Reply to review RC1

This study discusses the ARs over the North Atlantic from ensembles of 24 global climate simulations following the greenhouse gas scenarios RCP2.6, RCP4.5, RCP8.5 downscaled using a regional climate model (RCA-NEMO) with 0.22° resolution and the results are compared with against ER-I reanalysis data. The study finds that ARs would become more frequent and more intense in a future warmer climate especially in the higher emission scenarios under the assumption of RCP2.6. They also propagate further inland to eastern Europe in a warmer climate.

Though I am yet to complete the review, here is a major comment on the detection of atmospheric rivers. Authors have mentioned that they employed Lavers et al. (2012, 2013) method to detect ARs based on the 5 degrees binning along 10 degrees west. Though it is a well-known approach, one might see the spatial "patchy" and "noise" at a given time step in the AR detection (figure 1 in Lavers et al., 2013). To be specific, one might expect that a high-resolution data detection algorithm could retain values over a few grid points that satisfy the binned threshold but do not satisfy the AR criteria. This noise in turn would cause bias in comparing the long term (climatology etc) spatial variability among different models. Also, the authors mentioned that the ARs with 18 hours of persistence were considered. But I do not see any description of finding persistence. Hence, authors are encouraged to provide more details on these issues in the manuscript.

We thank the reviewer for this important note. It's difficult to properly extract this kind of noise which would require to develop advanced methods of filtering etc. This would be out of the scope of the present manuscript. However, we agree that this should be mentioned in the section "3.4 Effect of downscaling". We included the following paragraph at the end section 3.3.2 (line 422):

"Finally, we note that the comparison between different spatial resolutions might also reflect different noise levels. This noise occurs when isolated grid points located outside the AR at a given time step exceed the IVT threshold but do not satisfy the geometric and temporal requirements. The different noise levels would then contribute to the total effect of downscaling."

We regret the confusion with the description of the algorithm. In the new version the algorithm is now described step by step in section "2.3 Detection of atmospheric rivers".

With persistence we mean "duration", i.e. when the algorithm detects an AR for less than 18 hours (corresponding to 3 consecutive output time steps), then these time steps are not retained and the atmospheric river is no longer classified as such (following basically Lavers et al (2012, 2013). We have made clear this in the revised version (step 7. in section 2.3).

Reply to reviewer RC2

This paper investigates the behavior of North Atlantic ARs in reanalysis and ensembles of global simulations in historical and future simulations. The manuscript is in good shape. It is well-written and has a clear structure. I don't have major concerns for the paper, but some minor suggestions and comments for discussion:

We thank the reviewer for a thorough review of our manuscript and his/her valuable suggestions that really helped to improve the manuscript.

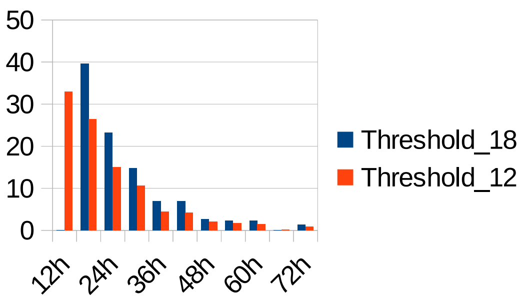
L193: Guan and Waliser (2015) does not provide a thorough review of AR detection methods, instead, GW15 explained their AR detection algorithm in detail. Since GW15's algorithm is not adopted in this paper, it is better to cite a different paper as the "overview". For example:

Shields et al. (2018): Atmospheric River Tracking Method Intercomparison Project (ARTMIP): project goals and experimental design, Geosci. Model Dev., 11, 2455-2474, https://doi.org/10.5194/gmd-11-2455-2018, 2018.

Thank you for this correction. We have replaced Guan and Waliser (2015) by Shields et al., 2018 in the revised version.

Figure 4: I wonder how the duration distribution would change if the duration threshold to tuned to 6 hours or 12 hours. How will the duration (including those excluded short-lived events (<18hrs)) change in the future climate?

We have have recalculated this exemplary for the MPI-ESM realization. The main effect is, as expected, an increase of about 55% in the total of detected ARs. The left hand figure below compares the distributions calculated with a threshold set to 18 hours (orange) and 12 hours (blue). Generally, both distributions are similar and indicate most ARs in the short term fraction (with >50% ARs lasting 30 hours or shorter). The right hand figure displays the relative change for the RCP8.5 scenario [%, i.e. future minus historical period *100 / historical period)]. In both distributions the tendency towards extra long (48 hours or more) ARs can be seen. However, these extended ARs remain typically rare (<10%) in both historical and future climate.



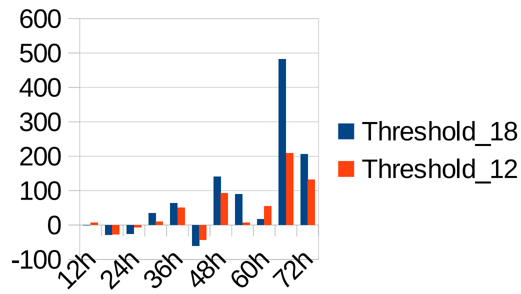


Figure 5a: instead of showing the actual number of AR days, it might be helpful to show the percentage of AR days to the number of days of the period.

