10 min sustained winds. The reference to hurricane status has been removed from the abstract. The abstract no longer presents the approach as a climate projection and now makes it clearer that this is a sensitivity experiment. The claims regarding the results and their implications have been softened. Abstract: "*The northeastern Atlantic basin is a region where the number of cyclones with tropical features can increase in the future due to anthropogenic climate change, a particularly important concern given the region's vulnerability to such systems. This work analyses the influence of higher sea surface temperatures (SSTs), expected in future climates, in the convective activity of Tropical Storm Delta. Delta, which caused strong damage over the Canary Islands (Spain) in November 2005, is representative of a tropical cyclone (TC) experiencing an extratropical transition (ET) on its path to western Europe. Six simulations of the storm were performed with the high-resolution atmospheric numerical model HARMONIE-AROME, divided into two sets of three simulations with different initialization times spaced 6 h apart: a control group and a warm group. The warm group differs from the control group only in the application of a uniform +2 ºC SST perturbation to the initial and boundary conditions of their simulations. The convective activity was analysed only in the convective cells near the cyclone's centre, employing the cloud tracking package Tobac, based on brightness temperature. Results show that increases in low-level water vapor flux, together with lower LCL and LFC levels and increased CAPE, create an environment more favourable for the development of deep moist convection in the warmer ocean simulations. These thermodynamic changes lead to more frequent intense moist updrafts and a larger number of convective cells associated with the cyclone, with greater vertical extent and higher precipitation rates. Consequently, the higher SSTs would cause Delta to become a more intense and deeper TC, through increased latent heat release. These results may contribute to a better understanding of the potential sensitivity of convection within cyclones with tropical characteristics to warmer oceanic temperatures, which could be reached in future climate change scenarios*."

Introduction

5) The introduction gives good motivation, but it moves too fast from one Delta case to the whole western Europe region. I would keep the scope narrower. The paper should better explain why Delta is a good representative case. Is it representative of many future systems, or only a useful example?

Following your suggestion, the introduction has been modified. It has also been added in the lines 68-71: "*Delta was chosen as the case study because it could be representative of future systems, as it underwent a TT and an ET during its life cycle, both of which are projected to become more frequent under future climate change scenarios in the northeastern Atlantic (Montoro-Mendoza et al., 2026; Baker et al., 2022; Michaelis & Lackmann, 2019)*.".

Data and Methodology

6) The +2°C SST perturbation needs more justification. The authors say it comes from a 30 year trend, but the trend calculation, uncertainty, and method are not shown in the main paper. A linear extrapolation for many decades is uncertain. The authors should show the trend analysis or move the claim to a weaker form.
The trend calculation and the linear extrapolation have been removed from the justification, and the claims have been weakened. The choice of +2 ºC is now on lines 102-105:
"*The +2 ºC perturbation value was selected based on the historic evolution of the November averaged SSTs in the northeastern Atlantic (Fig. S1 in the supplementary material), based on the HadISST dataset (Rayner et al., 2003), showing that a +1 ºC monthly anomaly relative to November 2005 has already been reached, and the goal of this paper was to analyse the potential response to future oceanic warmings*."

7) Could you please explain whether the SST perturbation is uniform in space and time. In other words, if SST is updated during the run or only at the initial time and boundaries.
The SST perturbation is uniform in time and space, and SSTs are not updated by HARMONIE-AROME during the run.

8) Please give more information about HARMONIE AROME cy43h1. State if this version differs from the standard version and which physics options are used. Also it would be better to include a map showing full model domains or you can simple overlay domains in Figure 1.
HARMONIE-AROME cy43h1 is the version employed by the Spanish meteorological agency (AEMET) for the operational forecasting. It differs from the reference version (40h1) in:
"- New physiographic data base: ECOCLIMAP Second Generation
- New clay and sand database (SOILGRIDS)
- SURFEX 8.1 version (Le Moigne et al., 2018)
- 2 patches for Nature tile and disabling Surface Boundary Layer Scheme
- Modified values of minimum stomatal resistance Rsmin
- Increase maximum Richardsson number from 0.0 to 0.2
- HARATU turbulence scheme updates mixing at the top of PBL
- EDMF and microphysics optimizations."
More information can be found in Calvo Sánchez et al. (2022).

Model domain is now included in Fig. 1c.

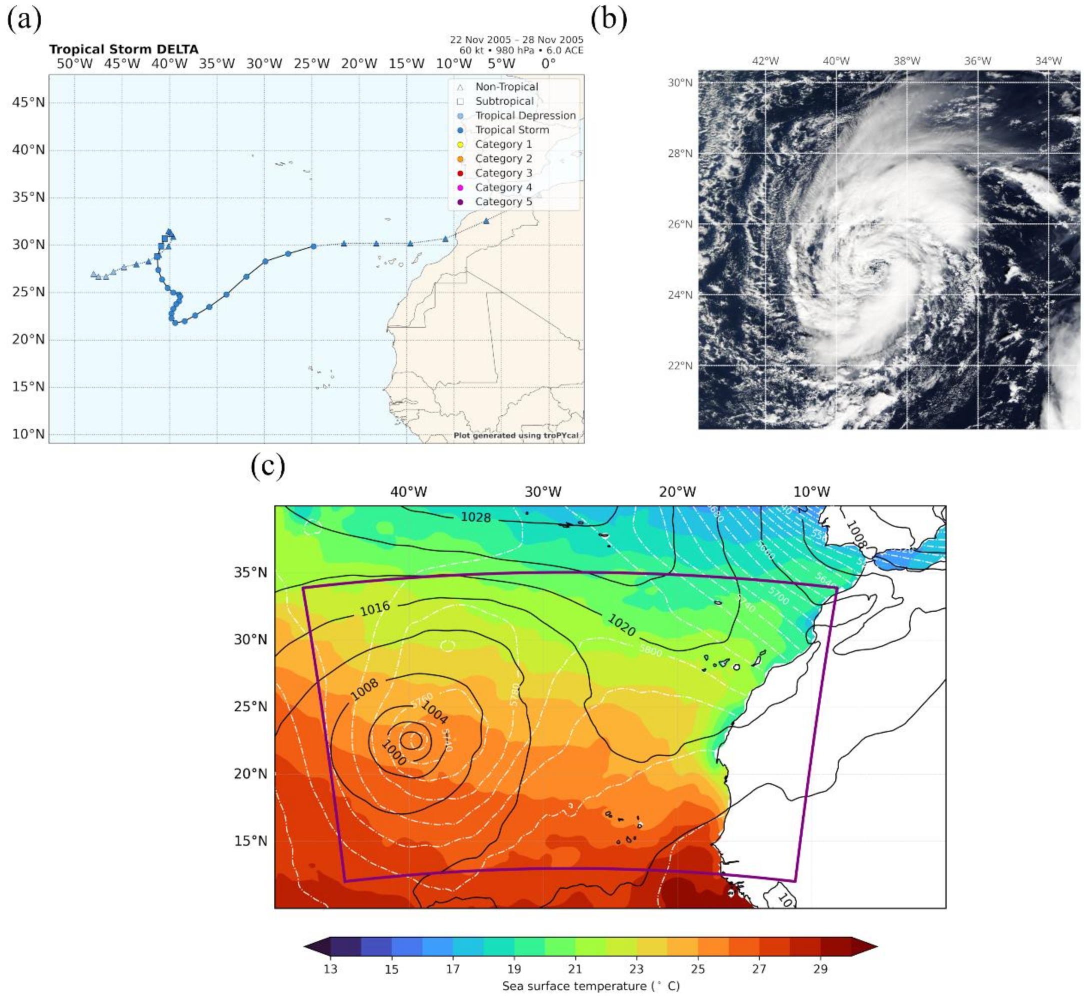

Figure 1: (a) Delta's trajectory from the best track from HURDAT; (b) Delta's satellite image (TERRA / MODIS Corrected Reflectance imagery) on 24 November 2005 00:00 UTC, obtained from Nasa Worldview; (c) Initial synoptic situation for the mean SLP (black contours; hPa), SST (coloured; ºC) and the 500 hPa geopotential height (white dashed contours; m) on 26 November 00:00 UTC from ERA5, utilized as initial conditions for CSIM, in the HARMONIE-AROME domain (purple).

Calvo Sánchez, F. J., Campins, J., Díez Muyo, M. V., Escribà, P., Martín Pérez, D., Morales Martín, G., Sánchez Arriola, J., & Viana Jiménez, S. (2022). Evaluation of HARMONIE-AROME cycle 43h2.1 at AEMET. ACCORD Newsletter, 2, 166–172. http://hdl.handle.net/20.500.11765/13549

9) Could you please discuss spin up more clearly? The grey area in Figure 2 shows spin up, but it is not clear whether spin up hours are removed from all analyses.

Thanks for the comment. The 12 h spin up was initially shown in the original manuscript in some figures as a grey window during those hours. This has now been erased since the addition of simulations with different initialization times and their different spin up periods make the grey window useless.

Additionally, convective cells identified by Tobac in the spin up hours are neglected and not considered in the PDFs from Fig. 5, Fig. 6, or the areas from Fig. 6a, although they are represented in the lines in Fig. 6a for completion. The spin up is now explicitly mentioned in lines 147-148:

"*Also, cells occurring during the first 12 h of their respective simulation are not considered in the analysis to remove the contribution from spin up*."

10) The authors use all grid points inside all detected convective cells and apply Mann Whitney and Fisher tests. I couldn't get the point here because grid points inside one cloud are not independent. Nearby time steps are also not independent. This means that the number of samples is artificially large and the p values are too optimistic. Therefore, the statement that the large sample size increases statistical robustness is not robust in this context. The authors should redo the statistical analysis using more independent samples such as cell mean values, hourly cyclone mean values, object based values, or block bootstrap by time. They can also bootstrap by convective object.

Thank you for pointing out that important detail. In order to fix this problem, the statistical analysis has been modified and now the Mann Whitney and Fisher tests are applied between mean values in the convective cells. This is introduced in the lines 165-167:
"*The Mann-Whitney U test (Mann & Whitney, 1947) and the Fisher F-test (Bisgaard & Vardeman, 1995) are applied to the feature mean values for each group, in order to assess differences in the central tendency and variance forced by the higher SSTs (at the 95% confidence level)*."

