# Response to Reviewer 1

**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.**

We are thankful for the constructive comments from the reviewer. We hope that all concerns have been duly addressed in the revised version of this paper.

Comments by the reviewer are in **bold**, followed by our replies. Figures from the original manuscript are referred to following the manuscript's order, while new figures included in this document are labelled as Fig. AR# (Author Response).

## 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.**

We thank the reviewer for pointing this out. We have clarified the data sources and calculation methods in the revised manuscript (Section 2.3).

Specifically, we address your questions as follows:

Data: The diabatic tendencies are derived from the short-term forecasts of the ERA5 reanalysis (interpolated from model levels to pressure levels). Temperature tendencies due to physical parameterisations are accumulated variables, where we use the same procedure as Tsopouridis et al. (2021); Weijenborg and Spengler (2020). Specifically, they are accumulated ±3 hours around the respective timestep. We calculate the diabatic heating, excluding radiation, by taking the temperature tendencies due to all physical parameterisations and subtracting the radiative heating tendencies.

Exclusion of radiation: Following Papritz and Spengler (2015), radiative tendencies are excluded because they primarily act as a slow background process. Removing them allows us to isolate the rapid, non-radiative diabatic heating (mainly latent heat release) that rapidly enhances local baroclinicity and drives the core dynamics of cyclone development. This focus aligns with our goal of evaluating biases in 12-h short-term forecasts.

Surface and free troposphere: We do not include surface heat fluxes in our metric. Even though diabatic heating from boundary layer turbulent mixing is included, given that our vertical integral is restricted to the troposphere (700–300 hPa), the heating is dominated by latent heat release. Surface latent heat fluxes can play a crucial indirect role by supplying the boundary layer moisture that subsequently condenses and releases heat.

We have explicitly added these details to Section 2.3 to make the methodology more transparent.

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 contributes 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, the can compare the categorization for a subsample with snapshot vs. accumulated tendencies.**

The reviewer raises a valid physical point regarding the accumulation of diabatic effects. However, diabatic heating in cyclones is not evenly distributed but highly localised in time and space. Furthermore, it has been demonstrated that intense latent heat release is concentrated during the rapid deepening phase (e.g., Binder et al. (2016); Madonna et al. (2014)). Furthermore, accumulating heating within a moving spatial domain (e.g., a 750 km radius around the cyclone centre) over time averages over distinct air masses. A rigorous accumulation would require a Lagrangian trajectory tracking, which is beyond the scope of our composite analysis.

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.**

The reviewer raises a valid concern. It is a highly relevant question whether the observed forecast biases are mainly due to the diabatic heating itself or only reflect a latitudinal background effect, given the dependency of moisture availability on latitude.

We conducted a further subsampling analysis by focusing on the latitude band of 45°–55°N. Although the sample sizes differ between the two groups in this band, we applied a Welch's t-test to account for this. Fig. AR1 clearly demonstrates that even when the latitudinal band is confined, the two groups separated by diabatic heating intensity still exhibit distinct different bias patterns. Specifically, the strong heating group maintains a pronounced southwestward displacement bias, whereas the weak heating group only shows a slight westward bias. The areas with statistical significance ($p < 0.05$ indicated by stippling) clearly cover the region of the south-westward bias in the strong heating group. This difference confirms that the diabatic heating acts as an independent factor for the forecast biases, rather than unequal sample sizes, random synoptic noise or latitudinal background effects. We have also added expansions to the discussion in Section 2.3.

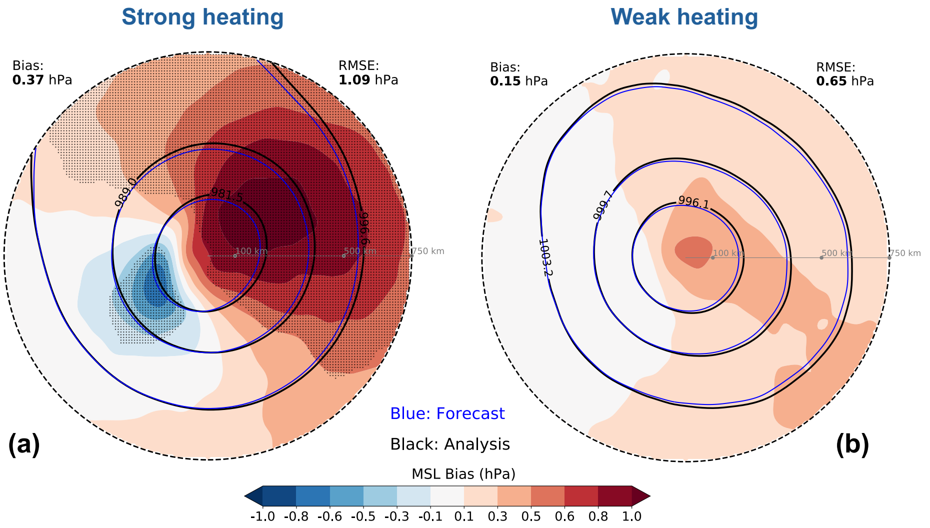

Figure AR1: As in Figure 2, but only for latitude 45°–55°N

4) **Propagation bias for strong-heating cyclones: The authors present in Section2.4 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.**

We thank the reviewer for this highly constructive suggestion. We agree that a brief discussion of how diabatic heating could lead to these propagation biases is valuable for our findings.