We fully agree. In the revised version we have adopted Figure 5a accordingly. Likewise Figure 8a which displays the change in AR day frequency in a future warmer climate is adopted in the same way.

Table 3 and L367: Is it possible that the dynamical field is more active in the future simulation as well?

Table 3 shows that the number of AR increased in the future runs: it makes sense if the historical AR threshold is applied in the future run – however, here the future ARs are detected with IVT thresholds calculated from future simulation, so the higher moisture load is reflected in the future IVT threshold to some extent. Therefore, I am curious if the change in dynamical filed also contribute to the increase in future AR number.

Yes, it is likely that both dynamical as well the thermodynamical changes contribute to the increase in AR number. Hence, it could be that more ARs are initiated in the source region (outside our model domain) or that the pathway from the source area to Europe changed. This can not be analyzed in our regional model. However, this question was addressed in global CMIP5 models (which are more suited for this question). Lavers et al. (2013) argue for mostly thermodynamical changes (moisture increase) for the future increase in ARs. Gao et al., (2016), likewise found thermodynamics playing a dominant role but discussed also potential dynamical changes ARs.

Lavers, D. A., Allan, R.P., Villarini, G., Lloyd-Hughes, B., Brayshaw, D.J., Wade, A.J.,, Future changes in atmospheric rivers and their implications for winter flooding in Britain, *Environ. Res. Lett.* **8,** 034010, DOI: 10.1088/1748-9326/8/3/034010, 2013b

Gao, Y., Lu, J., & Leung, L. R., Uncertainties in Projecting Future Changes in Atmospheric Rivers and Their Impacts on Heavy Precipitation over Europe, Journal of Climate, 29(18), 6711-6726; DOI: https://doi.org/10.1175/JCLI-D-16-0088.1, 2016.

L450: I am not sure if I fully understand the methodology. Are the origins of ARs being categorized by the southernmost latitude? If so, isn't it more related to the curvature of AR's shape than the actual origin?

It's not the southernmost. First, for every AR that is detected over land we check the time and the latitudinal position where the AR strikes the 10°W meridian. For this we used AR masks generated during the detection. For the post-statistical analysis we then summarize by regions south of 45° degree between 45-60° and >60° to obtain the results. The method is now fully described step by step in section 2.3 "Detection of atmospheric rivers".

L563: please rewrite, grammar problem.

Done. The paragraph:

"The regional climate model is further used to investigate the dynamics of ARs in present and future climate. For this in an ensemble of in total 34 simulations was carried to downscale global climate model scenarios from the CMIP5 suite with a coarse resolution between 1.4 – 3°. The models were used to downscale three greenhouse gas scenarios (RCP2.6, RCP4.5, and RCP8.5)."

reads now:

"The regional climate model was further used to investigate AR in present and future climates. Thus, an ensemble of global CMIP5 climate simulations (1.4°–3° resolution) was downscaled to reach a 0.22° resolution. In total, 34 simulations were carried out for the greenhouse gas emission scenarios RCP2.6, RCP4.5, and RCP8.5. "

Reply to reviewer RC3

The authors make use of downscaled versions of global climate simulations from the CMIP5 ensemble to study the atmospheric rivers reaching Europe in the present and future climates. They find that ARs will become more frequent and stronger in the future, especially in the RCP8.5 scenario. The results also show that the orientation of ARs will change with for example more ARs coming from the south reaching Scandinavia (Norway).

The study is interesting but I got confused with some of the fields presented in the figures, which quality is not great. Some information about the methods and the fields displayed in the figures is missing, the methodology and results are not well discussed and compared to previous studies, and there are many technical mistakes (typos, English, missing words). Therefore, I think the manuscript needs major revision in order to be in a publishable state.

We thank the reviewer for a thorough review of our manuscript and his/her valuable suggestions that really help to improve the manuscript. We regret that we were too short in the description of the methods and other parts of the text.

We have now profoundly revised the manuscript considering recommendations off all reviewers. Major changes include:

1. A “step by step” description of the AR detection method is included in section”2.3 Detection of atmospheric rivers”

2. New section “2.4 Detection in future climate” explains how AR are detected in the future runs. The reason why we used IVT 85th percentiles from the future climate are described and the consequences for the interpretation are given.

3. New section “2.5 Calculation of indices” describes precisely how climate indices in Figures 5, 8, and 12 have been derived. Figures 5 and 8 are now fully consistent (showing the same set of indices).

4. In section “5.1 Uncertainties with respect to the choice of CMIP5 models” We added a paragraph that shows that uncertainty (i.e. model spread) is generally higher in the extreme precipitation regime than in the low or mean precipitation regime.

5. Section “4.3 Influence of dynamical changes” is completely rewritten. The method has been made clear now and the interpretation is more concise.

6. All figures have been redrawn. They now contain visible lon/lat labels. Maps do show the same domain.

Please find our point to point replies below:

Major comments:

About the methods:

- The temporal scale of the RCA model outputs is not clear. From line 209, it seems to be 6-hourly but it would be great to also mention it in section 2.1 or in section 2.2. In the same line, is the extreme precipitation determined from 6-hourly or daily data?

