Review of egusphere-2025-3109
"Dynamic Forcing Behind Hurricane Lidia's Rapid Intensification"
By M. Lopez-Reyes, M. L. Martin-Perez, C. Calvo-Sancho, and J. J. Gonzalez-Aleman

General comments:

This study examines an ensemble forecast of Hurricane Lidia (2023), which was a high-impact rapid intensification (RI) event in the eastern North Pacific basin. Lidia interacted with a mid–upper-tropospheric trough near the time of RI. The authors focus on two groups of ensemble members, which were defined as the strongest and weakest 20% of ensemble member forecasts. The authors argue that in the members that intensified the most rapidly, the upper-tropospheric trough provided quasigeostrophic (QG) forcing for ascent over the tropical cyclone (TC), which aided convective development and TC intensification. Alternatively, in a subset of members that did not intensify as rapidly, QG forcing was weaker. The authors argue that because some thermodynamic conditions were similar between the strongest and weakest members, such as sea surface temperatures and potential intensity, it is likely that dynamical forcing played a key role in the RI process.

Overall, I found the manuscript to be well-written and followed a logical order. I believe the authors are aiming to better understand an important physical phenomenon (TC–trough interactions) in a basin that is not well studied. However, I have significant concerns about the authors' arguments over the impact of relative humidity in the RI process. For example, the authors only show the relative humidity field at one forecast time in Fig. 10, which appears to show regions of significant differences between the ensemble groups. However, the authors conclude that "relative humidity differences between the two ensembles were small and not statistically significant". This claim doesn't appear supported by the evidence shown and a more thorough evaluation of the mid–upper-tropospheric moisture evolution is warranted. I also have multiple other scientific comments that I would like the authors to address, which largely pertain to clarifying aspects of the manuscript, including whether or not the TC circulation was filtered in the calculation of the QG forcing for ascent.

Scientific comments:

1) Line 49: What do the authors mean by ventilation here? Thermodynamic ventilation or ventilating mass?
2) Introduction (general comment): The introduction would benefit from a brief elaboration on why Hurricane Lidia is a storm of interest. What were the impacts of the storm?I understand the TC is described in later sections, but at this point, a reader may not know anything about the storm and could be confused over things like when and where RI occurred.
3) Lines 121–125: I think one should be careful with language like "main dynamical... drivers of TC RI" when talking about the TC's environment. From a kinematic perspective, TC intensity change is driven by the TC's ability or inability to draw in angular momentum surface via the evacuation of mass out of the boundary layer through

convection. For example: **https://doi.org/10.1002/qj.4133 and** https://doi.org/10.1175/MWR-D-24-0029.1

4) Line 143: Do the authors mean for “identifying” trough interactions rather than “making”?
5) Lines 150–151: While true, should we expect the dynamics of TC–trough interactions to be different in the eastern North Pacific?
6) Lines 154–155: I am confused by this sentence. The authors seem to be comparing the Atlantic basin to itself.
7) Line 183: I am confused by this claim. Doesn’t NOAA routinely run the Hurricane Analysis and Forecast System (HAFS), which is an operational mesoscale model, for TCs in the eastern North Pacific?
8) Line 187: What challenges? It would be helpful to be more specific here.
9) Line 203: Do the authors mean the forecast spans 96 h?
10) Lines 214–215: Does this mean the lowest 20% and top 20% of intensity forecasts? Over what period was this determined? It would be helpful to be more specific.
11) Lines 215–216: As a general comment, it would be helpful to explicitly show when RI began in both observations and each model forecast at some point. My apologies if I missed this.
12) Line 216: Do the ensemble members have the resolution to truly capture the changes in peak winds? How well did the ensemble members reproduce the best track intensity?
13) Lines 223–224: How is shear computed here? Is the TC vortex removed?
14) Line 261: What about prior to RI onset? And does this mean for the observed RI period or the simulated RI period in each ensemble member?
15) Lines 278–281: It would be helpful to show the evolution of the wind field over this period using the reanalysis.
16) Figure 1: The legend of panel a and inset of panel b are quite small and difficult to read.
17) Figure 1: While a nice-looking figure, It would be easier to compare the model forecasts with observations in panel b if the two were shown along the same axis. I also recommend revising the x-axis label in panel b from "time step" to "forecast hour" for improved clarity.
18) Line 298: "demonstrates" is a strong word choice at this point in the manuscript, considering only one figure has been shown thus far. I recommend revising this word to "suggests".
19) Line 305: How was the successful simulation of RI determined? That any forecast point exceeded the 30 kt/24 h threshold?
20) Lines 321–322: While I agree with the previous statements, I do not understand how this claim was arrived at. The timing of the synoptic-scale forcing has yet to be shown at this point in the manuscript.
21) Lines 325–326: Mostly a curiosity question: what contributes to these large SST differences? Does the model include upwelling processes? Or are these related to differences in TC track?
22) Figure 2: Were any aspects of the TC circulation filtered out in the calculation of PI? Also, what are the units for panels a and b? Neither the caption nor figure specifies this.
23) Figure 2c caption: What radial range? Please specify in the text.

