This manuscript aims to evaluate the current climatology and assess changes under future climate scenarios of the concurrence between atmospheric rivers and extratropical cyclones undergoing explosive development – frequently referenced as explosive cyclones – in the North Atlantic. Being the explosive development of extratropical cyclones and atmospheric rivers crucial in driving extreme weather in the mid-latitudes, this topic is relevant, deserves to be investigated and it fits the scope of the Earth System Dynamics journal.

The manuscript is well-structured and well written and it is pleasant to read. It applies well-known datasets and detection and tracking methods previously published and discussed in the literature. However, in my opinion, some points are too succinct and need further details and explanations before the manuscript is accepted for publication.

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

Some points that need further clarification are:

- The authors use TempestExtremes Code for Detecting and Tracking Extratropical Cyclones and Atmospheric Rivers (ARs) for the North Atlantic region [25-65ºN; 80◦W-10◦E]. In Appendix “A4 Number of Cyclones and ARs detected in ERA5 and CMIP6” the words Cyclone and track are used as synonyms; in Figure 1 “EC track density climatology” is presented and “Units are the number of cyclones per 1.5◦ spherical cap per month”; and in Figure 2 “AR frequency climatology” is presented with “Units are the percentage of time-steps detected as AR”. Before a climatology can be presented and discussed, a clarification must be presented, and methods should detail the definitions and how this has been computed. In both cases, the authors must clarify if the systems are being tracked and considered or if the systems’ timesteps are considered independently. All the processes to produce the cyclones’ tracks and ARs datasets must be better explained. From my understanding, the systems are considered, but this is not clear from the discussion and captions of Figures 1, 2 and 8.

This is correct (the systems are being tracked and considered), we agreed the methodology needs clarification. We will ensure that the text clearly states what is being referred to in each case and will harmonise the nomenclature for consistency. Additionally, we will expand the Methods section to describe how the climatologies presented in Figures 1 and 2 are calculated. In the main body of the manuscript (including Figures 1 and 2), we refer to the time steps of the tracked systems (the systems' time steps are considered independently). Only in Appendix A4 do we refer to the total number of tracked systems (the systems are being tracked and considered).

- The North Atlantic region [25-65ºN; 80◦W-10◦E] is considered. The authors should discuss the artefact over the western boundaries of the domain. A buffer area should be considered for the identification and tracking of the systems.

Thank you for highlighting this issue. We acknowledge the artefact in the western boundary of the domain in cyclone climatology. We identified that the cyclone tracking algorithm creates this issue specifically at the western boundary, where cyclones move eastward as they enter the domain. It creates stationary "artefact" cyclones that have

their MDP along the boundary. To address this and ensure it does not affect our results, we will apply a 10º buffer zone at all boundaries of the domain. The new tracking domain will be 15-75ºN, 90ºW-20ºE, while for the analysis, only the time steps of tracks within the original domain [25-65ºN, 80ºW-10ºE] will be considered.

We found that this issue was impacting the concurrence results in Figures 4 and 5, where the peak of concurrence was initially 6 hours after the MDP. By correcting this issue and adding the buffer zone, the peak of concurrence now aligns with the MDP. This correction brings our results in line with those of Eiras-Barca et al. (2018). The previous shift in the peak of concurrence was due to the "artefact" cyclones having the MDP at the boundary meaning that these cyclones had the MDP at the first time steps of their tracks. This shifted the curves in Figures 4 and 5 to the right as the "artefact" cyclones were adding a bias only to the times after the MDP. All results that depend on the tracking will be updated.

- The method to identify the concurrences of Extratropical Cyclones and ARs also needs further explanation: it is presented through Figure 3 and the Xynthia case study, but this example elucidates the doubts and need for clarification on the methods. From this example, five timesteps are consistent with the concurrence of the cyclone under explosive development and the occurrence of the AR. The shaded areas in Figure 3 that depict the regions identified as ARs should have some correspondence with the cyclone track and should be described in the text as well. It is not clear to me how many times cyclone Xynthia and the concurrent AR are considered for the climatological assessment. I would say Figures 1 and 2 correspond to timesteps – and not Cyclones/AR.

We will improve the description of the methodology to make it as clear as possible. We will also modify Figure 3 (see also reply to Mika Rantanen) to make it more useful to understand the methodology. As you suggest, we will include in Figure 3 which AR (shades) correspond to which time step in the cyclone track (crosses). Each cyclone time step and the concurrent AR (or not) is only used once in the climatology assessment or further in the following results sections (same for Xynthia).

- As mentioned previously, the method to identify the concurrences (Ln 130-134) must be further detailed. Please discuss the choice of the Maximum Deepening Point (MDP). An explanation should be given for the choice of the 1500 km threshold. It is not clear if a sensitivity analysis was performed, nor if this metric is constant for all cyclone's sequential timesteps. To the best of my understanding, each detected AR candidate may have more than one grid point being detected as AR in the same timestep. Certainly, the authors have considered this and all these aspects should be presented and discussed in the methods section. Additionally, how this method differs from Eiras-Barca et al. (2018) should be highlighted.

Thank you once again for your insightful comment. We will revise the text in the Methods section for clarity. We selected the maximum deepening point (MDP) as a time reference because it allows us to better assess the influence of ARs on cyclone cyclogenesis. Additionally, using the MDP makes our results directly comparable to those of Eiras-Barca et al. (2018).

