

Review for

Distinct bias structures for extratropical cyclones with strong or weak diabatic heating

by

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Summary:

In their study, Yu et al consider forecast biases of extratropical cyclones depending on the degree of diabatic heating attributed to them. Studying these heating-dependent biases is of importance, particularly as the cyclones with strong diabatic heating exhibit significant biases. The text is generally well written, and the overall discussion can easily be followed. Furthermore, the figures are of high quality, and they clearly support the statements in the text. Still, there are some aspects that deserve further consideration. At some places, the statements remains relatively descriptive, or speculative. It remains also not so clear how the heat-dependent biases 'only' reflect the latitudinal distribution of weak- and strong-heating cyclones; or a link is speculated about dry intrusions (DI) and/or warm-conveyor belts (WCB), but the statements remains rather qualitative.

In summary, I think that the study is of great interest to the research community, and it also fits perfectly into the scope of Weather and Climate Dynamics. To make it publishable, however, some major revisions are needed, as outlined below in detail.

Major concerns:

1. Diabatic influence of surface fluxes: In section 2.3, it is written how the diabatic influence on a cyclone is quantified. Essentially, this is done by taking from ERA5 the diabatic tendencies due to all parameterizations, except for radiative tendencies. From the text it becomes *not* clear how this is done, whereby I assume that the ERA5 field for all diabatic tendencies is taken and the AER5 of radiative heating is subtracted from it. The authors should write more explicitly which fields are available from ERA5 short-term forecasts (?), and describe in greater detail which diabatic processes are included (or not). Furthermore, the reader should be informed why radiative tendencies are excluded and whether they are expected to have a big impact. Please make also clear whether diabatic surface heat fluxes, latent and sensible ones, are included or not. To my knowledge the diabatic tendencies from ERA5 do not explicitly include them. A short discussion on free-tropospheric diabatic tendencies and the ones originating from the surface would be helpful.

2. Snapshot diabatic tendencies vs. accumulated ones: The cyclones are categorized into the ones with strong diabatic heating and weak diabatic heating *at the time step of maximum intensification* during the cyclones' life cycle. This is certainly a valid approach, however, it makes me also wonder how the

accumulation of diabatic effects/tendencies prior to this time step contribute to the forecast biases. I agree with the authors that the strongest diabatic effect might be expected to coincide with the time step of maximum intensification, but still it could be possible that in time span from genesis to maximum intensification substantial diabatic tendencies are accumulated. If so, taking the tendencies at this single time step of maximum intensification could be misleading. I don't expect the authors to redo their analysis based on accumulated tendencies. But they should carefully discuss why it is appropriate to only take on snapshot to do the classification. Possibly, they can compare the categorization for a subsample with snapshot vs. accumulated tendencies.

3. Latitude effect: The authors clearly state that strong-heating and weak-heating cyclones occur, on average, at distinctly different latitude. As expected, strong-heating cyclones are predominantly found at lower latitude, where moisture availability is higher. Given this difference in latitude, one wonders however if the differences/biases of the two heating categories are because of these heating effects, or if it is just a latitudinal effect. Would it be possible to get subcategories of cyclones having their time step of maximum intensification at rather similar latitudes, but still differing substantially in their diabatic heating? Or, similarly, how do cyclones compare with similar diabatic heating but occurring at different latitudes? I think the study would benefit if the latitude effect is somewhat more carefully discussed.

4. Propagation bias for strong-heating cyclones: The authors present in Section 2.4 a good way how to get rid of the propagation bias of strong-heating cyclones, and they also introduce the need to handle this bias in a clear and didactic way by first showing the 'uncorrected' in Figure 2, before showing the corrected ones in Figure 3. I see that the focus of the study is on structural biases, but it would still be of interest to briefly discuss how diabatic heating could lead to these propagation biases. What is the seasonality of this propagation bias? Which processes could be responsible for the bias? I would appreciate at least a brief discussion on this points.

5. Qualitative/speculative vs. quantitative/confirmed statements: The discussion of the sea-level pressure, moisture, wind and temperature biases remain at a rather qualitative level, which is also reflected in many 'could be' / 'might be related' statement. Some specific examples are:

- L163: *"The TCWV deficit is thus consistent with insufficient moisture transport in the warm sector, most likely coinciding with warm conveyor belts."*

- L173: *"This bias might be attributable to error sources from both microphysics and dynamics"*

- L175: *"the bias in TCLW could indicate issues with the microphysical scheme"*

- L197: *"Given that these biases for the strong heating group are primarily associated with the warm sector, the weaker asymmetry and bias could also be due to weak heating cyclones occurring primarily at higher latitudes (Fig. 1b), limiting the moisture supply, resulting in less pronounced biases."*

- L213: *“This negative bias indicates an overestimation of inflow (convergence), which is most likely associated with an overestimated vertical motion resulting from the frontogenesis bias (Fig. 4d).”*
- L243: *“This overestimation of upper level PV is likely associated with enhanced descending along dry intrusions (DI), which can result in larger cold and dry air advection within the cold sector (Catto and Raveh-Rubin, 2019).”*
- L247: *“The underestimations of both the upper-level PV and geopotential are most likely due to misrepresentations in diabatic heating and are thus consistent with biases presented earlier, such as the underestimation in water vapour transport (Figs. 4a,e,f), wind (Fig. 3c, 6a), and temperature (Fig. 6e).”*
- L285: *“This intensification of frontogenesis is likely driven by the overestimated wind speeds within the CCB/SJ/DI region, which enhance the local kinematic deformation and convergence”*
- L287: *“The intensified ascent can lead to an increase in condensation, thereby resulting in the observed positive bias in total column liquid water”*
- L289: *“This may be exacerbated by limitations of the data assimilation (DA), as liquid water is not directly constrained and the DA relies on indirect adjustments to thermodynamic and kinematic fields.”*

The study would benefit a lot if some of these statements (and others not listed here) can be made more strong, e.g., by applying some extra analysis. I don't think that all 'speculative' statements must become hard facts, but at the moment that the number of these 'vague' statements is too large.