11) The Gaussian filter with sigma equal to 5 needs units. Is this 5 grid points, 5 km, or another scale? This matters because the filter can remove or modify real pressure structures.
The Gaussian filter takes a sigma equal to 5 grid points, corresponding to 12.5 km, which is a good resolution for a large-scale field such as SLP. The units are now included in the manuscript, in the line 117.

12) The 5° by 5° tracking box may reduce the influence of other systems, but it does not fully avoid it. Other pressure systems can still affect the pressure field. The authors should explain this more carefully.
The idea of the 5º x 5º originated because a simple track on the minimum filtered SLP would eventually start to track a low-pressure system that appears on the northwest of the domain in the last steps of the simulation, instead of following Delta. So, the concept of the box avoids this, looking for the following minimum filtered SLP in an area near the prior one. The choice of 5º as the length of its side was based on being close to 500 km, which is a reasonable diameter of influence for tropical cyclones when considering the maximum wind speeds or averaging other variables on the cyclone. For example, the analysis with Tobac is performed inside a 250 km radius.
The manuscript now explains this better, in lines 115-121:
"*Tropical Storm Delta cyclone tracking is performed considering hourly mean sea level pressure (SLP), after applying a Gaussian filter with σ=5 grid points (12.5 km) to smooth the small-scale perturbations related to deep moist convection. At each timestep, Delta's position is defined as the location of the minimum filtered SLP inside a 5° × 5° search box. At the initial timestep, the box is centred on the cyclone position reported by the NHC (Beven, 2006). At each subsequent timestep, the box is centred on the cyclone position identified at the previous timestep, so that the search is constrained to the region where Delta was located the hour earlier. This process avoids the influence of other synoptic systems that could appear in the complete domain in the simulated period and affect the tracking.*"

Results and Discussion
13) The earlier ET in the warmer run needs more support. I would be more diagnostic to add vertical wind shear, upper level PV, and outflow diagnostics.
As stated earlier, we agree that the earlier ET is an interesting result that would need further analysis. We have added some preliminary diagnostics (introduced in Comment 3) to the response to support this idea (Fig. R1). Vertical wind shear between 200 and 850 hPa (Fig. R1a) shows a coherent evolution, since the wind shear, a mechanism that favours the loss of symmetry in TCs (Evans et al., 2017), has larger values in +2SIM than CSIM and starts increasing earlier. In addition, Delta enters an environment with a larger baroclinic instability sooner in +2SIM, as shown by the earlier rise in the lower tropospheric Eady Growth Rate (Fig. R1b). This could be associated to the faster translation velocity in +2SIM (Fig. S2 in the supplementary material), which would cause the TC to reach the baroclinic environment of the northeastern Atlantic earlier in its lifecycle, enhancing the baroclinic development of the cyclone. Figure R1c shows that in the +2SIM simulation group there is also a larger dynamical forcing in Delta's surroundings during the tropical phase, as reflected by more negative values of the Trenberth forcing, favouring synoptic vertical motions of air. This could be due to a stronger interaction between the cyclone and the upper-level trough (López-Reyes et al., 2026) in the warmer ocean simulations. Finally, Delta displays an earlier growth of fronts in +2SIM than in CSIM (Fig. R1d), a typical structure of extratropical cyclones. These processes support the idea of the earlier ET in the warmer ocean simulations.
Further analysis on these and more mechanisms related to the ET and the TC structure are expected to be performed for a future study, thus avoiding an excessively large manuscript in this case.
Therefore, the discussion of the earlier ET in the reviewed manuscript after showing the CPS appears in lines 254-262, referencing the cyclone's velocity, which was already in the supplementary material:
"*Later, the ET starts, when the threshold B > 10 m is exceeded (Hart, 2003) and the cyclone exhibits an asymmetric structure. In Fig. 4a, the onset of the ET happens earlier in +2SIM than in CSIM, with about 5 h*

of difference on average. This suggests that the higher SSTs would favour Delta's transition towards a baroclinic extratropical cyclone, thus happening earlier than in CSIM. One of the factors that could contribute to this earlier ET is the faster translation speed in +2SIM before the transition (Fig. S2 in the supplementary material), which would cause the TC to reach the baroclinic environment of the northeastern Atlantic earlier in its lifecycle. Other environmental mechanisms, such as vertical wind shear or the upper-level trough, could also have a relevant role in the earlier ET. A detailed analysis of these processes would be pertinent to understand the complete behaviour of the TC and the ET. However, that analysis is beyond the scope of this paper, which focuses on the convective activity."

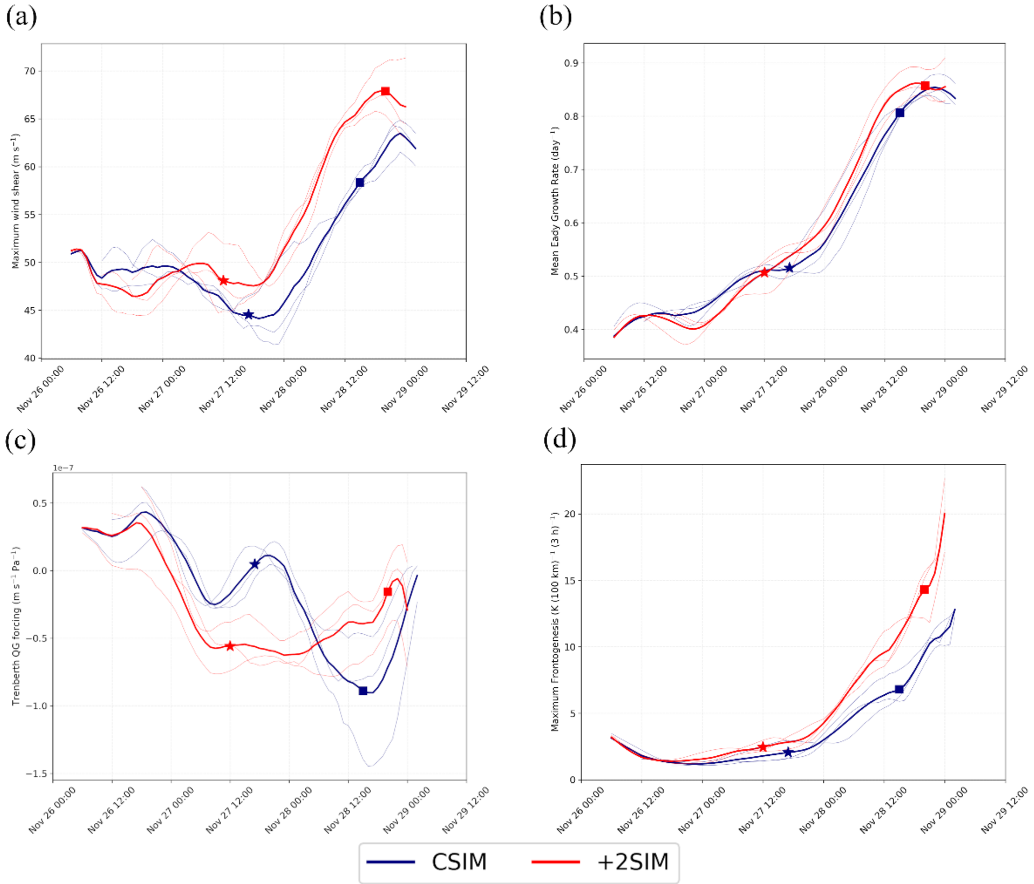

Figure R1: Comparison of different ET diagnostics surrounding Delta in the CSIM and +2SIM mean, smoothed with running means in 12-hour windows: (a) maximum vertical wind shear between 850 and 200 hPa, (b) mean Eady Growth Rate, (c) mean Trenberth forcing and (d) maximum frontogenesis. Stars and squares denote the beginning and the end of the ET, respectively, in both groups Individual members of each group are also plotted as thin lines, in red when they belong to +2SIM and in blue when they belong to CSIM.

Evans, C., Wood, K. M., Aberson, S. D., Archambault, H. M., Milrad, S. M., Bosart, L. F., Corbosiero, K. L., Davis, C. A., Pinto, J. R. D., Doyle, J., Fogarty, C., Galarneau, T. J., Grams, C. M., Griffin, K. S., Gyakum, J., Hart, R. E., Kitabatake, N., Lentink, H. S., Mctaggart-Cowan, R., … Zhang, F. (2017). The extratropical transition of tropical cyclones. Part I: Cyclone evolution and direct impacts. In Monthly Weather Review (Vol. 145, Number 11, pp. 4317–4344). American Meteorological Society. https://doi.org/10.1175/mwr-d-17-0027.1

López-Reyes, M., Martín, M. L., Calvo-Sancho, C., & González-Alemán, J. J. (2026). Dynamic forcing behind Hurricane Lidia's rapid intensification. Weather and Climate Dynamics, 7(1), 523–545. https://doi.org/10.5194/wcd-7-523-2026

14) The comparison with Ophelia is interesting but speculative. The paper has not shown that convective asymmetry is the key reason for the different ET response. This comparison should be weakened or supported with more analysis.

Thanks again for the comment. The discussion about the role of the convective asymmetry in the ET and the comparison with Ophelia have been removed. Now the only direct reference to that article is in lines 276-277: "*The simulations also suggest an earlier ET with higher SSTs, although this may be case dependent, since Ribberink et al. (2026) found that higher atmospheric temperatures and SSTs resulted in latter ETs when simulating Hurricane Ophelia (2017).*"

15) I couldn't get the point by "instant warm seclusion" and how it differs from a classical warm seclusion. The warm seclusion discussion is a bit unclear. Please improve it.

Earlier in this response (Comment 3) we have explained that the discussion about the warm seclusion has also been removed from the revised manuscript, to focus on the convection and make a more complete analysis in a future article. Nevertheless, we briefly explain the concept of the instant warm seclusion here.

In the original manuscript, the CPS suggested that in +2SIM Delta could be experiencing a warm seclusion, as it developed a frontal structure while retaining its deep warm core (Fig. R2). In fact, +2SIM Delta fitted the description of an instant warm seclusion, as defined by Sarro & Evans (2022). The difference between this concept and a regular warm seclusion comes from the initial state of the cyclone prior to the seclusion. The traditional warm seclusion usually involves an asymmetric cold core extratropical cyclone whose warm front bents backwards and ends up isolating the warm air in the cyclone centre (Shapiro & Keyser, 1990). On the other side, the instant warm seclusion starts as a symmetrical warm core tropical cyclone that loses its symmetry and develops a frontal structure, while keeping its deep warm core. This happened to Delta in +2SIM, although in the original manuscript it was based only on the CPS. For further information about the instant warm seclusion, we encourage the referees to consult Sarro & Evans (2022).

