This manuscript investigates the concurrences of explosive mid-latitude cyclones and atmospheric rivers and their future changes in the North Atlantic. The authors use ERA5 and CMIP6 climate models, tracking software, and study the link between ECs and ARs in the present and future climate. The main finding is that the concurrences of ECs and ARs is going to increase in the future, regardless that the number of ECs themselves show a decreasing trend.

I enjoyed reading the manuscript. I think it's very well written and easy to follow. The language was good, and the used datasets and methods are appropriate for the design of the study. Thanks to the authors for that. I also think that the topic of the study falls within the scope of the journal.

Despite the overall good presentation of the manuscript, I have some minor concerns related to the methods and the main result. I have listed them below, in addition to some line-to-line comments. I hope these are of help to the authors and they can address them before the paper is published.

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

1. Methods. At L134 you say that the ARs are detected within a 1500 km radius from the cyclone centre. At quick thinking, it sounds quite a large distance, given that e.g. Rudeva and Gulev (2007) found that the effective radius of oceanic ETCs is about 900 km. In the Introduction, you write (probably correctly) that the release of latent heat by the moisture of the ARs is an important mechanism in deepening the ECs. For me, it feels that if the AR is located very far (such as > 1000 km) from the cyclone centre, it cannot be involved in the deepening of the system. So, how did you arrive at the 1500 km value, and have you investigated how sensitive your results are to the used distance?

Thanks for this comment. The main reason to use the threshold of 1500 km for the radius detection of ARs in the surroundings of a cyclone is to be able to compare our results with the study of Eiras-Barca et al. (2018), where they evaluated the concurrence of ARs and cyclones with this radius for the historical period but using different tracking algorithms. In addition, we believe that the moisture brought by an AR (even if this is located further than 900 km) still influences the cyclone as in many cases is not the AR delivering its moisture directly to the cyclone centre but is the WCB or the feeder airstream that connect the enhanced moisture area of the AR with the cyclone and ultimately enhancing its intensification (Dacre et al. 2019). For this reason, ARs within 1500 km of the cyclone can contribute to its deepening as other airflows within the cyclone transport the moisture that potentially contributes to intensification. Finally, we tested the distribution of AR around the cyclone centre and detected a decline of ARs after 1500 km, this motivated us to keep the 1500 km already used by Eiras-Barca et al. (2018) and would keep our results comparable to theirs. Will provide this test and the sensitivity of the detection radius.

2. Results. Perhaps the headline result of your study is that the concurrences of ECs and ARs show an increasing trend in a warmer climate. In Sect 5.1 you

discuss that the number of individual ECs show a downward trend with climate change, and also that the change of detected ARs is very modest, almost flat, in a warming climate. At L201-204 you say that the increase of concurrence in a warmer climate points to the changes in characteristics or ARs or cyclones, i.e. as I understood, changes in the dynamics.

In any case, I was still missing a more detailed explanation or mechanism of how the concurrence of ECs and ARs can increase with climate change when neither individually shows a clear upward trend (I think this is a rather important finding which I haven't heard before!). I understand it may be challenging to find any clear explanation, but in the absence of one, it would be good to at least state out loud this dilemma clearer, for example in the conclusions or in the abstract. Now it feels like it is being swept under the rug as it is only briefly mentioned at L202-204 and not again in the conclusions.

Thanks for the comment, we agree that would be great to have a physical explanation of the results found here, and we hope this study encourages future research in this direction. Unfortunately, our results we cannot state which are the reasons for these changes but we will expand this in the discussion and add it to the conclusions.

Line-to-line comments:

L42. "Extratropical Cyclones". Do you mean explosive cyclones? If not, please decapitalize.

We mean extratropical cyclones, we will decapitalise.

L45. "undergoes further amplification compared to surface water vapor". I'm not really sure what you mean by further amplification and why this is the case. This sentence could be rephrased.

We mean that integrated water vapour (IWV) is expected to experience a larger increase than surface water vapour under climate change. We will rephrase the sentence to make it clearer.

L73. Why did you select 2009 as the ending year? Was that because you wanted to have 30 years in the historical period, consistently with 2070-2099 for the future? Please add an explanation to the text. And also, does your time range include OND 1979, i.e. should it be ONDJFM 1979/1980 - 2008/2009?