We have added a discussion on this topic in the Conclusion section, as follows:

'The pronounced southwestward displacement bias observed in the strong heating group is relevant to the influence of diabatic heating through two processes. First, intense diabatic heating accelerates cyclones (Stoelinga, 1996; Coronel et al., 2015). Thus, the propagation bias suggests a misrepresentation of diabatic heating or its effects in the forecasts, with the bias being more pronounced in the strong heating group than the weak heating group. As the composite cyclones are moving eastward, the westward positional bias manifests a slower movement speed. Second, our results show an underdevelopment of the upper-level ridge exclusively in the strong heating group, which is related to the southward displacement bias (Coronel et al., 2015). The combination of this southward bias and the westward propagation delay explains the distinct southwestward displacement bias in the strong heating cyclones.'

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.**

We thank the reviewer for this constructive observation. We have carefully reviewed the manuscript and agree that using too much phrasing (e.g., "could be", "might be related") weakened the presentation of the results.

Upon reviewing the specific lines you mentioned, as well as other speculative statements throughout the text, we realised that the vast majority of them originate from our interpretations related to the warm conveyor belt (WCB). Because the classic identification of WCBs requires Lagrangian trajectory calculations, it is highly challenging to implement within our current composite framework.

To address this limitation and transform our qualitative speculation into hard, quantitative evidence, we instead use moisture transport axes (MTAs).

As defined by Spensberger et al. (2025), the MTA algorithm explicitly "extracts well-defined maxima in the water vapour transport". As noted in their study, depending on the synoptic context, these coherent moisture filaments are referred to with a variety of names, including atmospheric rivers, warm moist intrusions, and warm conveyor belts.

By applying the same MTA mask as used in the previous study Yu et al. (2025), we quantified the occurrence frequency of these moisture filaments relative to the cyclones for the two different heating groups. This quantitative approach allows us to substantially reduce the speculative content throughout the manuscript. The detailed MTA composite results are presented in our response to Point 6 below.

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 sub-**

**set 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.**

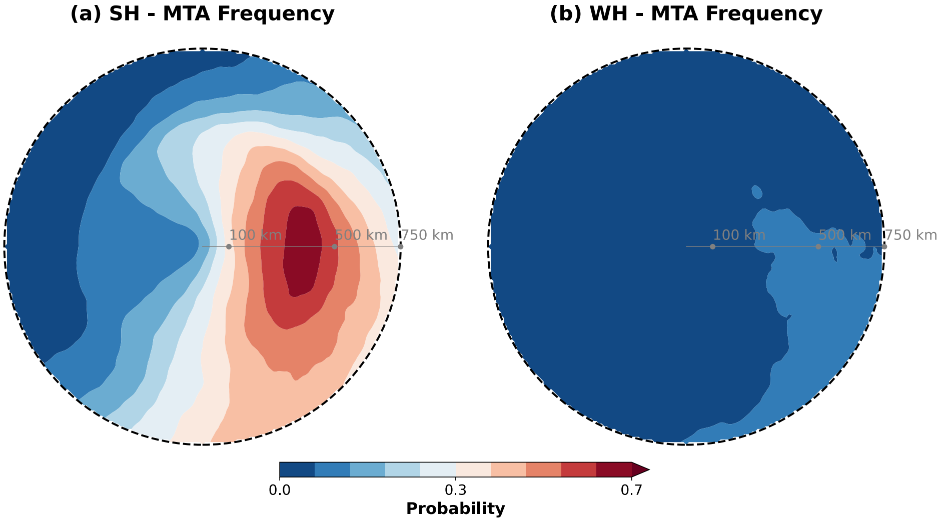

Figure AR2: Cyclone-relative composite spatial frequency of Moist Transport Axis (MTA) for (a) strong heating (SH) group and (b) weak heating (WH) group. A 200-km distance threshold is applied around the MTA line to calculate its spatial frequency.

We sincerely thank the reviewer for this constructive comment. We agree that establishing a stronger link to WCBs significantly strengthens related statements of our study. We are also very grateful for the recommendation of the relevant literature.

As briefly introduced in our response to Point 5, we establish the connection by calculating the occurrence frequency of MTAs for both cyclone groups (Fig. AR2). For the strong heating group, over 70% of the cyclones feature these intense moisture filaments within their warm sectors. In contrast, for the weak heating group, the occurrence frequency is less than 10%. With this clear quantitative proof, we have thoroughly revised the previously speculative phrasing regarding WCBs throughout the manuscript. Furthermore, we agree that Heitmann et al. (2024) is relevant and have added it into our text.

## Specific/minor comments:

- **L17: "Our findings highlight the impact of diabatic heating on structural cyclone forecast biases that can guide future model improvements." This stement remains rather vague, and I would either strengthen or remove it from the abstract.**

  We have revised the final paragraph of the abstract.