Additionally, Fig. R2 supports this idea of Delta as a warm seclusion in the +2SIM group, showing both a warm core and a frontal structure.

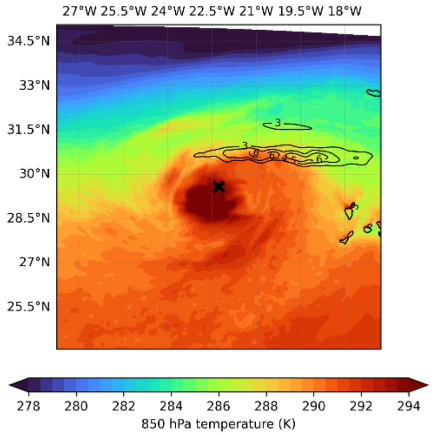

Figure R2: 850 hPa temperature field (coloured), 850 hPa frontogenesis (black contours) and cyclone's position (black cross) at 12:00 UTC 28 November for the +2SIM mean.

Sarro, G., & Evans, C. (2022). An Updated Investigation of Post-Transformation Intensity, Structural, and Duration Extremes for Extratropically Transitioning North Atlantic Tropical Cyclones. Monthly Weather Review, 150(11), 2911–2933. https://doi.org/10.1175/MWR-D-22-0088.1

Shapiro, M. A., & Keyser, D. (1990). Fronts, jet streams, and the tropopause. In C. W. Newton & E. O. Holopainen (Eds.), Extratropical Cyclones: The Erik Palmén Memorial Volume (pp. 167–191). American Meteorological Society.

16) The control run has a track bias at the beginning and does not reproduce the observed pressure fall very well. The paper says HURDAT may have uncertainty, but this does not remove the need for validation. I think the control simulation should be compared with independent data such as satellite brightness temperature, observed precipitation where possible, and ERA5. The fact that the +2°C run matches the best track better than the control run is important. The authors should discuss why this happens and what it means for the control experiment.

To address this concern, several sources of observational data have been employed to further validate the control simulations. We have considered that comparing the simulations with ERA5 was not robust since ERA5 is the source of data for the initial conditions of the simulations. Therefore, satellite brightness temperatures and precipitation were the observed variables that we decided to use to compare with the control simulations. This is introduced in lines 112-114:

"*Other sources of observational data employed to validate the control simulations include GPM-IMERG final run accumulated precipitation (Huffman et al., 2015) and MSG SEVIRI (Aminou, 2002) infrared (10.8 µm) brightness temperatures (BTs).*"

The validation of the control simulations is now included in the results with Fig. 3 and the discussion between the lines 210-219:

"*To further assess the control simulations, several sources of observational data were employed. Satellite infrared brightness temperatures from MSG-SEVIRI and accumulated precipitation from IMERG were* *compared with CSIM (Fig. 3), showing that, overall, the set of simulations correctly reproduces the location and spatial structure of the convection during the tropical phase and the precipitation, although it underestimates the intensity of these processes. Overall, these results indicate that the simulations reproduce Delta consistently although with a weaker intensity. This can be attributed to both the HARMONIE-AROME model, and to the initial conditions from ERA5, which systemically underestimate TC intensity (Sarro & Evans, 2020; Malakar at al., 2020). Nevertheless, ERA5 is the best source of data available for the initial conditions for simulations of an event from 2005 (Lui et al., 2021; Osinski & Radtke, 2020). Therefore, since the goal of the article is not to reproduce the storm to perfection, but to study the changes that are forced by higher SSTs, the simulations are valuable. Thus, considering that the simulations come from the same sources and have similar biases, the results will be focused on the differences between them.*"

Adding two simulations with different initializations and getting similar results supports the idea that the biases may come both from the HARMONIE-AROME model and ERA5.

The fact that the +2 ºC simulations fit better with the observations is because the forcing provided by the higher SSTs (southward shift and more intense TC) coincidentally balances the biases in the control group (limitations in the trajectory during the first days and overestimation of the SLP). However, comparing both groups of simulations is still valid and reasonable, since both come from HARMONIE-AROME and ERA5 and will have a similar bias.

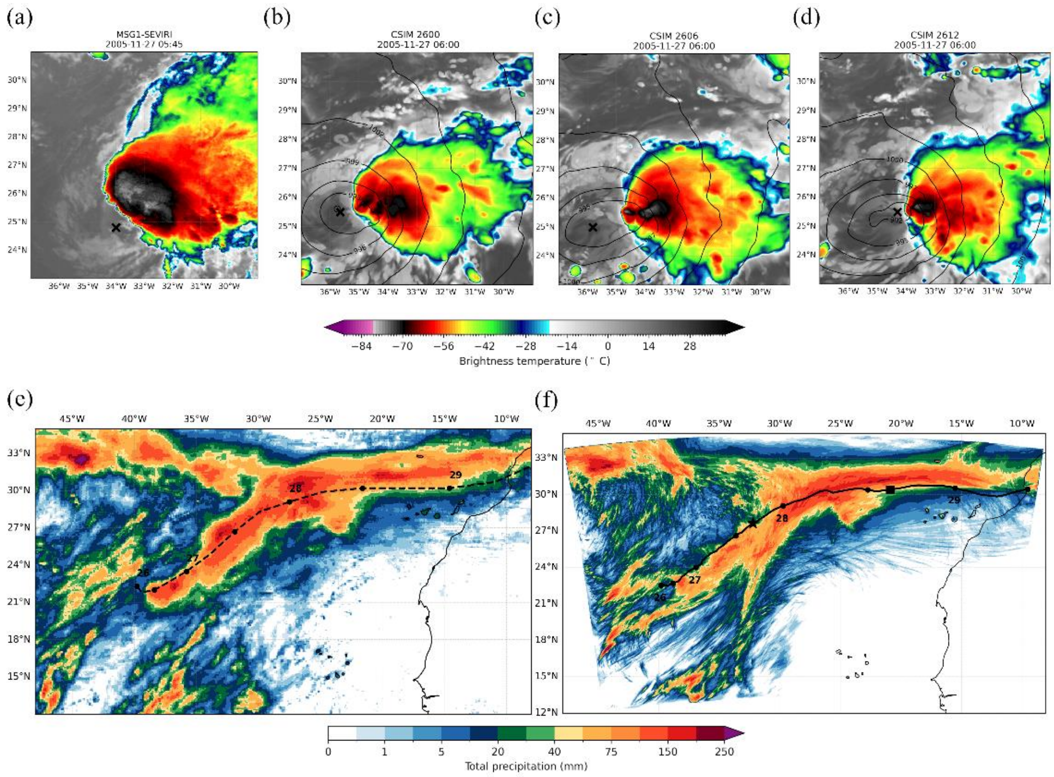

Figure 3: Satellite BT image (coloured) and cyclone's position (black cross) at 06:00 UTC 27 November from (a) BT from MSG SEVIRI and position from HURDAT, (b) CSIM initialized at 00:00 UTC, (c) CSIM initialized at 06:00 UTC, and (d) CSIM initialized at 12:00 UTC. Simulated plots include the filtered SLP field in black contours. Accumulated precipitation (coloured) and cyclone's trajectory (black line) between 00:00 UTC 26 to 30 November 2005 from (e) IMERG and HURDAT (dashed), and (f) CSIM mean. Dots indicate positions at 00:00 and 12:00 UTC, while day numbers are plotted at 00:00 UTC. Stars and squares denote the location of the beginning and the end of the ET, respectively.

17) The use of Tobac is interesting and could be novel for a tropical cyclone case. But the method needs stronger validation. Brightness temperature alone can include anvils and cirrus. I guess it does not always identify active convection. The authors should show examples where Tobac objects are overlaid on BT, reflectivity, vertical velocity, and precipitation.

An example of convective cells identified by Tobac overlaid on different fields is shown next in Fig. R3. This plot shows how BT includes anvils and non-convective points in the identified cells indeed. However, examining the rest of the variables (reflectivity, vertical velocity and precipitation) we can see that BT is the best choice of these 4. This is because the area with highest precipitation does not match exactly with the location of the maxima in reflectivity or vertical velocity. Therefore, if one of these variables was employed to segmentate the cells, some important grid points could be lost. BT, although is the loosest choice, ensures that high precipitation areas, updrafts and downdrafts are included in the cells, and thus in the analysis. The second criterion employed in the study, only to consider cells with at least 1 grid point reaching 35 dBZ reflectivity, helps to erase the contribution from identified cells that are purely cirrus or anvil, and no active convection is being produced inside their area.

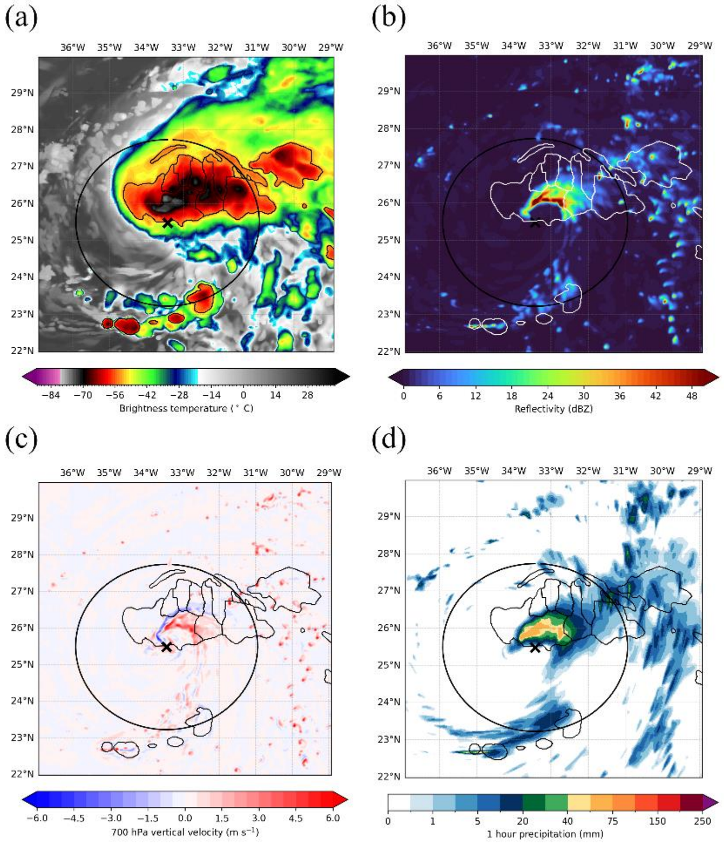

Figure R3: Convective cells identified by Tobac in +2SIM (initialization at 00:00) at 08:00 UTC 27 November overlaid on (a) Brightness temperature, (b) Reflectivity, (c) 700 hPa vertical velocity, and (d) 1 hour precipitation. The black cross marks the cyclone's centre with a circumference of 250 km radius.