All data analyzed for this study has 6-hourly resolution. In the new version we noted this at several places. We included this as own column in Table 1 (section 2.1). Also, it is additionally noted in the new description of the detection algorithm (2.1), and in the new section “2.5 Calculation of indices”. It is also noted and in the new paragraph decscribing the ERAI reanalysis data at the end of chapter “2.2 The high resolution ensemble”.

- The authors use an AR minimum length threshold of 1500 km. This is quite short compared to previous studies and I guess the reason for this low value is the limited extension of the domain to the west. Can the authors state the reason for such threshold and maybe discuss it in light of previous studies?

In this study we followed the previous approach from Lavers and Villarini (2013) who used a minimum length of 1500 km to detect ARs in an ERA-I reanalysis product. However, we agree that due to our limited domain the algorithm does not detect

1. ARs that do not reach Europe but remain outside over the Atlantic Ocean. Thus, our study can not be compared 100% to global CMIP studies on ARs that take into account also those ARs.

2. Over the western Iberian Peninsula, which is located relatively close to the western model boundary, some AR could be missed or detected with delay (as it may take longer to reach the 1500 km criterion). Over the UK and Norway this does not play a significant role as these countries lie far away from the models lateral boundary.

These aspects are now included in the method section 2.3 (Detection of atmospheric rivers) under step 5.

Lavers, D. A., and Villarini, G. , The nexus between atmospheric rivers and extreme precipitation across Europe, *Geophys. Res. Lett.*, 40, 3259– 3264, doi:10.1002/grl.50636, 2013

- Can the authors confirm that only one AR is detected at every timestep? What happens when an AR covers two latitudinal bins and/or exceeds the IVT 85th percentile in two adjacent 5º bins?

Yes, we can clearly confirm this, as this is checked by our algorithm. ARs are detected separately for each of the 5° latitudinal bins. At this stage it is indeed possible that one AR time step is recorded twice (in two lititudinal bins). Therefore, after detection, the whole AR record is checked for double time steps (year, month, day, and hours information is saved). All double time steps are removed.

This is now explained in section 2.3 Detection of atmospheric rivers under step 6.

- Do the authors define a mask of every AR to link them to precipitation?

Exactly, for every single AR a mask array is created which contains likewise information about the exact date and time. All this in 6-hourly resolution. This is mentioned now in section 2.3 under step 8.

- Lines 223-224: I think the authors should emphasize the fact that the AR detection threshold is different between the present and future periods in contrast to previous studies and what the advantage of this method is. As for now, it is written later in the manuscript (section 5.2) but it should appear upfront. I believe that this choice limits the influence of the larger moisture content in the atmosphere in the future climate on the results. Is that true? Can this aspect be discussed if relevant?

We also think this fact should be emphasized more prominently already in the methods section.

As stated in Lavers et al., (2012; 2013) the 85th percentile of all 12:00 (noon) time steps represents approximately the median value of moisture content within real observed ARs in todays climate (1998-2005). Thus, our approach conserves the relationship between the median moisture content of ARs with the 85th percentile of all noon time steps. The other way to do would be to apply the historical threshold also to the future atmosphere which contains much more moisture also in the background field. This emphasizes more the thermodynamical aspect but does not ensure full compliance with the algorithm developed for present day AR characteristics.

We have put this description and the discussion in the new section "2.4 Detection in future climate".

- I believe the ERA-Interim reanalysis is not really described. For example, the spatial and temporal resolution used in the study is missing.

We now added a short paragraph summarizing information on ERAI relevant to our study (last paragraph section 2.2, line 184). Additionally, Table 1 contains most important parameters for the ERAI data set and compares it to the data sets derived from the RCA climate model.

- In the caption of Fig. 2 and line 329, it is written that ARs are tracked. However, the AR tracking is not explained. I suspect the tracking is used to check the AR persistence and involves ARs masks. Therefore, please add this information in section 2.3.

Yes, the word tracking is misleading here (as it suggests a kind of Lagrangian approach). We did not really track ARs but, as you said, checked persistence of ARs over time. This has been corrected (section 2.3, step 7)

About Section 3.2.3:

Lines 351-354: Can the authors explain a bit more how a negative temperature bias in the regional climate model is linked to a "too high moisture load" and to the higher number of ARs in September in the hindcast compared to ERA-Interim? I would expect a higher temperature to be linked to more moisture. Moreover, wouldn't it be more useful to look at the precipitation in the hindcast, in ERA-Interim, and in the E-OBS dataset, instead of looking at the temperature? Could it also be useful to assess the difference in the specific humidity between the hindcast and ERA-Interim?

We have strongly revised section 3.2.3. Effect of Downscaling" (now section 3.4 in the new version). This section focuses now on the comparison between the hindcast RCA-ERAI and ERAI to show the added value of high resolution. Therefore we have removed the paragraph on a potential temperature bias because this was too speculative.

