The title of this manuscript does well to summarize what to expect. The overall quality of the manuscript is good. The writing is clear. The authors have carried out a substantial amount of application of existing Lagrangian tracking algorithms to reanalysis and climate model data, and then they have done some interesting sorting of the data. Ultimately the results suggest only a small signal amidst the noise of midlatitude storms. However, for the important issue of explosive cyclones, perhaps a null result is still useful. As ever, I do think we need to be cautious because there is always the lingering doubt about these model's ability to capture the physics of explosive cyclones.

I appreciate the author's choice on method of tracking ARs using the Laplacian of the IVT, so that they are not just picking up the thermodynamic signal. However, I have a fundamental issue with the way in which some concepts are explained in the introduction, and some questions about the interpretation of the results. These issues and questions are described below.

First of all, we would like to thank the anonymous reviewer for the helpful comments on the manuscript.

Major Comment:

Lines 37 – 41: This is a section in the introduction in which the authors seek to make a physical explanation for why the presence of atmospheric rivers (ARs) impact explosive cyclones (ECs). However, I do not think these studies prove cause versus effect. I posit that in many, or perhaps half of the cases, it might be the case that rapidly intensifying cyclones have substantial upper-level forcing that drives more poleward transport of water vapor. This would lead to more ARs found in the surroundings of ECs, but the cause is not the upper-level circulation, not the latent heat release. (Isn't this substantiated by your result that more ARs are found to be associated with the cyclones after their maximum deepening point? – line 147.)

I want to make clear about my point: If the upper-level circulation is held fixed (e.g., in a modeling study for a single event or a baroclinic wave), then the storm intensity and intensification rate will increase with more water vapor (i.e., the presence of a stronger AR). However, that is different from saying that the presence of ARs leads to explosive cyclones. For me, the explanation provided by the authors in this section needs more nuance and explanation.

Relatedly, the papers being referenced in this section all state that their results "suggest" a relationship, but none of them claim it to be conclusive. So, I request that the authors add more caveats and details to this explanation. This would impact the introduction, the interpretation of results and the conclusions.

We thank the reviewer for their valuable feedback and agree with the concerns raised. In response, we have revised lines 37–41 to reflect the suggested changes. Additionally, based on comments from other reviewers, we recognized an issue with the tracking of cyclones. Specifically, we did not account for a buffer zone, which affected our results regarding line 147 and the peak of intensity after the maximum deepening rate. Upon revising the plots, the peak has now been perfectly aligned with the maximum deepening rate of the cyclones, consistent with previous studies and the theory outlined in the introduction.

Lines 37–41: The climatological relationship between ECs and ARs has been previously studied and the literature evidences that ARs are more often found in the surroundings of EC than non-ECs (Eiras-Barca et al., 2018; Zhang et al., 2019; Guo et al., 2020). **ARs are important sources of moisture for cyclonic systems, and it has been suggested that they can enhance cyclone deepening through moist diabatic processes (Ferreira et al., 2016;), such as cloud condensation (Pinto et al., 2009). However, the extent to which these moist diabatic processes, compared to other factors such as upper-level forcing, influence cyclone intensification can vary from case to case (Ginesta et al. 2024).**

Minor Comments:

Line 180: Figure 4 (and all similar plots): I suggest you replace h with the word hours to reduce any chances for confusion from a viewer.

We will change Figure 4.

Line 215: I am a bit puzzled by the AR intensity analysis in Section 5.2. In the methods section, you do a good job of explaining why the use of the Laplacian is important. Now you are back to working with IVT itself. Why? Given that storm forcing from latent heating (e.g., the change in diabatic potential vorticity) is related to the gradient of the heating, not the absolute value, this choice of defining AR intensity based on the absolute value should be explained in more detail.

The laplacian of IVT was used only for AR detection. We use the IVT itself because we want to quantify how much the intensity of the ARs will change in the different future scenarios. The IVT is the most used variable to study AR intensity, is well correlated with cyclone intensity and is also a proxy for the potential amount of precipitation (Ferreira et al. 2016; Guan et al. 2023). Our aim in this study is to assess changes in future scenarios of ARs and ECs, we acknowledge that our study has a limitation in giving a physical explanation for the intensification mechanisms between them, in this context studying the gradient of IVT would be a good way to do it. For our purposes, we believe that the IVT-max might be a better-fitting variable and will facilitate comparison with other studies of ARs in climate projections (Zhang et al. (2024)).

Line 247-8: Here you state:

“The results from ERA5 show the same behaviour for both types of cyclones but with lower intensity”. Could you clarify this sentence to explain what intensity is referring to? Is it the intensity of the relationship or the intensity of the cyclones? If it is the intensity of the relationship, then perhaps you should also include a sentence or two here reminding the readers of the multiple reasons for potential biases in the models.

We agree that the original sentence was unclear. We will added: “**The results from ERA5 show similar behaviour for both types of cyclones when compared to the models. Before the MDP, the models tend to simulate lower SLP for ECs with ARs and higher SLP for ECs without ARs. After the MDP, the models generally simulate higher SLP for both ECs with and without ARs. For non-ECs, the models have higher SLP values after the MDP compared to ERA5. However, the ERA5 values fall within the ensemble spread of historical values, indicating that they are within the uncertainty range of the models.**”

Additional papers on water vapor and storm intensity that must be cited and discussed when discussing the results, given the nature of this manuscript:

Pfahl, S. and Sprenger, M.: On the relationship between extratropical cyclone precipitation and intensity, Geophys. Res. Lett., 43, 1752–1758, 2016 https://doi.org/10.1002/2016GL068018

Booth, J. F., Naud, C. M., and Jeyaratnam, J.: Extratropical Cyclone Precipitation Life Cycles: A Satellite-Based Analysis, Geophys. Res. Lett., 45, 8647–8654, 2018

https://doi.org/10.1029/2018GL078977

Sinclair, V. A. and Catto, J. L.: The relationship between extratropical cyclone intensity and precipitation in idealised current and future climate, Weather and Climate Dynamics, vol. 4, no. 3, pp. 567–589. doi:10.5194/wcd-4-567-2023, 2023

We thank the reviewer for the suggestions. We will cite them accordingly.

Additional References:

Guan et al. (2023): https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2022JD037180

Ferreira et al. (2016):
https://www.sciencedirect.com/science/article/pii/S1474706516000048

Zhang et al. (2024): https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2023JD039359