18) The sensitivity tests should be shown more clearly in the main text. A table with main metrics for all 16 threshold and step choices would help.

After considering your suggestion, we have seen that one only table would be excessively loaded, so it was divided into two, following the same criterion than the one followed when dividing the original Fig. 4 in two (now Fig. 5 and Fig. 6). These two tables were included in the supplementary material (Table S1 and Table S2 in the supplementary material), and a summary of those in the main manuscript (Table S2). This is described now in lines 286-289:

"*Tables S1 and S2 in the supplementary material summarise the percentage of change in the main metrics shown in Figs. 5 and 6 for all variables in Table 1. These changes between +2SIM and CSIM are reported for all threshold-step combinations. Table 2 summarises these results by presenting the average values*."

Table S1: Percentages of change of each statistic for the thermodynamic variables studied in Fig. 5 between +2SIM and CSIM for every threshold-step combination.

BT threshold	Step	*WVFlux* (mean)	LCL (mean)	LFC (>30 m; mean)	CAPE (mean)
250 K	25 K	+24 %	-17 %	-32 %	+39 %
	20 K	+8 %	-10 %	-35 %	+4 %
	15 K	+25 %	-20 %	-31 %	+38 %
	10 K	+20 %	-19 %	-32 %	+39 %
240 K	25 K	+9 %	-12 %	-39 %	+3 %
	20 K	+32 %	-20 %	-31 %	+49 %
	15 K	+11 %	-14 %	-38 %	+19 %
	10 K	+26 %	-21 %	-33 %	+46 %
230 K	25 K	+3 %	-11 %	-37 %	+10 %
	20 K	+7 %	-16 %	-42 %	+12 %
	15 K	+16 %	-22 %	-40 %	+27 %
	10 K	+15 %	-19 %	-30 %	+26 %
220 K	25 K	+26 %	-32 %	-28 %	+59 %
	20 K	+23 %	-27 %	-27 %	+52 %

	15 K	+16 %	-29 %	-30 %	+36 %
	10 K	+16 %	-31 %	-31 %	+44 %
Average		**+17 %**	**-20 %**	**-34 %**	**+31 %**

Table S2: Same as Table S1 but for the variables of the convective response in Fig. 6.

BT threshold	Step	Area (mean)	Area (sum)	*wq* (p95)	LHR (p95)	CT (mean)	Refl (p95)	TP (p95)	TP (cum)
250 K	25 K	-31 %	+46 %	+21 %	+32 %	+5 %	+13 %	+10 %	+42 %
	20 K	-24 %	+36 %	+3 %	+13 %	+3 %	-3 %	-2 %	+26 %
	15 K	-23 %	+1 %	+18 %	+32 %	+4 %	+13 %	+5 %	+13 %
	10 K	-45 %	+16 %	+19 %	+29 %	+3 %	+9 %	+8 %	+24 %
240 K	25 K	-41 %	+54 %	+9 %	+18 %	+3 %	+7 %	+11 %	+49 %
	20 K	-52 %	-12 %	+42 %	+49 %	+4 %	+30 %	+33 %	+7 %
	15 K	-42 %	+17 %	+10 %	+21 %	+2 %	+4 %	+5 %	+17 %
	10 K	-47 %	+12 %	+18 %	+27 %	+4 %	+8 %	+9 %	+14 %
230 K	25 K	+30 %	+55 %	-9 %	+4 %	+2 %	-8 %	-15 %	+32 %
	20 K	-26 %	+47 %	+2 %	+11 %	+2 %	-2 %	-3 %	+27 %
	15 K	-52 %	+43 %	+20 %	+28 %	+3 %	+12 %	+19 %	+51 %
	10 K	-47 %	+5 %	+13 %	+22 %	+3 %	+5 %	+6 %	+5 %
220 K	25 K	-36 %	+9 %	+24 %	+31 %	+4 %	+17 %	+16 %	+26 %
	20 K	-37 %	+36 %	+24 %	+31 %	+4 %	+15 %	+19 %	+64 %
	15 K	-17 %	+5 %	+10 %	+17 %	+3 %	+8 %	+4 %	+12 %
	10 K	-48 %	+12 %	+16 %	+21 %	+3 %	+7 %	+8 %	+13 %
Average		**-34 %**	**+24 %**	**+15 %**	**+24 %**	**+3 %**	**+9 %**	**+8 %**	**+26 %**

Table 2: Percentages of change of each statistic in Fig. 5 and Fig. 6 between +2SIM and CSIM averaged for every threshold-step combination.

	WVFlux (mean)	LCL (mean)	LFC (>30 m; mean)	CAPE (mean)	Area (mean)	Area (sum)
Average	+17 %	-20 %	-34 %	+31 %	-34 %	+24 %
	wq (p95)	LHR (p95)	CT (mean)	Refl (p95)	TP (p95)	TP (cum)
Average	+15 %	+24 %	+3 %	+9 %	+8 %	+26 %

19) The area of convective cells is highly threshold dependent. Therefore, conclusions about area, fragmentation, and organization should be more cautious.

We agree that this metric is sensitive to the choice of thresholds, as was already noted in the original manuscript. However, the addition of the simulations has notably reduced the variability of this metric by averaging the percentage changes across groups. Table S2 shows that 15 out of the 16 threshold combinations result in a decrease in the mean cell area, while 15 out of 16 result in an increase in the total cell area. This consistency in the sign of the changes across threshold combinations provides stronger support for the robustness of the result.

Nevertheless, we have further moderated the discussion of cell area in the revised manuscript (lines 349–355) to acknowledge the sensitivity of this metric while retaining the main result:

"*Figure 6a also shows the percentage of change in the mean and total area of the convective cells in +2SIM relative to CSIM. Although this metric is sensitive to the thresholds used by Tobac to identify and divide convective cells, the results are consistent across the different threshold combinations tested (Fig. S7 in the supplementary material). After averaging over all thresholds and steps, the results show a decrease in the mean cell area (-34 %) and an increase in their total area (+24 %). This suggests a greater fragmentation, with more but smaller convective cells in the cyclone in the warmer group (Wasko et al., 2016), and a convective structure with a larger horizontal extent, although some uncertainty associated with the identification method remains.*"

20) The phrase "convective initiation" is not fully correct if a cell is selected because it exceeds 35 dBZ at any time. It is better to call them "convective cells with strong reflectivity". and why was 35 dBZ chosen for this storm and this model?

The choice of 35 dBZ was originally based on Henderson et al. (2021), where this is said:

"When tracking growing cumulus clouds using radar and satellite observations, convective initiation (CI) is commonly referred to as the time during which growing convection contains a radar reflectivity ≥ 35 dBZ because that threshold is highly correlated to convection that eventually develops into a mature cumulonimbus cloud (Roberts and Rutledge 2003; Mecikalski and Bedka 2006)".
Those cells have now been renamed to convective cells with strong reflectivity, although the 35 dBZ threshold is still employed.

Henderson, D. S., Otkin, J. A., & Mecikalski, J. R. (2021). Evaluating convective initiation in high-resolution numerical weather prediction models using GOES-16 infrared brightness temperatures. Monthly Weather Review, 149(4), 1153–1172. https://doi.org/10.1175/MWR-D-20-0272.1

21) The paper says lower LCL enhances latent heat release. This is too simple. Lower LCL means condensation begins lower. It can help parcels reach saturation, but it does not by itself prove larger total latent heat release. As I mentioned in my general comments, the authors should use more diagnostic analysis to show the linkage more clearly.
As it was stated earlier, now the article analyses directly latent heat release employing the same proxy as Jin et al. (2026), obtaining the following results, discussed in lines 367-376:
"*Therefore, the higher SSTs force an increase in the environmental humidity and more frequent strong updrafts, which produce an overall stronger convection within the cyclone and a larger latent heat release (Fig. 6c). This is reflected as a broader distribution of the LHR proxy in the convective cells, increasing the frequency of the highest positive values in +2SIM. Larger 95th percentiles are found in every threshold-step combination (Fig. S9 in the supplementary material) for the warm simulations. Overall, the warmer ocean enhances the latent heat release during the tropical phase, as it can be seen in Fig. 7, where the +2SIM mean displays a substantially larger vertically integrated LHR surrounding the cyclone centre than CSIM, in the moment of the start of their respective ET. Additionally, Fig. 7 also shows more extreme negative values of the LHR proxy in +2SIM, which proves the wider distribution in Fig. 6c, although the positive values are the most relevant. Thus, this increased latent heat release would be the potential reason of the intensification of Delta in the tropical phase in +2SIM relative to the control group*."

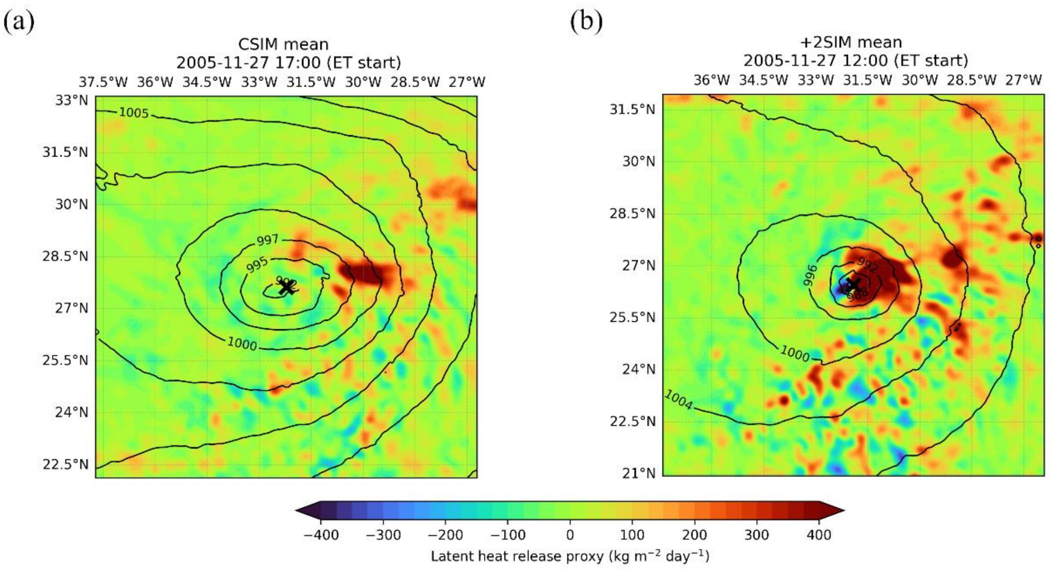

Figure 7: Comparison of the average latent heat release proxy in (a) CSIM and (b) +2SIM, at the start of their respective ET. The mean cyclone's position is marked with a black cross and the mean SLP field is indicated with black contours, for both groups.