We used until 2009 for the historical period to have 30 years of data [1980-2009], the same as for the future periods [2070-2099]. The data includes 30 "natural" years and from there we subtract the winter months, in total 180 months of data each period. This means that each period has 29 full winters (ONDJFM) and two half winters, JFM for the first year and OND for the last year.

L95. What's the unit of NDR? Isn't it hPa / h. Now there's no unit after 1.

The NDR does not have units, it is a dimensionless variable (Lim and Simmonds, 2002). Apologies for the confusion, formula (1) in the denominator of the first fraction should be 24 hPa (not 24 h). The DR_24h unit is also in hPa, thus when divided by 24 hPa the result is the dimensionless index NDR.

L99. Here you start to speak about the number of cyclones detected. However, I think you should more clearly repeat the domain of tracking. The domain of ERA5 was presented at L74. Is this the same domain where you apply the tracking software?

The domain where the tracking is applied is the same for all datasets [25-65ºN; 80ºW-10ºE], now we will apply a buffer zone of 10º at each boundary of this domain (ERA5 and all CMIP6 models). Thanks for this comment, we will be more clear with the tracking domain in the manuscript. For further explanation of the tracking area and the implementation of the buffer zone, I kindly refer to our answer to the second comments of Referee 2 for a more detailed explanation.

L99. How do you treat those cyclones which form or decay outside the domain and only travel across it? How can you be sure what the MDP of a given cyclone is if only part of the cyclone's life cycle occurs inside the domain?

First, cyclones can only have their tracks inside of the domain [25-65ºN; 80ºW-10ºE], and the MDP is calculated with these tracks so the MDP corresponds to the part of the track inside the domain. If a cyclone has the MDP before/after entering/leaving the domain we could not detect that MDP. Now, with the implementation of the +10º buffer zone at the boundaries, the tracking is applied to a larger domain, and the MDP is calculated for the tracks in the extended domain with the buffer area. Then, the analysis only includes the parts of the tracks in the original domain [25-65ºN; 80ºW-10ºE], thus MDP can be outside of the domain but the analysis is only done with the part of the track inside. This ensures that we detect the correct MDP, and improves the representation of the cyclones that form or decay outside the domain.

L117-118. These threshold values seem a bit subjective. How did you arrive at them? I think the justification for these values should be mentioned.

These thresholds are defined and tested by the TempestExtrem developers, we use the same as in Ullrich et al. (2021). In addition, we add the threshold most used in AR detection and tracking algorithms (IVT>250kg/m/s) for validation. The TempestExtrem tracking algorithm used in our study has been compared to other tracking algorithms showing a large agreement (Collow et al. 2022). Thanks again, we will add this to the manuscript.

L118. "The detected candidates are concatenated if at least one grid point is detected as AR in sequential timesteps". Does this mean that the AR area at the next time step must overlap spatially with the AR area at the previous time step?

Exactly, we will improve the sentence to make it more clear.

L134. The presence of AR. I understand that the location of the EC is clearly defined, i.e. it means the location of the minimum SLP. Yes. But it is unclear to me how the location of the AR is defined in relation to the EC. If the AR is wide, does that mean that it is sufficient if the closest grid point of the AR is at most 1500 km from the centre of the cyclone?

Yes, or in other words: if at least one grid point detected as an AR is within 1500 km from the centre of the cyclone, then that cyclone is concurrent with an AR. We will improve this sentence to make this clear.

Fig. 3. Does the map show Xynthia's full life cycle, the whole track? Not a big deal, but I missed the MDP of Xynthia. Can you show its location on the map? I think it would better tie Fig. 3 to the following figures where the time axis is shown in relation to MDP.

Yes, it shows Xynthia's full track (the track we detect with our algorithm). Thanks for the recommendations, we'll include the MDP point and time with respect to the MDP.

L146-147. "Initial stages of the cyclone formation / dissipation stages of the cyclones". In Fig. 4, you show only 72 hours (3 days) of the cyclone composites. Arguably many of the tracked cyclones last longer, meaning that their formation or decay can be days before/after MDP. Figure 7 shows that, on average, the minimum SLP of the cyclones has not increased much even 36 hours after the MDP. So is it correct to speak of dissipation in this context? Could it be better to just say 36 hours after / before MDP?