Regarding the choice of the 1500 km threshold, I kindly refer you to our response to the first comment in our reply to Mika Rantanen. As you noted, our methodology aims to align as closely as possible with that of Eiras-Barca et al. (2018) to ensure comparability. The main difference lies in the tracking methodologies for ARs and cyclones, as we use different tracking algorithms but still, those algorithms share similar configuration parameters. However, in terms of cyclone/AR concurrence detection, explosive versus non-explosive cyclone classification, and the calculation of the MDP as a time frame, we follow the same methodology. We will emphasize all these points in the Methods section.

- The clarification of these methodological aspects is vital for the discussion of the results: authors should clearly state if this study evaluates "the concurrence of ECs and ARs in the ERA5 reanalyses and we compare it with those obtained in climate models" considering only once each EC and AR.

We will emphasize in the text that we only consider once each cyclone (EC or non-EC) and AR time step in our analysis.

- All the analysis is performed for the extended winter period (October to March). This should be indicated in the figure captions. Please clarify, in the methodology, how the inter-seasonal variability is defined if only the extended winter season is considered. I suppose the authors mean interannual variability.

Thanks again for helping us to explain better the methodology. We will add in Figure captions that the period analyzed is extended winter (ONDJFM).

We mean interannual variability (variations between extended winters in different years), we will change interseasonal for interannual everywhere in the manuscript. The interannual variability is defined as the standard deviation among the 30 years of each period, we will add this definition in the methods section.

- CMIP6 models' information and discussion of results: additional detail should be included for the choice of one single member for each model and not the ensemble – how this particular member has been selected and how this choice may affect the final results. This should be included in the methodology and the discussion. Please refer to whether one may state that model X overestimates/underestimates the results or if model Y is more adequate for the analysis if only one member has been used. Please also discuss if the biases quantification is reliable. To the best of my knowledge, this assessment is not enough to make a comparison between models. A multimodel ensemble framework with varied combinations of GCMs is extremely useful and allows for reducing the uncertainty in climate projections for future scenarios and for a tendency assessment, but it can hardly be used to intercompare models when only one member is used. Please, define "the internal variability of the datasets" (ln 161) in the methodology section and how it is assessed in this manuscript.

The limitation on the number of CMIP6 models and ensemble members is explained in lines 80-82. We will further elaborate on this in the Methods section and include it in the Discussion and Conclusions. We were restricted to using one member per model because the other ensemble members did not have the necessary variables to calculate IVT, which is essential for studying ARs. In essence, we used all available members from

CMIP6 models that had the required variables for the historical period and the three scenarios.

We acknowledge, both here and in the manuscript, that the limited number of members is a limitation. For this reason, we assess changes between the present and future using the multi-model mean of the ensemble (ensemble of 6 members from 6 different models). We also agree that stating a particular model overestimates or underestimates results may not be appropriate when using only one member per model, and we will revise this wording in the text.

In line 161, we refer to “the internal variability of the datasets” as the model's spread, or in other words, the spread within the multi-model ensemble. We will clarify this in the manuscript.

- It would be useful if the results presented in the Appendix should be accompanied by a short description and discussion. Please avoid using expressions like “little change” (ln 190-191) or “lower-than-expected increase” (ln 194)– please quantify. A percentage could be added to tables.

Thanks for the comment, we will add a description and improve the information in the Appendix. We will review the text to avoid these expressions.

- Ln 200 – this sentence deserves additional information or a reference. It is out of context in this paragraph. These would be relevant results but evidence must be shown.

We will provide additional context for this sentence. For a more detailed explanation, I kindly refer you to our response to Mika Rantanen.

- Conclusions: please discuss what is the novelty, for the present period, from the literature. The sentence “The fact that AR concurrences are larger after the MDP suggests that mature ECs (when they are deeper) can facilitate the formation of ARs in their surroundings” (ln 272-273) deserves to be further discussed and justified. Firstly, it is well known that the detecting and tracking methods still have large uncertainty in detecting the absolute minimum central pressure of an extratropical cyclone; secondly, the difference should be quantified; finally, and most importantly, the only conclusion that these results allow us to obtain, in this state, is that additional AR are detected – we cannot state that they only formed at that particular timestep.

Thank you for your comments. We will ensure the novelty of our results is emphasized. In lines 272-273, we intend to convey that as more ARs are concurrent with ECs around the MDP, this suggests that ARs are more likely to occur when the EC is at its maximum deepening stage. This finding is supported by other studies, such as Zhang et al. (2018) and Eiras-Barca et al. (2018). This result holds true for both the historical period and all future scenarios. Additionally, we demonstrate that concurrences increase across future scenarios at all stages, not just around the MDP.

We agree and acknowledge that tracking algorithms come with inherent uncertainties. We have quantified the number of individual cyclone tracks detected (Table A2 in Appendix A4) and will highlight and expand these results in the text. As mentioned in a

previous comment, we will also include the percentage changes in the table. While our results have not identified clear changes in cyclone track counts, we believe that despite uncertainties introduced by tracking algorithms, our results (among other findings) confidently indicate an increase in the coincidence between cyclones and ARs, as well as an increase in ARs and their intensity.

References:

Eiras-Barca et al. (2018): https://esd.copernicus.org/articles/9/91/2018/

Zhang et al. (2018):
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GL079071