22) I couldn't understand the units of WVFlux. If q is included, the unit cannot be simply m s^{-1} without explanation. Am I missing something? In a similar manner, the unit of wq also needs checking. If it is vertical velocity times specific humidity, the unit should be explained.
Relative to the units of *WVFlux* (and *wq*), the specific humidity (*q*) is taken adimensional (kg/kg, kg of water vapor on kg of air). Therefore, the flux stays with velocity units (m s^{-1}).

23) The paper reports precipitation increases mostly as percentage changes. It would be useful if the authors can also give absolute values. What are the total accumulated precipitation in CSIM and plus 2SIM?

Now the discussion of the precipitation also includes absolute values of total accumulated precipitation, in lines 397-399:

"*Moreover, Fig. 8 shows that the total accumulated precipitation caused by Delta during its whole lifecycle is greater in +2SIM than in CSIM, reaching higher values (with a grid point maximum of 217 mm in CSIM mean and 400 mm in +2SIM mean across the simulated period) and displaying a larger affected area.*"

25) The highest precipitation occurs in the second half of the track during ET. This is important and needs more discussion. Why is the ET phase wetter than the tropical phase? Is it due to stronger baroclinic lifting, track change, moisture transport, or slower motion?

This process is now directly discussed in lines 401-406:

"*Interestingly, the highest values of accumulated precipitation are produced in the second half of Delta's track, once the ET is already started. This could be due to the slower motion during the ET in +2SIM (Fig. S2 in the supplementary material), although other factors like the dynamical forcing or the baroclinic instability may be contributing too. This result is particularly relevant in terms of the impacts associated with Delta, as its influence over the Canary Islands and Madeira occurred after it had fully transitioned into an extratropical system.*"

We have followed a similar approach to when discussing the earlier ET, refer to the role of the cyclone velocity. We also mention other potential mechanisms favouring it, although a more detailed analysis on the synoptic processes and the cyclone structure in a different paper would be a desired outcome, as stated earlier in this response.

26) I would be careful with hurricane categories. The model gives 10 minute sustained winds, while the Saffir Simpson scale uses 1 minute sustained winds. The authors convert HURDAT winds for comparison, but they also need to be careful when assigning categories to model winds. If model winds are converted to 1 minute values, please state the conversion. If not, avoid category labels or use them only as approximate.

Thank you for the comment, the reviewer is right. Originally, in Fig 2c the Saffir Simpson scale was on 1 min average while the HARMONIE-AROME wind speeds were on 10 min averages. The same conversion from 10 min to 1 min that was applied to the HURDAT data is applied to the Saffir Simpson thresholds, multiplying them by 0.88. Even so, the claims about hurricane categories have been removed.

27) I think the impact discussion is too strong. One idealized case cannot prove that western Europe will face more violent and damaging cyclones. The conclusion should present this as a possible hazard signal that needs more study.

Conclusions: The conclusion should be shorter and more careful. The last sentence about urgent political action is not appropriate for this paper. The study is one idealized sensitivity test. It can motivate further work and risk assessment, but it cannot support such a broad policy statement. A better conclusion would say that warmer SSTs can strongly modify Delta in this model experiment, but more cases, ensembles, and full climate perturbations are needed before general statements about future western Europe risk. The authors should also clearly state the main limitations in the conclusion: one case, one model, no ensemble, SST only perturbation, and object tracking uncertainty.

It is true that the impact discussion was too strong. Following your suggestions, the end of the conclusions has been modified. In lines 470-483;

"*In the higher SSTs simulations, the cyclone would strengthen through higher latent heat release due to the enhanced convection during the tropical phase. Moreover, this intensification experienced by Delta produces a more severe ET, with a larger precipitation area and especially stronger wind speeds, reaching very harmful wind gusts over 200 km* h^{-1}*. This would be particularly damaging for the Canary Islands, as they were the main area affected by Delta's ET.*

It is important to point out that this experiment is based on idealized simulations of one isolated study case. Therefore, the results presented here should be interpreted as a potential relation between the SSTs and different features from a TC experiencing an ET, instead of a future climate projection. Future research on this topic could include study more cases with different models to add more consistency to the results. Moreover, the Pseudo Global Warming Approach (Schär et al., 1996) could be employed to perturb the atmospheric components too, besides the SSTs, according to CMIP6 climate projections.

To conclude, this work has shown that warmer oceanic conditions could lead to an intensification of the convective activity and an enhancement of the latent heat release within a TC, consequentially turning more *severe. Even so, further review is needed to support this potential relation and improve the understanding of TC characteristics and their potential response to future warmer climates, due to their high human and socioeconomic impacts*."

28) Figures

Figure 1: The size of the legend and labels from panel a has been increased. Panel c now includes 26 November. Simulations start on 26 November since this day was the optimal choice to perform simulations long enough to include part of its tropical phase, the ET and the passage over the Canary Islands, but not so long as to need to perform spectral nudging.

Figure 3c and 3d in the original manuscript: These plots are not included in the manuscript anymore since the claims about convective asymmetry have also been removed.

Figure 4 in the original manuscript has been split into two figures, which are now Figures 5 and 6.

Figure 5 in the original manuscript: Cyclone tracks with numbers for each day have been added to the plot (now in Fig. 8).

Figure 6 in the original manuscript: This plot is not included anymore in the article, since averaging per groups smooths the convective tops and erases the purpose of the figure.

Editorial issues have been fixed.

Reviewer #2:

Major comments.

1) The authors run a single realization of each SST value. As they later point out, an ensemble simulation would make the results much more robust. In addition, a single realization might suffer from perturbations introduced during the initialization process that might affect the subsequent evolution of this relatively short simulation. But these potential problems are not discussed at all. The authors state that the initial conditions were taken from ERA5 reanalysis, but these only have a resolution of 0.25 degrees, and so may not represent well the actual intensity of the tropical cyclone at the time of initialization. Furthermore, without an ensemble of simulations, it is difficult to know whether the results of the perturbation simulation here using only one realization will be robust or not. This is a major weakness of this paper.

We thank the reviewer for this comment, and we agree that the lack of simulations was one the weak points of the manuscript. Thus, following your suggestions, the study has now been modified to add more simulations to the study. In the reviewed manuscript, these are introduced in the lines 92-97:

"*A total of six simulations of Tropical Storm Delta were performed, divided into two sets: a control group (CSIM) and a +2 °C SST group (+2SIM). Each set consists of three member simulations initialized on 26 November 2005 at 00:00, 06:00 and 12:00 UTC. All simulations have a duration of 97 h and are conducted over the same domain (12º–35º N, 48º–8º W), centred over the northeastern Atlantic. The difference between the two groups is the +2 ºC SST perturbation added to the initial and boundary conditions in the abovementioned domain in the simulations from +2SIM. CSIM is driven by initial and boundary conditions from ERA5 reanalysis*".

We decided to add more simulations to the experiment, in order to make its results more robust. The study still focuses on the change produced by the +2 °C SST perturbation in the group average, although the individual members are also plotted in the Figures to add consistency and show their variance. The PDFs from Fig. 5 and Fig. 6 include the three simulations in one distribution for each group.

Regarding the use of ERA5 for the initial conditions, we now address this limitation in lines 213-217, when validating the simulations from CSIM:

"*Overall, these results indicate that the simulations reproduce Delta consistently although with a weaker intensity. This can be attributed to both the HARMONIE-AROME model, and to the initial conditions from ERA5, which systemically underestimate TC intensity (Sarro & Evans, 2020; Malakar at al., 2020). Nevertheless, ERA5 is the best source of data available for the initial conditions for simulations of an event from 2005 (Lui et al., 2021; Osinski & Radtke, 2020)*".

2) Initialization issues: it is well known that a simulation of a tropical cyclone may become unstable or unrealistic if the simulation does not begin with an appropriately initialized vortex. No mention of this issue is made in this paper.

We agree that a well initialized vortex is essential when simulating TCs. In our study, although no specific vortex initialization method is applied in our HARMONIE-AROME simulations, the initial conditions are obtained from ERA5, which already considers these issues during the assimilation. This potential problem is now mentioned explicitly when introducing ERA5 as the initial conditions in lines 96-99:

"*CSIM is driven by initial and boundary conditions from ERA5 reanalysis, with 0.25º horizontal resolution, 137 hybrid vertical levels and 1-h temporal resolution (Hersbach et al., 2023). ERA5 employs a 4D-Var data assimilation system that includes specific developments to address TC initialization, providing initial conditions that can better represent the TC vortex (Lui et al., 2021).*"

In addition, the simulations are initialized on 26 November, several days inside Delta's lifecycle and after its peak intensity, helping the initial conditions to display a coherent vortex for the simulations.

3) Line 347: It is not at all clear that Figure 4f shows that the positive values of the vertical transport of moisture are becoming more extreme in the +2SIM. For positive vertical transport, they look about the same as in the CSIM. In addition, there is a considerable shift towards the left in +2SIM (for negative values), and the naïve reader might then assume that the 95th percentile is actually lower in +2SIM than in CSIM. Please check these numbers.

It is true that at first sight the positive part of the distributions of *wq* in Figure 6b looks very similar in CSIM and +2SIM. However, these include a large number of points, so not all of them can be included in the x axis of the figure. However, the 95th percentiles and their percentage of change (shown in the top right corner) have been calculated numerically, showing that, in fact, this percentile increases in +2SIM relative to CSIM, doing so in many of the threshold-step combinations employed. For example, for the threshold-step combination in the main article (threshold 220 K and step 15 K), the 95th percentile is 0.0143 m s^{-1} in CSIM and 0.0156 m s^{-1} in +2SIM, increasing a 10%. Even so, it is true that the distribution also shifts towards more negative values, thus turning wider overall in the higher SSTs simulations.