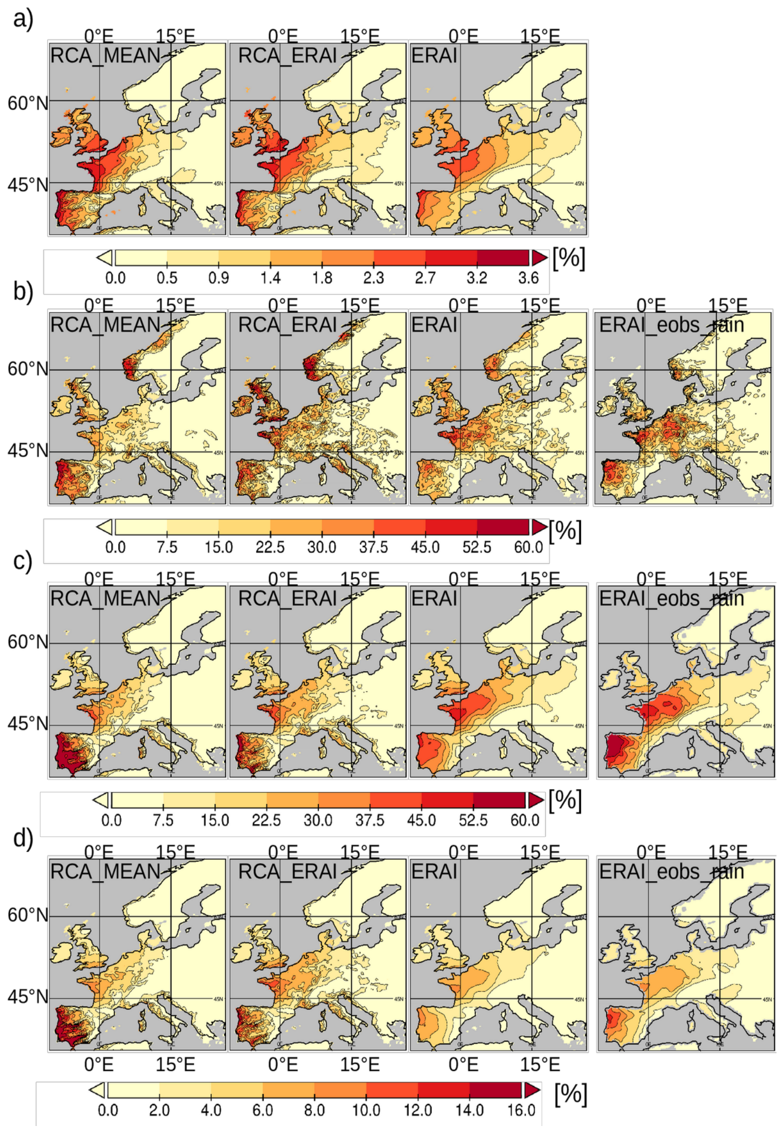
Lines 354-356: This sentence must be rewritten. It is not clear at all.

The sentence is now removed (see previous comment above).

What would be the impact of using the E-OBS precipitation instead of ERA-Interim's in the results displayed in the right column of Fig. 5b-d?

We recalculated figures 5b-d (below) using the AR masks from ERA-I combined with precipitation from the E-OBS data set (rightmost column). We note, that by doing so, we violate the IVT equation shown in line 203 which means there is no physical consistency between the atmospheric moisture content and precipitation. However, the result is shown in the right-most column. For the percentage of yearly maximum precipitation attributed to ARs (%AMP, see below figure 5b), the spatial pattern is more or less the same. Differences occur over Norway and the UK where ERAI_eobs_rain shows a weaker signal compared to ERAI.

With respect to the fractional contribution of AR induced precipitation to the total >95th percentile precipitation (Figure 5c) and the fractional contribution of AR induced precipitation to the total amount of precipitation (Figure 5d) we note that the pattern is the same as for ERAI (third column) but the amount of rain related to ARs is overall higher compared to ERAI.



Revised Fig. 5. a) AR frequency expressed as total number of days a grid cell was covered by an AR during the historical period. b) Percentage of annual maximum precipitation related to ARs (%AMP) c) fractional contribution of AR forced precipitation to the >95th percentile precipitation (%95P). d) same as c) but for the total precipitation (%TP).(Note that in a) the unit has been changed from "total number AR days within 30 years" to %AR days of total days as recommended by reviewer RC2).

About section 5.2:

Gao et al. (2016) showed that ARs became more frequent north because of the poleward shift of the eddy-driven jet. That is not what the authors seem to obtain by distinguishing the "origin" of the ARs. I believe it would be more interesting and useful to look at the eddy-driven jet response in all scenarios to explain the responses in the number of days with ARs and AR-forced precipitation?

That's true. Gao et al. found in CMIP5 models a relationship between the low level jet position in historical climate and future changes in ARs .

We agree it would be interesting to look on the eddy driven jet in our RCA simulations. However, due to its limited domain RCA excludes significant part of the North Atlantic ocean. Therefore it is not suited to analyze changes in the position of the Jet Stream as this feasible in global models. Please also note our revised section "4.3 Influence of dynamical changes". In the previous version we gave the impression of a shift in the ARs source region. That was wrongly formulated. Indeed, we mean changes in the route ARs take east of 10°W and across Europe. Therefore, our results are not contrary to Gao et al. (2016) but complementary. We made this clear in last paragraph of section 5.2 Differences to global projections (lines 623ff).

