

Response to reviewers for "Grid-Spacing Sensitivity of Rossby Wave Breaking to Mesoscale Diabatic Processes" by Rixen et al.

We thank David Parsons for his thorough reading and we addressed all the comments in the itemized response to the reviews below. We carefully revised the manuscript according to your comments and replied to them in blue font in the pages below, and the corresponding updates in the manuscript are highlighted in italic font.

1 - Please consider including a more direct cautionary statement that investigating only three forecast events each representing a type of flow pattern is a great step forward but should be treated with some caution.

Thank you for this suggestion, we added a statement on this: *"While investigating three detailed forecast events - each representing a distinct flow pattern - provides valuable physical insights and represents a major step forward, generalizing these findings should be done with caution until validated across a broader sample of cases."* (lines 513-515)

2 - A slightly deeper dive into showing why forecasts are not improved for the tropical cyclone transition would be worth doing. You might only need to add a sentence or two and a figure in the supplementary materials.

It is true that the forecast skill (ACC) only shows little improvement at high resolution. We discuss this in section 3.2.3, and argue that there is some inherent low predictability in the system. We believe that going in depth on the predictability of this case would only make the manuscript longer, and therefore we refer to previous literature: *"Overall, these results are consistent with Keller et al. (2026), who emphasized how sensitive the forecast of this blocking pattern (and the subsequent formation of Storm Daniel) was to the position of Hurricane Franklin."*(lines 375-376) [...] *"Ultimately, this case study illustrates the inherently low predictability of certain forecast busts, demonstrating that decreasing grid spacing alone is insufficient to fully overcome these initial-condition constraints. Dolores-Tesillos et al. (2026) also documented the persistent difficulty in simulating atmospheric blocking, confirming that reducing grid spacing alone does not resolve these representation errors."* (lines 407-410)

3 - One issue to consider adding, again that would be very brief, is does this work provide any guidance on convective parameterization. Besides the magnitude, as the horizontal model grid resolution changes does the height of the upper outflow of convection also change? You mention the magnitude, but does the height also change. If so, this result could be useful to modifying convective parameterization.

We looked into the height of the outflow for the different cases and the results varied quite substantially. In the first case, the MCSs do show a grid spacing sensitivity, with finer grid spacing leading to stronger outflow also at high altitude. However, in the other cases, this was not the case, i.e., WCB outflows were for example at similar altitudes. We mention these findings in each case's subsection:

"Beyond generating stronger outflow, finer grid spacings also produce outflows at higher altitudes than coarser resolutions (not shown). [...] However, we did not observe any differences in WCB outflow height with varying grid spacings" (lines 269-270 and 300-301).

4 - The research that explores dynamics of different types of bursts and grid spacing is a great step forward. It is also great to see a link of this research to the likelihood of large errors in climate modeling.

We appreciate this valuable suggestion. Studying the grid spacing sensitivity of Rossby wave breaking is especially interesting, as models with coarse grid spacing have long standing biases in resolving these events.

5 - The methods are also sophisticated. The path of utilized are using higher resolution and ensembles for 8 days, plus initial conditions of SSTs, sea ice, precip etc. Please clarify if these initial conditions are different than employed in the ECMWF forecasts and, if not, does your use of these fields have an impact on forecasts.

The initial conditions used are the same as the ones employed in the ECMWF forecast. However, we did use SST and sea ice fields from observations (from the Climate Data Record (CDR) produced by the European Space Agency (ESA) Climate Change Initiative Sea Surface Temperature project) to stick with the method by *Prein et al., 2026*. Furthermore, the main comparisons in the manuscript are between ICON simulations at different grid spacing, which use all the same SST boundary conditions and allow for a fair comparison. We included a comment on this topic in the manuscript to clarify: *“Overall, the ICON simulation uses the same initial conditions as ECMWF forecast, except for the observational SST and sea ice data, which follow the simulation set-up from Prein et al., 2026.”* (lines 169-171)

6 - This comment is minor, but my experience in examining the role of convection in impacting Rossby waves relies on relatively large long-lived mesoscale convective systems and is not impacted by isolated convection. Hence, consider becoming more specific on the stating you are looking at the role of MCSs and not just convective clouds. Shallow convection, isolated deep convection (i.e., supercells) and short-lived MCSs generally do not create major changes to Rossby waves.

Thank you for the comment; we clarified that point in the conclusion of the paper: *“Importantly, we do not expect shallow convection or isolated deep convection and short-lived MCSs to create major changes in Rossby waves, but focus instead on large MCS that can impact large-scale dynamics.”* (lines 493-495)

7 - Pg. 4 – Small correction needed (?Prein et al., 2017)

Has been corrected.

8 - A minor point to the introduction (or perhaps the conclusion) is to note that since we are looking at three cases with different dynamic patterns, the next step would be a more systematic and larger exploration of the overall benefits of high resolution modeling. For example, a follow-up approach would be a systematic look at the far more bust cases.

We agree that it would be very interesting to extend such an analysis to a larger database of bust cases. An further, equally interesting direction would be to study if busts are consistently (or at least very often) associated with Rossby wave breaking deficiencies and missed interactions over the US/North Atlantic: *“Building on these insights, future research should aim to generalize these results across a larger catalog of events and diverse regions.”*

In particular, expanding high-resolution simulations to additional forecast bust cases would help to assess result consistency, though such an effort remains computationally demanding and beyond the scope of this study.” (lines 555-557)

9 - Current research in a soon to be submitted manuscript from a U Reading PhD student found that using ACCs alone is a better approach to investigate busts as some forecast failures can have very low ACCs and relatively good RSMes. Your work takes the approach of “substantial phase error, as captured by the ACC, and a significant amplitude error, as quantified by the RMSE”. Hence, one could state that you are examining only a subset of forecast busts so that you are likely to only be concentrating on a dynamical subset of busts with specific dynamic structures. Not sure if a revision is needed before this other paper is published or if this finding has come out elsewhere.

That is a very good point we did not think of adding in the paper. We clarify this point in the data and methods section: *“This approach targets a specific subset of forecast busts characterized by both phase and amplitude errors, rather than encompassing the full spectrum of forecast busts (such as those driven mainly by phase errors despite high RMSE).” (lines 196-197)*

10 - Personally, I think that turning off the convective parameterization at 10 km is not a good forecast path, but it is good that you included this work as some previous research that supports this odd finding.

We agree that turning off deep convection is not appropriate for forecasts at 10 km resolution the results are only for specific case studies, making it difficult to generalize. Even at finer resolution IFS runs with a scale-aware convection scheme, highlighting the benefits of parametrization even at finer grid spacings. Furthermore, as we mentioned in the paper *“this may not hold in tropical or sub-tropical regions where synoptic-scale forcing is weaker and convective storms are less organized (Prein et al., 2025), and finer resolution may become more critical.” (lines 544-546)*

11 - The research that shows that one bust is still relatively poorly predicted at high resolution is interesting and consider proposing more clearly why you think that this result is happening.

We refer to our answer in comment 2 as it answers this one as well.