This discussion has been modified to address this change, in lines 356-366:

"*The thermodynamic changes shown in Fig. 5 lead to several changes in the vertical transport of moisture within the convective cells (Fig. 6b) in the mid-troposphere (700 hPa). The distribution becomes broader in +2SIM relative to CSIM, indicating a higher occurrence of the most extreme values of upward transport of moisture, with statistically significant changes in its variance. A pronounced increase in the 95th percentile is found for almost all the threshold-step combinations (Fig. S8 in the supplementary material), increasing a +15 % on average. Accordingly, the highest wq values, corresponding to strong convective updrafts, appear more frequently over the warmer ocean. It is noteworthy that the distribution in +2SIM shows a higher density at the lower values of wq than CSIM, giving that the overall frequency of updrafts could be decreasing with the higher SSTs. Even so, the occurrence of the most extreme moist updrafts increases steadily. The shift towards negative values of the vertical transport of moisture could also be associated to stronger downdrafts in the convective cells (Fig. 6b), a process that might play an important role in the development of the convection, as enhanced downdrafts can promote the initiation of new convective cells through cold pool intensification (Marion & Trapp, 2019).*"

Minor comments

4) Line 59: Consider adding the following reference: Emanuel, K., Alberti, T., Bourdin, S., Camargo, S.J., Faranda, D., Flaounas, E., Gonzalez-Aleman, J.J., Lee, C.Y., Miglietta, M.M., Pasquero, C. and Portal, A., 2025. CYCLOPs: A unified framework for surface flux-driven cyclones outside the tropics. Weather and Climate Dynamics, 6(3), pp.901-926

Thank you. The reference has now been added to the manuscript.

5) Line 111: how specifically is the estimated position calculated?

Maybe this concept was not sufficiently clear, so the following example illustrates the procedure. At the initial timestep (00:00 UTC on 26 November; timestep 0), the minimum of the filtered SLP field is searched for

inside a 5° × 5° box centred on Delta's position according to the NHC best track at that time. The location of this filtered SLP minimum is then taken as Delta's initial position in the simulation.
For each subsequent timestep, the same procedure is repeated, but the search box is no longer centred on the NHC position. Instead, it is centred on the filtered SLP minimum identified at the previous timestep. Each new position is determined by searching for the filtered SLP minimum within a 5° × 5° box centred on the diagnosed position from the preceding timestep. For example, at timestep 33, the search box is centred on the filtered SLP minimum obtained at timestep 32. This moving search window ensures that the tracking follows the same cyclone throughout the simulation while minimizing the influence of other pressure systems.
Thus, as an answer to the question, that estimated position is calculated as it was said in the manuscript, as the minimum filtered SLP.

6) Line 128: not all readers will be familiar with the term "watershedding" in this context.
The watershedding technique is a classical segmentation method employed by Tobac to separate individual identified features. It treats the BT field as a topographic surface, where lower BTs correspond to local minima. Starting from these minima, the algorithm progressively floods the surrounding regions until areas associated with different minima meet, defining the boundaries between individual features.
We encourage the readers to consult the Tobac documentation for more information about the watershedding technique in the segmentation: Heikenfeld et al., 2019; Sokolowsky et al., 2024; or the online guide in: https://tobac.readthedocs.io/en/latest/index.html.

Heikenfeld, M., Marinescu, P. J., Christensen, M., Watson-Parris, D., Senf, F., Van Den Heever, S. C., & Stier, P. (2019). Tobac 1.2: Towards a flexible framework for tracking and analysis of clouds in diverse datasets. Geoscientific Model Development, 12(11), 4551–4570. https://doi.org/10.5194/gmd-12-4551-2019
Sokolowsky, G. A., Freeman, S. W., Jones, W. K., Kukulies, J., Senf, F., Marinescu, P. J., Heikenfeld, M., Brunner, K. N., Bruning, E. C., Collis, S. M., Jackson, R. C., Leung, G. R., Pfeifer, N., Raut, B. A., Saleeby, S. M., Stier, P., & Van Den Heever, S. C. (2024). tobac v1.5: introducing fast 3D tracking, splits and mergers, and other enhancements for identifying and analysing meteorological phenomena. Geoscientific Model Development, 17(13), 5309–5330. https://doi.org/10.5194/gmd-17-5309-2024

7) Line 196: the better agreement between the simulated track in the +2SIM and the current climate observations may well be coincidental and due to the lack of an ensemble of simulations.
Multiple simulations have now been performed, and the simulations in the control group still show similar biases to the original one. The fact that the +2 ºC simulations fit better with the observations is because the forcing provided by the higher SSTs (southward shift and more intense TC) coincidentally balances the biases in the control group (limitations in the trajectory during the first days and overestimation of the SLP). In the results this is discussed while validating the control simulations, in lines 205-206:
"*However, Calvo-Sancho et al. (2023) showed that HARMONIE-AROME has certain limitations in reproducing cyclones trajectories, and during the latter hours the track fits better*."
And also in lines 213-219:
"*To further assess the control simulations, several sources of observational data were employed. Satellite infrared brightness temperatures from MSG-SEVIRI and accumulated precipitation from IMERG were compared with CSIM (Fig. 3), showing that, overall, the set of simulations correctly reproduces the location and spatial structure of the convection during the tropical phase and the precipitation, although it underestimates the intensity of these processes. Overall, these results indicate that the simulations reproduce Delta consistently although with a weaker intensity. This can be attributed to both the HARMONIE-AROME model, and to the initial conditions from ERA5, which systemically underestimate TC intensity (Sarro & Evans, 2020; Malakar at al., 2020). Nevertheless, ERA5 is the best source of data available for the initial conditions for simulations of an event from 2005. Therefore, since the goal of the article is not to reproduce the storm to perfection, but to study the changes that are forced by higher SSTs, the simulations are valuable. Thus, considering that the simulations come from the same sources and have similar biases, the results will be focused on the differences between them*."

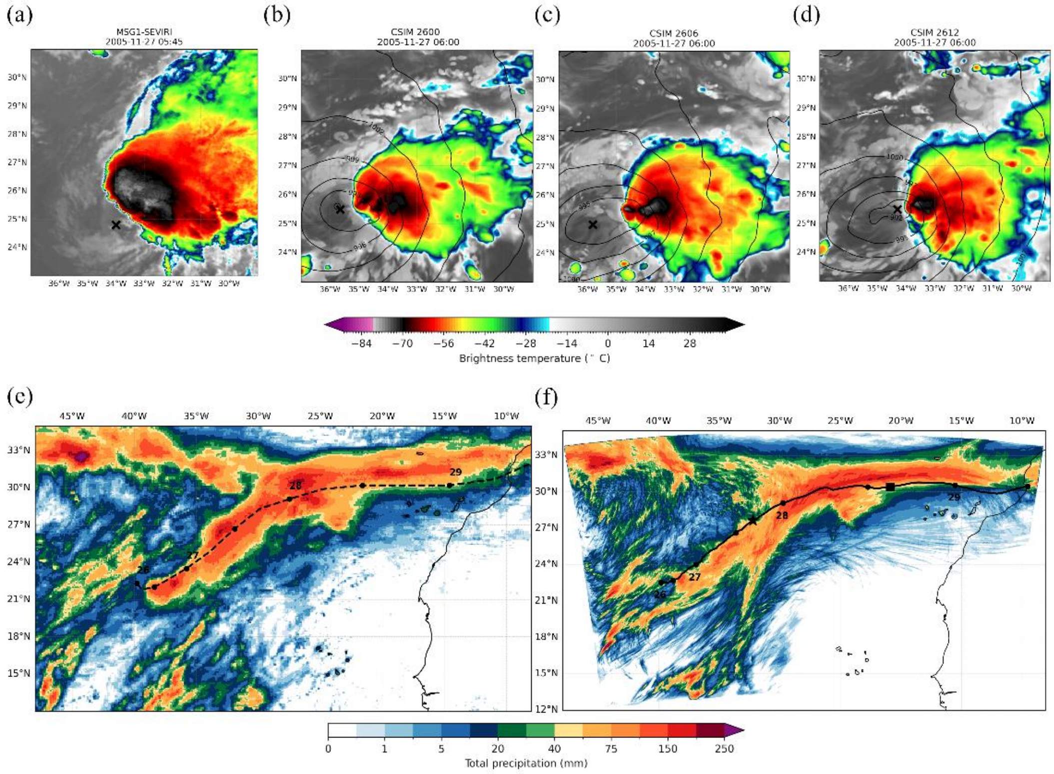

Figure 3: Satellite BT image (coloured) and cyclone's position (black cross) at 06:00 UTC 27 November from (a) BT from MSG SEVIRI and position from HURDAT, (b) CSIM initialized at 00:00 UTC, (c) CSIM initialized at 06:00 UTC, and (d) CSIM initialized at 12:00 UTC. Simulated plots include the filtered SLP field in black contours. Accumulated precipitation (coloured) and cyclone's trajectory (black line) between 00:00 UTC 26 to 30 November 2005 from (e) IMERG and HURDAT (dashed), and (f) CSIM mean. Dots indicate positions at 00:00 and 12:00 UTC, while day numbers are plotted at 00:00 UTC. Stars and squares denote the location of the beginning and the end of the ET, respectively.

8) Line 209: "Once the ET starts": when is this specifically?

This was originally referred to after 9:00 UTC 27 November, the start of the ET in +2SIM (initialized at 00:00), where the SLP falls until 04:00 UTC 28 November, approximately the first half of the ET. However, this sentence is not included anymore in the manuscript.

9) Line 226: please indicate specifically where it is shown that the latent heat release in the control simulation is less.

In the revised manuscript, latent heat release is now directly studied with the same methodology as the other convective variables. It is introduced in lines 129-131: "*Latent heat release is analysed with the same proxy as in Jin et al. (2026), the convergence of integrated water vapour transport integrated between 1000 and 500 hPa in grid points close to saturation (relative humidity over 80%)*".