Remove sentences "At least… (Ma et al. 2020). The authors…likely reasons." on lines 546-548 because it is not relevant for the present discussion.

We agree and removed the two sentences in the revised version.

About the figures' content:

- Figure 2: is the 85th percentile determined using all points within the 5º bins and all time steps? In any case, please state somewhere how the values shown in this figure were calculated.

Yes, it was calculated from all the grid cells in the range of the respective 5° bin and all time steps. It's now stated in section "2.3 Detection of atmospheric rivers" under steps 2 and 3.

- Figure 4, 7: how is the "average moisture transport over land" calculated? Are the authors using a mask for the ARs and averaging the IVT within the mask? Please explain.

Yes, this is exactly the way it was done. It is now explained in section 2.3 under step 8.

- Figure 5: Row a): the authors describe this row as AR frequency in the caption but, as written in the text, it is only a number of days. One would have to divide by the total number of days in the period to get a frequency. Moreover, since the model output is 6-hourly, I do not understand how the authors convert it to a number of days. What if an AR covers two days during its lifetime? Is it counted twice? Also, if the AR lasts a minimum of three time steps (because of the 18h minimum duration) in the same day, it is counted only once, correct?

For the analysis we classified a day as AR day if within the 24 hours at least one AR event was recognized. That means, even if only one of the four 6 hour time steps of day was impacted by an AR, this day is counted as AR day. Consequently, any AR that covers two subsequent days (lasting from 18:00 to 12:00 of the following day) is counted as 2 days. The explanation is now included in the new section 2.5 Calculation of indices.

In the new version Figs 5a, and 8a AR days are now calculated as frequency (AR days divided by total number of day in 30 years) and expressed as %.

Rows b), c), and d): I find not clear what is shown in those panels. It is worth mentioning in the text how those "indices" are calculated and keep the same names throughout the manuscript. Can the authors explain why ARs contribute to the yearly maximum (row b) over southwestern Norway or northern UK but barely contribute to the extreme precipitation (row c)?

We regret the confusion that our description of the indices caused. See our exact explanation below.

Row b): I understand this figure as the percentage of years (among the 30 years of the period) for which the maximum precipitation occurs when there is an AR. Is that correct? In any case, the text should be clarified. The same comment applies to the rows c) and d).

Exactly, if in 15 years, out of the 30 year period, the annual maximum precipitation can be related to an AR we get a value of 50%.

In figure c) we

1. sum up the accumulated precipitation that occurs in all rain events that exceed the 95th percentile rain rate.

2. sum up the accumulated precipitation that occurs in all rain events that exceed the 95th percentile rain rate and can be related to ARs

3. we calculated how big is the fraction (%) of the sum in step 2 to the sum calculated in step 1.

In figure d)

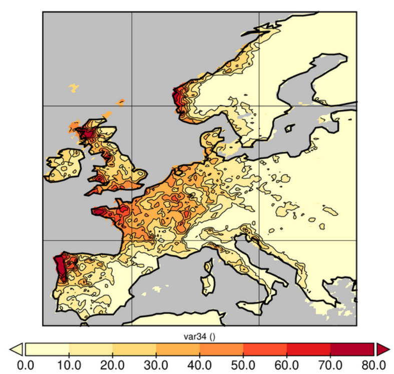
we do same. But instead considering precipitation events >95 th percentile only we consider all rain events (not only the >95th percentile events).

In the new version we now denote this “fractional contribution of ARs to total precipitation (%TP) or to the >95th percentile precipitation” (%95P). This is now described in the new section “2.5 Calculation of indices”.

Row c): how do the authors relate the low values for the Norwegian coast to the study of Benedict et al. (2019) who found that 85% of the extreme precipitation events are related to ARs?

Benedict, I., K. Ødemark, T. Nipen, and R. Moore (2019): Large-Scale Flow Patterns Associated with Extreme Precipitation and Atmospheric Rivers over Norway. Mon. Wea. Rev., 147, 1415-1428. https://doi.org/10.1175/MWR-D-18-0362.1

If we understand Benedict et al., (2019) right, then they calculated the percentage of the number (N) of >99th percentile rain events. This is not what we did. As done in Gao et al. (2016), we calculated the accumulated volume of precipitation (not the number of events; see above). In the below figure we applied the approach of Benedict et al. (2016) to our RCA-ERAI run. As can be seen, over southwestern Norway, up to 80 % of >99 percentile events are related to ARs which is in well agreement with Benedict et al., (2019).



% of number of AR related >99th percentile rain events to total number of >99th percentile rain events (as done in Benedict et al., 2019).

- Figure 8: is this figure showing a simple difference between the future and the historical experiments or does it show a relative change?

Figure 8a (AR days) shows a simple difference; not the relative change. Likewise, for figures 8b, 8c, 8d) and 8e) simple differences were shown (i.e. the % values for the future minus the % values for the historical). We have clarified this in the caption of Fig. 8 in the revised version.