6. Processes leading to diabatic heating: In line with point 5), the processes leading to the diabatic heating are not studied extensively. I see that this is **not** the main focus of the study, however, it would benefit from a more thorough process analysis. As a particular example, the diabatic heating and/or cyclone-structure biases are at several places linked to warm-conveyor belts (WCB) and cold-conveyor belts (CCB):

- L150: *“The strong heating group, on the other hand, exhibits a wind speed underestimation only in the warm sector, while wind speed is overestimated in the cold sector over (Fig. 3c), the region usually associated with the Cold Conveyor Belts (CCBs), dry intrusions (DIs), and sting jets (SJs) (Schultz, 2001; Browning, 1997, 2004).”*
- L164: *“The TCWV deficit is thus consistent with insufficient moisture transport in the warm sector, most likely coinciding with warm conveyor belts”*
- L249: *“Overall, these biases are most likely linked to an underestimated strength of the WCB”*

Since cyclone intensity has previously been related to WCB activity in the cyclone's warm sector, and since WCB are associated to strong diabatic heating due to, e.g., condensational heating, it would be 'excellent' if the authors could 'build' a somewhat stronger link to WCB and other airstreams in a cyclone. Optimally, the authors would

link their analysis with a dataset that shows the absence/presence, the intensity and the location of WCB at the time of maximum intensification. Optimally, of course, means that the authors would have such a dataset at hand for all their cyclones. I see that the identification of these WCB datasets is challenging! Still, the authors should consider whether there is a possibility for WCB identification of a subset of their cyclones and to include this analysis then into the manuscript?

Please note also that the following study could be of interest, as it also establish a 'maximum' WCB activity to the time step of maximum cyclone intensification:

Heitmann, K., Sprenger, M., Binder, H., Wernli, H., and Joos, H.: Warm conveyor belt characteristics and impacts along the life cycle of extratropical cyclones: case studies and climatological analysis based on ERA5, Weather Clim. Dynam., 5, 537–557, <https://doi.org/10.5194/wcd-5-537-2024>, 2024.

If the authors agree that this is relevant, they might include in their text, and partly base their answer to my concern on this (and similar) publications.

Specific/minor comments:

- L17: "Our findings highlight the impact of diabatic heating on structural cyclone forecast biases that can guide future model improvements." This statement remains rather vague, and I would either strengthen or remove it from the abstract.
- L41: Which instability? Further, is it 'increasing instability' or is it 'reducing stability'?
- L61: The research question remains somewhat vague, and I would appreciate if some (2-3) very concrete research questions are raised and later discussed in the conclusions. At the moment, the reader only learns that a composite view on the bias will be given.
- L74: 'tangential' and 'radial' relative to what? This becomes only clear in the result section, where the cyclone-centered composites are shown and discussed. There, the wind decomposition becomes clear, but not at this place.
- L85: "to be identified as a closed system" Please explain in 1-2 sentences what 'closed system' means in this context.
- L104: "all fields are rotated so that motion in all composites is aligned along the x-axis" Okay, but I am not completely convinced that the rotation brings a benefit, and other readers might also wonder. Please 1-2 sentence that motivates this rotation.
- Figure 2: The number of wind vectors could be increased?!
- Would it make sense to combine Section 3.1 and 3.3? Both are dealing with winds, and at the moment the paper's structure seems not immediately clear to me. On the other hand, the discussion in L140-147 could also be shifted to Section 2.4. Hence, already discuss in Section 2.4 the need for a position correction (propagation bias). If, on the other hand, the propagation bias itself becomes a feature to be further discussed, I think it is find to keep it here.

- L184: "Through thermal wind, the resulting enhanced temperature gradient strengthens the vertical shear of the geostrophic wind, consistent with the wind speed overestimation (Fig. 3c)." Be careful in using thermal wind balance to *cause* vertical wind shear. The thermal wind balance is a purely diagnostic relationship between horizontal temperature field and vertical wind shear, and – in my view – it is not valid to build any causality on it: Does wind shear react to temperature gradient, or is it the other way round: does the temperature gradient react to wind shear? I would stress that it is neither nor.

.- Figure 3 and 4: Some of the effects are rather small, especially if comparing the strong-heating SLP contours from analysis and forecast. Either the authors could provide some significance test, or they discuss in greater detail the physical relevance of the differences. Note that a difference can be statistically significant, but still physically not relevant (because the difference amplitude is so small). A brief discussion on this would be appreciated.

- Section 3.4: I am not completely convinced that the temperature field needs to be discussed separately?! Possibly, it can be combined with the (qualitative) discussion on the vertical-wind forcing?!