After this analysis, we obtain the following results, discussed in lines 367-376:

"*Therefore, the higher SSTs force an increase in the environmental humidity and more frequent strong updrafts, which produce an overall stronger convection within the cyclone and a larger latent heat release (Fig. 6c). This is reflected as a broader distribution of the LHR proxy in the convective cells, increasing the frequency of the highest positive values in +2SIM. Larger 95th percentiles are found in every threshold-step combination (Fig. S9 in the supplementary material) for the warm simulations. Overall, the warmer ocean enhances the latent heat release during the tropical phase, as it can be seen in Fig. 7, where the +2SIM mean displays a substantially larger vertically integrated LHR surrounding the cyclone centre than CSIM, in the moment of the start of their respective ET. Additionally, Fig. 7 also shows more extreme negative values of the LHR proxy in +2SIM, which proves the wider distribution in Fig. 6c, although the positive values are the most relevant. Thus, this increased latent heat release would be the potential reason of the intensification of Delta in the tropical phase in +2SIM relative to the control group*."

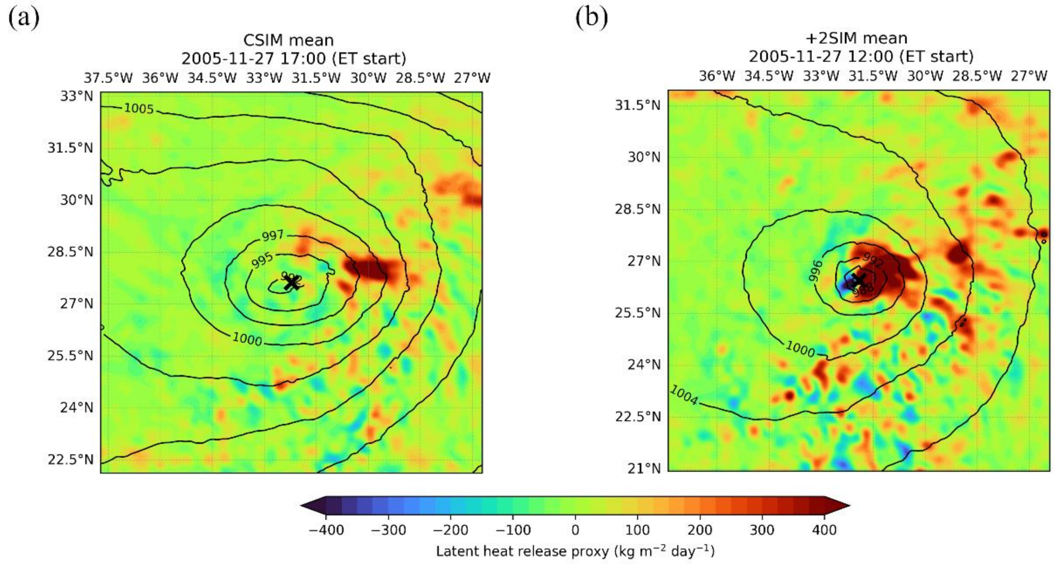

Figure 7: Comparison of the average latent heat release proxy in (a) CSIM and (b) +2SIM, at the start of their respective ET. The mean cyclone's position is marked with a black cross and the mean SLP field is indicated with black contours, for both groups.

10) Line 252: where is the dry intrusion shown?

The dry intrusion was introduced in Sect. 3.1, where Delta's synoptic evolution was described. This dry intrusion was produced because of a mesoscale tropopause depression, as stated and shown by Martín-León et al. (2006) in their report on Tropical Storm Delta. To show its presence in the simulations we plot in Fig. R4 the BT field from CSIM, initialized at 00:00, during 06:00 UTC 27 November (from Fig. 3b), signalling with a black arrow the dry intrusion, as it's done in Martín-León et al. (2006) in their Fig. II.27.

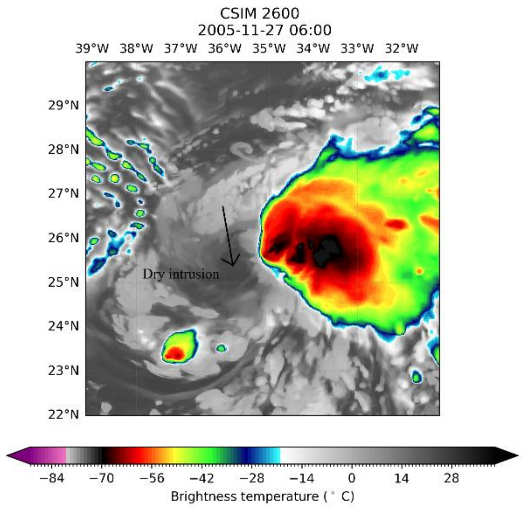

Figure R4: Brightness temperature field in CSIM initialized at 00:00, at 06:00 UTC 27 November. The black arrow indicates the position and direction of the dry intrusion in the TC, as in Martín-León et al. (2006).

Martín León, F., Alejo Herrera, C. J., Bustos Seguela, J. J. de, Calvo Sánchez, F. J., San Ambrosio Beirán, J. I., Sánchez-Laulhé, J. M., & Santos Muñoz, D. (2005). Arcimis: Estudio de la tormenta tropical "Delta" y su transición extratropical: efectos meteorológicos en Canarias. Agencia Estatal de Meteorología. http://hdl.handle.net/20.500.11765/1341

11) Line 260: please indicate specifically what time period is referred to here.

This sentence originally referred to the time period where the parameter *B* changed the most (from 08:00 until 18:00 UTC 27 November in +2SIM, approximately) and the translation speed of the cyclone had its first peak. However, this sentence is not included anymore in the manuscript, to reduce the claims about the ET and focus on the convection.

12) Line 278: where is the low-level cooling shown?

The low-level cooling is represented in both plots of the CPS, in the x axis, through the low-level thermal wind ($-V_T^L$), when this reaches negative values. To support the CPS and show the low-level cooling, the 700 hPa temperature of the cyclone in the CSIM group mean is represented next in Fig. R5, in two different timesteps of Delta's lifecycle, with positive (a) and negative (b) values of $-V_T^L$, showing the warm and the cold core in the lower levels, respectively.

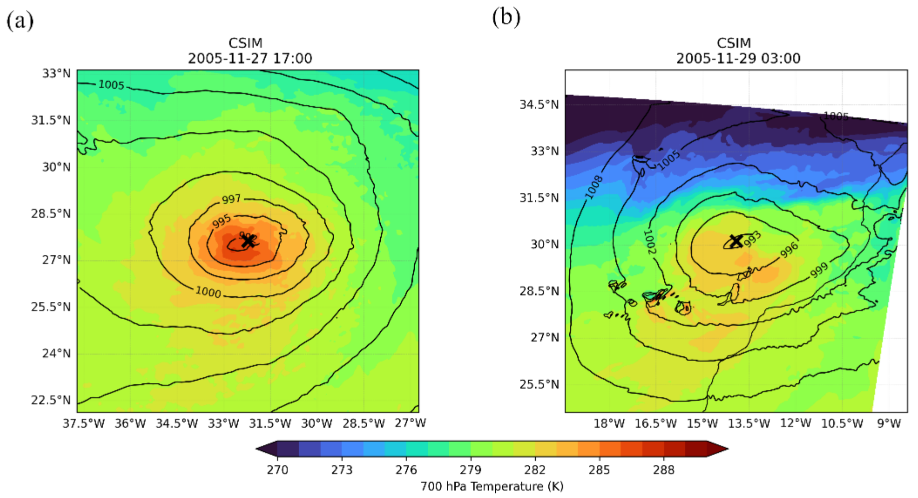

Figure R5: Comparison of the mean 700 hPa temperature in CSIM at (a) 17:00 UTC 27 November (b) 03:00 UTC 29 November. The cyclone's position is marked with a black cross and the SLP field is indicated with black contours, for each group mean.

13) Line 281: "its central SLP weakens at the same time but creates a much more severe ET". Sorry, it is not clear what is meant by this.

Thank you for the comment. What was meant by this is that in the higher SST simulations, Delta strengthens its SLP, turning more intense. One could expect that as a stronger cyclone, its lifecycle could be extended and persist for a longer time. However, it is seen that Delta weakens at a similar time in both SST groups. Therefore, the change in terms of the cyclone is purely in intensity, with notably stronger wind speeds, but not in longevity. This is said in lines 228-231:

"*Overall, these results show that the higher SSTs do not make Delta a longer lasting system, since its central SLP weakens at the same time, but creates a more severe ET (Garin et al., 2025), characterized by an intensification of its wind speeds, and very harmful wind gusts reaching over 200 km h^{-1} (not shown), which occur when Delta is passing by the Canary Islands*."

14) Line 324: "higher relative humidity": where is this shown?

After a spatial visualization of an example of 700 hPa relative humidity field in the +2SIM and CSIM groups, we need to apologize because we were wrong, it is not correct to say higher relative humidity. Therefore, this claim has been erased from the manuscript.

15) Line 358: "even less convective regions are developing clouds until higher levels." Is this the right way to say this? It looks like there is more cloud overall in +2SIM that in CSIM, so "even less" might be slightly misleading.

We agree that this sentence could be confusing. Thus, we have changed this sentence to a different version, in lines 380-382:

"*This increase in cloud top is significant in all the thresholds and steps (Fig. S10 in the supplementary material), meaning that both the convective cells and their anvils are being developed up to higher levels*".

16) Figure 5: the statement on line 370, that the precipitation in the +2SIM is greater than the CSIM, is almost certainly correct but would be more convincing if a difference plot were shown (+2SIM minus CSIM). This plot would also give a better depiction of geographical shifts in the precipitation pattern.

Since the precipitation field in CSIM is now included in Fig. 3e, we have considered that this suggestion was a good idea and that there was enough space. This plot is now included in Fig. 8b, also showing the cyclone track of both groups of simulations. In lines 400-401 it says:

"*The difference between the two groups (Fig. 8b) shows how the increases are not only caused by higher precipitation rates but also by the southward shift in the trajectory*."