- **L41: Which instability? Further, is it 'increasing instability' or is it 'reducing stability'?**

  We have rephrased the sentence by using 'reduces the static 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.**

  We thank the reviewer for pointing this out. To clarify our focus, we have added the specific objectives of this study at the end of the Introduction section. Accordingly, we have also provided direct answers in the concluding section at the end of our study. The added text in the introduction reads as follows:

  'By applying a composite approach, we provide a statistically aggregated view of how different diabatic conditions yield systematic structures in forecast biases. The objective of this study is to identify the link between the intensity of diabatic heating and 12-hour ETC forecast biases. Ultimately, this study helps to understand the shortcomings in representing diabatic moist processes and their related dynamics, which can guide future model improvements.'

- **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.**

  We have provided more detail for clarity. The revised sentence reads: 'In addition, to evaluate the low-level circulation and frontal dynamics, we compute tangential and radial wind components at 925 hPa relative to the cyclone centre, as well as kinematic frontogenesis at 850 hPa ...'

- **L85: "to be identified as a closed system" Please explain in 1-2 sentences what 'closed system' means in this context.**

  We have added a concise explanation in the text as follows: 'All tracked cyclone centres are required to be identified as a closed system at least once within their lifetime, in line with Murray and Simmonds (1991a,b). Specifically, the cyclone centre must be enclosed by at least one closed isobar, ensuring the feature develops into a distinct low-pressure system rather than an open trough.'

- **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.**

  We have expanded the explanation. As cyclones propagate in various directions, rotation is crucial to prevent implicit smoothing due to directions of propagation during the spatial averaging process. The revised text now reads: 'Following (Catto et al., 2010), cyclone-relative composites were calculated by averaging over all cyclones. During this procedure, all fields are rotated to align the propagation direction of each cyclone along the x axis. As individual cyclones move in diverse directions, this rotation is essential to prevent misalignment of features, such as warm and cold fronts, that would then be smoothed out during the averaging process.'

- **Figure 2: The number of wind vectors could be increased?!**

  Thank you for this comment.

  The main purpose of Figure 2 is to illustrate the wind biases. The wind vectors are included simply to indicate the general wind direction. Increasing their number would clutter the plot and distract the reader from the core bias information without adding new insights. We thus decided to keep the vector density unchanged.

- **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.**

  Regarding the combination of Sections 3.1 and 3.3: We carefully considered combining these sections. However, we prefer to maintain the current structure. While Section 3.3 also seems to be dealing with winds, it focuses specifically on the primary and secondary cyclonic circulations. This is tightly related to, and motivated by, the thermodynamic and frontogenetic biases found in Section 3.2. To address your valid point that the structure was not immediately clear, we have added a structurally guiding paragraph at the end of the introduction.

  Regarding the propagation bias: We appreciate the reviewer providing the option to keep this discussion in its current place. We prefer to retain it in Section 3.1 because the propagation bias is one of our key scientific findings. We briefly introduced it in Section 2.4 primarily to provide the necessary context for the re-centring part. We have now added a paragraph discussing this propagation bias in the conclusion section.

- **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.**

  The revised sentence now reads: ’In accordance with thermal wind balance, the resulting enhanced temperature gradient corresponds to a stronger vertical shear of the geostrophic wind, consistent with the wind speed overestimation (Fig. 3c).’

- **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.**

  We have added the relevant discussion to the first paragraph of the Conclusion section.

  ’This study employs a cyclone-centred composite framework to quantify short-term (12-hour) forecast biases for wintertime (DJF) maritime extratropical cyclones (ETCs) within the ERA5 for the period 1979–2022. To compare the influence of diabatic processes, cyclones are categorised into groups based on strong and weak intensity of domain-averaged diabatic heating. Overall, 12-hour forecasts of North Atlantic extratropical cyclones underestimate cyclone intensity at the time of maximum intensification. Notably, the bias patterns differ distinctly between the strong and weak heating groups. Although the 12-hour forecast biases are of small absolute amplitude, their physical relevance is significant: cyclones with stronger diabatic heating manifest more pronounced systematic biases tied to specific physical processes. Specifically, while forecasts underestimate cyclone intensity near the cyclone centre for the weak heating group, forecasts for the strong heating group not only underestimate cyclone intensity but also feature a propagation bias, manifesting as a southwestward displacement of the cyclone position.'

- **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?!**

  We agree with the reviewer that the temperature field can be combined with the discussion of other fields. Accordingly, we have integrated the temperature discussion into Section 3.3. This restructuring creates much more cohesive and logically sequential content, as demonstrated in the revised text below: 'These cyclonic circulation biases are associated with the thermal structure through altered temperature advection. The temperature bias for the strong heating group shows a pronounced cold bias in the upper right-hand quadrant. This is consistent with the underestimated cyclonic flow and water vapour transport in the warm sector, thus featuring reduced warm air advection (Figs.3c, 4e, 4f, and 6a). Additionally, the lower-left quadrant also exhibits a notable cold bias. This cold bias is linked to the upper level potential vorticity (PV) biases discussed in the following section.'

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