Thanks for this comment, other reviewers also pointed this out. We agree that formation/dissipation stages might not be appropriate as we analyse the cyclones +/-36 hours from the MDP. We will change this and modify the text accordingly to avoid referring to formation/dissipation stages in this context.

L157 and hereafter. For me, inter-seasonal means variations or comparisons between different seasons within a single year. So basically changes and differences from one season to another within the same year. Whereas inter-annual refers to variations between the same periods in different years, i.e. year-to-year differences. Do you mean inter-annual here and later in the manuscript?

Thanks again for this comment, other reviewers also ask the same. We refer to variations between the same periods in different years (we only analyse extended winter). We will change inter-seasonal for inter-annual in all the manuscript.

L174. What does the internal variability of the datasets mean in this context? I think the sentence needs rephrasing.

It means inter-annual variability, we will correct that.

L177. quantitative We will fix the typo, thanks.

Fig. 4. Could it be written to the panels the a-b represent averages over 30 years, and c-d standard deviations? It took me a while to understand that the panels c-d are

standard deviations, especially as I got confused about the meaning of “inter-seasonal” in their title (see the comment a few comments back).

Thanks for the comment. We will improve the clarity of Figure 4 regarding this, and we will make sure that we use inter-annual variability throughout the manuscript and make sure it is well explained what it means.

L184 and so on: 12 % or 12 percentage points?

12 percentage points. We will re-write this accordingly.

L192. “for almost all the models”. Well that’s one way of saying it if 4 out of 6 models are showing an increase of AR tracks between SSP5-8.5 and historical. In general, the values in Table A3 seem very unchanged, so I think it could be honest to say that there is not really a systematic change at all between the scenarios and historical. It could be just random variation (internal climate variability).

Thanks for the comment,

L199. Does this sentence refer to Fig. 5a? The reference to the figure could be added.

Yes, we will refer to Fig. 5a in the text.

L200. By the number of compound events do you mean a situation where at least one time step of the track of the cyclone centre is closer than 1500 km to the AR?

We mean that the total number of time steps that a cyclone (EC or non-EC) is concurrent with an AR increases. Before and in Fig.5 we show an increase in the rate of coincidence (increase in the ratio), here we want to emphasise that the total number (or absolute number) also increases. Because an increase in the ratio could be due to a decrease in the total number of cyclone time-steps, but not necessarily an increase of concurrent cyclone/AR time-steps.

L207. the inter-model spread of the SSP5-8.5 scenario Will correct this.

L228. CMIP6 Will correct the typo. Thanks.

L249. “... is very limited”. I think you could continue this sentence by for example “as the coloured lines in Fig. 7 are close to each other” or similar. It took me some time to understand where you got this conclusion.

We will expand this sentence and refer to Fig. 7 for better clarity.

Fig. 7e. I think you do not discuss at all why CMIP6 models seem to be more sensitive to the ARs than ERA5? Or did I understand it correctly? Why the coloured lines in Fig. 7e go much lower before MDP and much higher after MDP when compared to ERA5?

We will add this in the discussion of the results of Figure 7.

Conclusions. Currently, I think the conclusions (and in fact the whole paper) paper puts quite a lot of emphasis on the high emission SSP5-8.5 scenario. However, it has been shown to be unrealistic (https://www.nature.com/articles/d41586-020-00177-3), and the world is currently roughly on the path of the SSP2-4.5 scenario. It might be appropriate to add a few sentences of discussion on this, stating that the results should be interpreted always with the scenario in mind, and that the results of the SSP2-4.5 scenario are more likely in the future than those of SSP5-8.5.

Thanks for this point, actually our motivation to analyse 3 different emissions scenarios was that some of those can be unrealistic, thus we believe is more important to study more than one scenario. We will add this in the text and will make sure that results for all scenarios are discussed.

L283. SSP5-8.5 scenario Will correct the typo. Thanks.

Table A2-A3. It could be helpful to add the periods (the year ranges) used for historial and SSP scenarios also here.

We will do it, I hope it will help to interpret the table.

References

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Additional References:

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