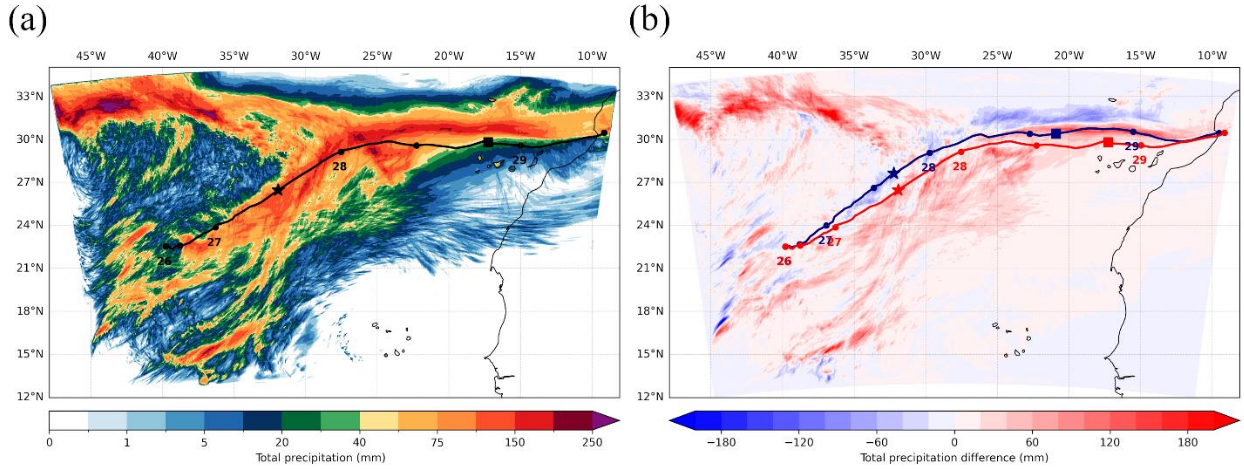

Figure 8: (a) Mean of the total accumulated precipitation between 26 and 30 November in +2SIM, with the trajectory plotted in black. (b) Difference in the average total accumulated precipitation between 26 and 30 November between +2SIM (a) and CSIM (Fig. 3f), with their respective cyclone tracks in blue for CSIM and red for +2SIM. Dots indicate positions at 00:00 and 12:00 UTC, while day numbers are plotted at 00:00 UTC. Stars and squares denote the beginning and the end of the ET, respectively.

17) Line 383: no diagnostic is given that shows that the baroclinic contribution is becoming dominant. Similarly, line 388: it is not shown that the interaction with the jet stream is the main reason that the cyclone becomes more baroclinically driven.

We agree that these claims were not supported by enough evidence. Following this, we have decided to remove most of the claims regarding the ET, baroclinicity or TC structure, since this study focuses on convective activity within the tropical cyclone. However, we provide in this response some preliminary results regarding these topics, which will hopefully be included in a future underway paper.

The ET diagnostics that are analysed, employing the same moving box as in the tracking, are:

-Lower tropospheric Eady Growth Rate (Hoskins & Valdes, 1990; Sarro & Evans, 2022) between 850 and 500 hPa.

-Vertical wind shear between 850 and 200 hPa.

-The 850 hPa frontogenesis through the Pettersen equation (Bluestein, 1993), calculated with the Metpy package (NSF Unidata, 2016).

-The upward motion due to dynamic forcing is calculated through the 200 hPa Trenberth approximation to the QG omega equation (Billingsley, 1998) and filtering the contribution of the cyclone as in López-Reyes et al. (2026).

These are shown in Fig. R6. Regarding the baroclinic nature of the cyclone, these plots show a higher vertical wind shear (Fig. R6a) and a higher baroclinicity (Eady Growth Rate; Fig. R6b) after Delta's start of the ET, in +2SIM relative to CSIM, and consequently an earlier development of fronts (Fig. R6d), suggesting a higher contribution from baroclinic conversion of energy. Figure R6c shows that in the +2SIM simulation group there is also a larger dynamical forcing in Delta's surroundings during the tropical phase, as reflected by more negative values of the Trenberth forcing, favouring synoptic vertical motions of air. This could be due to a stronger interaction between the cyclone and the upper-level trough (López-Reyes et al., 2026) in the warmer ocean simulations.

These are only preliminary results, and further research will be performed since the idea is to include the detailed analysis in a future article. However, we preferred to show this in the response to support some of the claims made in the original manuscript. Even so, as we stated, these are not included in the reviewed manuscript anymore.

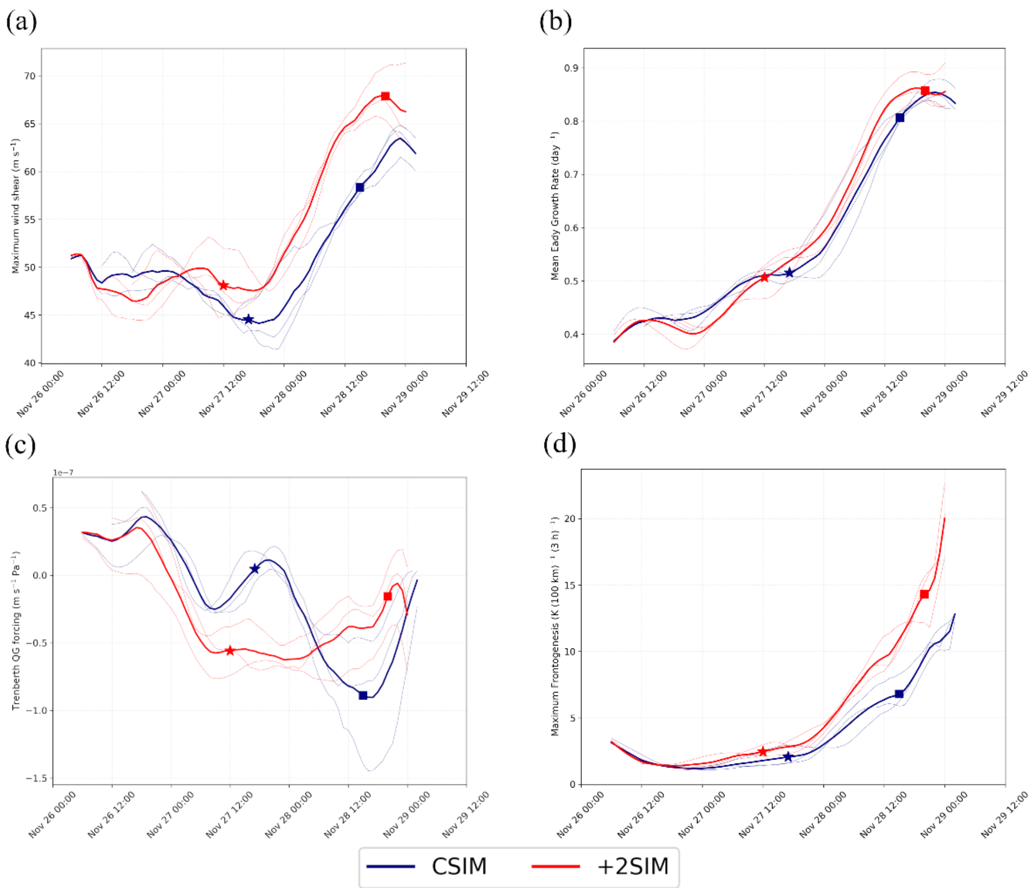

Figure R6: Comparison of different ET diagnostics surrounding Delta in the CSIM and +2SIM mean, smoothed with running means in 12-hour windows: (a) maximum vertical wind shear between 850 and 200 hPa, (b) mean Eady Growth Rate, (c) mean Trenberth forcing and (d) maximum frontogenesis. Stars and squares denote the beginning and the end of the ET, respectively, in both groups Individual members of each group are also plotted as thin lines, in red when they belong to +2SIM and in blue when they belong to CSIM.

Billingsley, D. (1998). A review of QG theory—Part III: A different approach. Natl. Wea. Dig., 22, 3–10.
Bluestein, H. B. (1993). Synoptic-Dynamic Meteorology in Midlatitudes. Volume II: Observations and Theory of Weather Systems. Oxford University Press.
Evans, C., Wood, K. M., Aberson, S. D., Archambault, H. M., Milrad, S. M., Bosart, L. F., Corbosiero, K. L., Davis, C. A., Pinto, J. R. D., Doyle, J., Fogarty, C., Galarneau, T. J., Grams, C. M., Griffin, K. S., Gyakum, J., Hart, R. E., Kitabatake, N., Lentink, H. S., Mctaggart-Cowan, R., … Zhang, F. (2017). The extratropical transition of tropical cyclones. Part I: Cyclone evolution and direct impacts. In Monthly Weather Review (Vol. 145, Number 11, pp. 4317–4344). American Meteorological Society. https://doi.org/10.1175/mwr-d-17-0027.1
Hoskins, B. J., & Valdes, P. J. (1990). On the Existence of Storm-Tracks. Journal of the Atmospheric Sciences, 47(15), 1854–1864. https://doi.org/10.1175/1520-0469(1990)047<1854:OTEOST>2.0.CO;2
López-Reyes, M., Martín, M. L., Calvo-Sancho, C., & González-Alemán, J. J. (2026). Dynamic forcing behind Hurricane Lidia's rapid intensification. Weather and Climate Dynamics, 7(1), 523–545. https://doi.org/10.5194/wcd-7-523-2026
NSF Unidata. (2016). MetPy (1.7.1). UCAR/NSF Unidata. https://doi.org/https://doi.org/10.5065/D6WW7G29
Sarro, G., & Evans, C. (2022). An Updated Investigation of Post-Transformation Intensity, Structural, and Duration Extremes for Extratropically Transitioning North Atlantic Tropical Cyclones. Monthly Weather Review, 150(11), 2911–2933. https://doi.org/10.1175/MWR-D-22-0088.1

18) Line 413: where is it shown specifically that the individual convective cells are smaller in +2SIM?

In Fig. 6a in the top right corner, the percentage of change in the average size of the convective cells in +2SIM relative to CSIM is shown, displaying an average reduction in this metric. However, as Reviewer #1 pointed out, this metric is highly sensitive to the threshold-step choice, therefore this sentence has been removed, and the claims about the area along the paper have been weakened.

19) Line 450: where is this mechanism shown?

The enhanced latent heat release is now directly analysed in the article in Fig. 6c and Fig. 7 (Comment 9), and the strengthening of the cyclone over the warmer ocean is discussed in the article and shown in Fig. 2 from the reviewed manuscript (and in the original manuscript). The claims about the interaction of the cyclone and the upper-level trough and the wind shear have been removed from the article, since they need further analysis, and to focus the article on the convection. The preliminary results from the Comment 17 (Fig. R6) suggest that this mechanism could be possible, as seen in the higher wind shear and more negative Trenberth forcing, but we consider that this is still not enough evidence and more diagnostics would need to be added.

Technical corrections have been applied to the manuscript.