Caption: I would not call what is displayed “climatological indices”. Please remove. Panel b) does not show “precipitation rates” if it is similar to Fig. 5. Please use the same wording for Fig. 8 as for Fig. 5.

We agree and have fully harmonized figures 5 and 8 in the new version. All indices are described now in section “2.5 Calculation of indices”

“Note all non-robust” -> Note that all non-robust

We changed this in the Fig. 8 caption in the new version.

What is the difference between panels c and d? One of the two is not shown in Fig. 5.

Figure 8c was showing the change in % of number of events in the 95th percentile (similar to Benedict et al., 2016 but for the 95th percentile instead of the >99th percentile fraction). We have removed 8c as it added not significant information to what was shown in the other panels. Figs. 5 and 8 display now the same set of indices.

Why is the unit in panel c is % if the what is shown is “the number of of events” as written on lines 400-401?

Panel 8c is now removed from Fig. 8 (see our answer to the above comment).

- Figure 10: Are the authors sure that panel a) is for ARs originating north of 60ºN and panel b) for ARs originating south of 45ºN? It does not seem in agreement with the text. For example, the sentence “the RCA ensemble clearly shows a relative increase of those ARs originating south of 45ºN (Fig. 10a).” However, Fig. 10a only shows negative values and the caption says that panel a) is for ARs originating north of 60ºN. Please make sure that the caption and text (from line 458 to 475) correspond to the figure. Can the category 45-60ºN be displayed as well? In the caption, what does “relative contribution” mean? Does it show the relative difference between the future and historical experiments or is it a simple difference?

This was a mistake. It should be

“the RCA ensemble clearly shows a relative increase of those ARs originating south of 45ºN **(Fig. 10b)**.” (Figure 10b in the previous version showed increase of originating ARs south of 45°.)

In the new version we have also included the category 45-60°N. The section “4.3 Influence of dynamical changes”, which discusses Fig. 10, has been revised. The main conclusion is that AR from the south increase stronger than ARs from the north. This increases their relative share over land nearly everywhere.

- Figure 11: Why does the color bar for the standard deviation panels exhibit negative values? A standard deviation is positive. Is the STD panel a difference between the future and historical standard deviation or the standard deviation of the responses displayed?

The standard deviation denotes the inter-model spread of the responses. We have now completely redrawn Fig. 11 (Figure 12 in the revised version) which is now in the same style as the other 2D maps. No negative values in the scale of standard deviation any more.

About the figures’ quality:

I think the quality of the figures should be improved.

Figures 2, 3, 4, 8, 9, 10, and 11 exhibit gray frames around the panels and around the color bars. Could they be removed?

The frames were introduced when they were inserted in our word processing package. The original png figure files have no visible frames. We will deliver the original png files to the Copernicus office and so we hope there will be no frames in the published version.

Figure 9 exhibits a colored line in between the two panels as if another figure was below.

The figure was accidentally inserted twice. This has been fixed.

Figures 1, 5, 9, and 10 exhibit weird coastlines over Greece. Moreover, Crete and the Balearic Islands are missing.

We ave tried to make the plots with higher resolved coastlines. However, it turned out, that at higher resolution the regions with “noisy coastlines” come out as black opaque areas due to multiple lines plotted at short distance. In these regions (e.g. coasts of UK, France Norway) this completely covered the color shades and so destroyed the important information. Therefore, we decided to keep the less coarser resolved coastline for the benefit of the information we want to show.

Figure 11 has too small labels for the latitudes (longitudes missing) and for the color bars. Moreover, the coastlines are discontinued at 20ºE.

This figure has now been redrawn and now contains larger lon/lat information. It is figure 12 in the new version.

Figures 4, 6, and 7: the frames, tick marks, and background grids are almost invisible. Please make them darker or black.

Grids, frames and tick marks have been made thicker.

Figures 7, and 11, and Tables 2 and 3: Please arrange the GCMs in alphabetical order as the authors did for Fig. 2.

It would be great if all figures showed the same domain.

The figure/tables are now rearranged in alphabetical order. All maps show the same domain now.

Figure 8: can the columns be rearranged such that RCP2.6 is on the left and RCP8.5 on the right? I find it more intuitive and it would be consistent with Figs. 2 and 7 and Tables 2 and 3.

The columns are now rearranged accordingly.

Figure 2: the gray and yellow lines are barely visible.

Figure 2 has been revised now. Grey and yellow lines turned to dark green and orange respectively.

Figure 5: In the left column, panels b), c), and d) should have RCA_MEAN as title instead of RCA_ENSM. Better remove the titles of the three bottom rows as it would make the panels bigger, improving their readability.

We now used consistently RCA_MEAN instead of RCA_ENSM. Also, we move the titles inside the maps which allowed larger maps.

Minor comments:

Lines 25-28: it seems from this sentence that Norway is in Central Europe. Please rewrite this sentence, maybe splitting it in two.

We agree and removed the second part of the sentence. Itread now:

"Over central Europe, the models simulated a smaller propagation distance of AR toward eastern Europe than obtained using the ERAI data."