24) Figure 3: Are these TC-centered images? How was the TC center determined in this study?
25) Lines 355–357: I question the accuracy of this claim. Yes the authors explore PI and SST, but what tropospheric humidity/ventilation? For example, Fischer et al. (2023; https://doi.org/10.1175/MWR-D-22-0037.1) examined a high-resolution ensemble simulation of a TC–trough interaction case in the North Atlantic. They found a key influence in the timing of RI onset was the degree to which dry air from the nearby trough made it into the TC inner core and eroded convection there (ventilation). Is it not possible humidity/moist entropy differences played an important role here too?
26) Lines 362–363: What is the ventilation layer of the TC? I'm not sure what the authors mean by this.
27) Line 367: Fischer et al. (2019) found that zonally narrower upper-tropospheric troughs are more favorable for RI. Isn't this the opposite pattern shown in Fig. 4?
28) Line 370–371: Both Peirano et al. (2016; **https://doi.org/10.1002/2016GL069040**) and Fischer et al. (2019) found that EFC tends to be not as important for TC intensification as vertical wind shear. I know it is briefly touched upon later, but showing time series of vertical wind shear magnitude in each ensemble group would be helpful.
29) Lines 377–378: What do the authors mean by this phrasing (QG approach)?
30) Line 378: Do the authors mean ventilation as in the evacuation of mass? This seems inconsistent with previous literature which refers to ventilation in the thermodynamic sense (e.g., Tang and Emanuel 2010, 2012). I recommend revising to avoid confusion.
31) Line 382–383: Is this shown anywhere?
32) Lines 394–397: I am confused how the seasonality of the case is important here. Can the authors please clarify?
33) Figure 5: Does the uptick in EFC precede the onset of RI? It would be helpful to clarify the time-lag relationship in the text.
34) Figure 6e: What is the shaded vertical column between hours 60 and 70? What is this supposed to represent? The figure caption does not specify.
35) Lines 431–433: To what extent are the differences near the location of the TC related to the TC circulation itself? Is the TC circulation filtered? If not, the stronger members in the highest intensification rate group may be associated with a vertically-deeper vortex and the advection of the TC's vorticity, rather than the trough's vorticity, at 300 hPa is showing up in this figure. Can the authors please clarify? The wavenumber-1 asymmetry in the QG forcing for ascent near the TC position appears consistent with this.
36) Line 438: Aren't the authors looking at upper-levels of the troposphere here rather than "mid-levels"?
37) Lines 455–456: Similarly, do the authors mean below the 300 hPa shown in Fig. 7? I wouldn't call 300 hPa mid-levels.
38) Lines 469–470: Again, I question whether it's fair to make this claim without examining the role of tropospheric moisture.
39) Line 478–479: The differences between the two ensemble groups are not significant, however, until forecast hour 50. According to Fig. 1, this looks to be near the start of RI. Can the authors please clarify?

40) Lines 499–502: Actually, Fischer et al. (2019) found TCs in the NW trough cluster have the lowest rate of RI of the three TC–trough configurations examined (see their Fig. 6). RI is preferred when a cutoff low exists to the SW of the TC location.
41) Figures 8a–d: Contour labels would be helpful here.
42) Figure 8: Again, is the TC vorticity being filtered out here? Otherwise this may just be a representation of differences in TC strength/structure rather than synoptic-scale forcing for ascent.
43) Line 493: It is difficult for me to see the broader trough in the P80 ensemble in Fig. 8.
44) Lines 525–526: How do the area-averaged shear values compare (e.g., 0–500 km from the TC center)? Such metrics are more commonly used in the literature.
45) Lines 534–537: As noted above, how does RH vary prior to the onset of RI? This seems critical to show to properly claim thermodynamic differences did not contribute to the differing intensity evolutions.
46) Line 542: Can the authors please clarify how this could decrease VWS?
47) Figure 10: These are nice visualizations. However, I do not see any information regarding the spatial scale that these domains span. What is the increment of each radial ring?
48) Lines 556–557: What is this claim based on? I see large regions of significant differences in Fig. 10l.
49) Line 557: Furthermore, the authors only show RH at one forecast hour. How did RH evolve prior to RI onset?
50) Lines 568–573: Hamaguchi and Takayabu (2021; https://doi.org/10.1175/JAS-D-20-0334.1) show how upper-level forcing for ascent can moisten the mid–upper troposphere in tropical depression disturbances. Perhaps a similar sequence is seen here?
51) Lines 613–614: Is it fair to say these members have no trough interaction? Or just that the interaction isn't as favorable? There still appears to be a trough in these members.
52) Lines 627–628: As noted above, it appears the authors only show one time step. What about at other forecast hours? Furthermore, it appears from Fig. 10l that there are indeed regions of significant differences in RH.

Typographical errors/suggestions:

1) Line 133: I believe the “Leroux” study is misspelled here.
2) Lines 211–212: This seems to be an incomplete sentence.