Sometimes, the authors write ERA-I and sometimes ERAI. Please be consistent in the text and captions and make sure the same acronym is used everywhere (I suggest ERAI).

We now use consistently ERAI.

Line 138: why is the coupling between the RCA model and NEMO only over the North and Baltic seas? What about the Mediterranean and the Norwegian seas?

The regional setup of NEMO was originally developed for the North Sea and Baltic Sea which requires a very high spatial resolution (~3.7 km on average, and 56 vertical levels). Hence NEMO needs extremely high computational resources. Therefore the other sea had to be excluded. This is now explained in section "2.1 The regional climate model RCA"

Line 151: "in a huge ensemble": to which ensemble do the authors refer to? Does it have a name? This ensemble seems similar to the one used in the present study so why not using it?

Yes it's the same ensemble. We have changed the sentence now (line 159):

" It has been employed in previous studies to investigate the present climate and simulate the mean response to global climate change by downscaling global climate scenarios (Dieterich et al., 20119….)"

Caption of Figure 1: Please rewrite it. This figure mainly shows the topography of the domain in brown and the bathymetry in blue-green colors.

We have now corrected the caption Fig. 1.

The authors very often use very "greenhouse gas scenarios". I suggest to use instead "greenhouse gas emission scenarios" or probably better "GHG emission scenarios".

We now use consistently "greenhouse gas emission scenarios" throughout the manuscript

Lines 202-203: please add the units of g, the wind, and dp.

Units of g [$m^3/(kg * s^2)$] Wind [m/s] and *dp* [Pa] are now added.

Line 203: “In the two hydrostatic models…”: I assume that the authors here refer to the regional climate model RCA and to the IFS model. Why does the reader need this information?

We agree and removed this sentence.

Caption of Fig. 2 : “IVT thresholds” -> 85th percentile of IVT

“…for the ensembles’ historical…” -> … for all models and the historical…

We changed the caption accordingly

Line 278 (page 10): “heavy precipitation”: please write that this is defined using the 95th percentile in order to relate it to what is shown in Fig. 5c.

We agree this is important. In the new version we have included a complete description of all indices in section “2.5 Calculation of indices”

Lines 279-280: What are “the mean climatic conditions in Europe”? Moreover, it rains a lot in Southwestern Norway and that is not reflected in Fig. 5a.

This mainly refers to the different climates found in Europe, i.e. wetter conditions in Northern Europe / dryer conditions in southern Europe.

What we basically want to show is: in very humid regions with plenty of rain like Norway, ARs can not contribute so much to the total and to the >95th percentile rain as they do in dryer regions (like Iberia). We have rewritten the sentence:

“The spatial pattern mainly mirrored the AR frequency pattern but it also reflected the varying long-term mean hydrological conditions in Europe:…” (line 381)

Line 296: What is meant with “weather regimes”? Moreover, what is the point of lines 295-298?

We have reformulated this section and removed these lines.

Line 321: Do you mean effect of the downscaling? With “regionalization”, it sounds to me like the authors split the domain into different regions.

We used the two term synonymous. We now replaced “regionalization” by “downscaling” to avoid any confusion.

Line 324: Can the authors explain the “factor of 10”?

The factor refers to the grid cell size in RCA (550-600 km^2) and the ERAI reanalysis (~6000 km^2) given in table 1 of the former manuscript. We have changed this now and noted the grid distances in x and y metrics (~24*24km for RCA; 80*45 km for ERAI).

Line 378: “AR” -> ARs

Changed.

The reference Massoud et al. (2020) does not seem appropriate here as the paper is not about the US but about the Middle East. Maybe the authors meant the following reference:

Massoud, E. C., H., Lee, P. B., Gibson, P., Loikith, and D. E., Waliser (2020): Bayesian Model Averaging of Climate Model Projections Constrained by Precipitation Observations over the Contiguous United States, Journal of Hydrometeorology, 21, 2401-2418. https://doi.org/10.1175/JHM-D-19-0258.1

Thank you for this correction. The reference has been changed.

Lines 418-419: what is meant with “contribution anomalies”? Could it be replaced by responses?

This has been rephrased to %95P and %TP and is now consistent with the new description of indices in the methods section (section 2.5 Calulation oof indices)

Line 488: RCA-IPSL and RCA-MPI have weaker responses than GFDL, CAN, and NORESM over the North Atlantic, don't they?

Sorry, we apparently confused the individual plots. Thank you for the correction. We have replotted the figure 12 and revised the text. Also the maps were reordered alphabetically.

Lines 524-525: "This was done… period (Neiman et al. 2008)." I do not understand how this sentence justifies the use of a different 85th percentile in the historical and future experiments.

We have put the explanation now in the newly added section "2.4 Detection in future climate" in the methods section where also the consequences are discussed.

Lines 558-559: "Generally the AR imprint…eastern Europe." Isn't this sentence in contradiction with Fig. 5 where larger values are found over western Europe?

Yes, in case of western France and the UK this is true. In case of Iberia the impact in ERAI is lower. We have changed the sentence accordingly.

Over Iberia, the AR imprint on the analyzed indices in the ERAI data set was weaker but stronger in distant parts of eastern Europe.

Technical issues:

Consider using commas much more often than currently.

Thank you very much for the careful reading. After the revision of the text and the figures the manuscript was by a native speaker.

Line 22: "ER" -> ERA

done.

Line 24: "eat" -> east

done.

Lines 34, 426: "maximal" -> maximum

changed consistently throughout the manuscript

Line 37: "Iberia(15" -> Iberia (15

done.

Line 40: "likely the originate" -> likely originate

done.

Line 41: "from >60 ºN" -> from latitudes >60 ºN

done.

Line 64: "Pacific Sectors of the World Ocean" -> Pacific sectors

done

Lines 75, 79, 194: "Laver" -> Lavers

corrected.

Line 80: "However they" -> However, they

done.

Line 90: "AR" -> ARs

done.

Line 103: "framework employed" -> frameworks

done.

Line 114: "4.5,RCP8.5" -> 4.5, RCP8.5

done.

Line 118: "a validation RCA" -> a validation of the RCA

done.

Line 135: "2005)and" -> 2005) and

done.

Caption of Fig. 1: "Bathymetricy" -> Bathymetry

done.

Line 165: "hindcasst" -> hindcast

corrected.

Line 180: "W/m2" -> W/m^2

done.

Line 206: Remove “Then”.

done.

Lines 223-224: “for each of the ensemble members respectively” -> for each model

done.

Caption of Fig. 3: “below the threshold” -> below the 85th percentile

done.

Caption of Table 3: “number ARs” -> number of ARs

done.

Caption of Fig. 4: “b):” -> b)

done.

Line 253: “AR” -> ARs, “Fig. 4” -> Fig. 4B

done.

Line 281: “Bretagne” -> Brittany (to be consistent with line 579)

done.

Line 300: Add parenthesis before Fig. 5.

done.

Lines 279, 290, 299, 300, 422: "Figure" -> Fig.

Whenever referenced in parenthesis we have now consistently written "Fig." instead of "Figure". Only when it is at the beginning of a sentence we wrote Figure.

Line 304: "respectively calculated as 0.98" -> respectively 0.98

done.

Line 306: remove "respectively"

done.

Line 314: "is" -> are

this sentence has been rephrased to improve readabilty.

Caption of Fig. 6: "detercted" -> detected, "precentage" -> percentage, "AR" -> ARs

done.

Line 323: "is" -> are

Line 325: "of spatial" -> of the spatial

Line 332: "ERAI-" -> ERAI

Line 333: "and larger" -> and a larger

Lines 335, 559: "distal" -> distant

Line 336: "This implies ARs" -> This implies that ARs

Line 338: "as in in semi aride": remove one "in"

Line 339: "effect the of" -> effect of

Line 346: "Fig. 5b" -> Fig. 5d

Line 360: remove "which is notably lower"

Section "Effect of Downscaling has been completely rephrased (line 320-360, old bersion). Typos have been fixed.

Line 383: "frequency" -> number

As stated above we have now recalculated the frequencies of AR-days in Figs 5a and 8a.

Line 398: "5b) no" -> 5b) but no

the sentence has been rephrased.

Line 400: "Figure 8c shows the number" -> Figure 8c shows that the number

the sentence has been rephrased.

Line 404: remove "Apart from this"

the sentence has been rephrased.

Line 412: "Figures" -> Figure

done.

Line 420: "stronger" -> more

done.

Line 421: “response” -> responses, “is” -> are

done.

Line 424: the reference to the paper of Teichmann et al. 2018 is missing in the reference section.

Thank you. The reference is now added.

Line 440: add comma between “latitudes” and “it” and remove comma after “found”.

Line 445: “main driver AR” -> main driver of AR

Line 446: “Jet” -> jet

Line 451, 454: maybe not use “incidents” but rather events

Line 455: “for high” -> for the high

Line 460: remove “degree”

Lines 473-474: “sectors” -> sector

The section lines 440 to 476 (old version) has been revised and typos have been fixed.

Line 483: “frequency of ARs” -> number of days with ARs

Figures 5a and 8a display now frequencies (see above)

Line 484: “similar RCA” -> similar to the RCA

done.

Line 489: “RCA-ECE show” -> RCA-ECE shows

done.

Lines 490-491: “one realisation shows wider” -> RCA-MIROC shows wide, and remove “(RCA-MIROC)” at the end of the sentence.

Done.

Line 496: add comma after RCA-MPI

done.

Line 501: “the most heavy” -> the heaviest

done.

Line 504: “is” -> are

done.

Lines 507-508: remove at least one of the “likewise”.

Removed “likewise” in line 507.

Caption of Fig. 11: “forrcing” -> forcing

done.

Line 519: “2016; 2016;” either a reference is missing or there is one “2016” too much.

This has been fixed

Li**ne 541: “Norway(Fig. 11c)” -> Norway (Fig. 11c)**

done.

Line 551: “model with” -> model ensemble with, “was applied” -> was created

done.

Line 552: “regionalization” -> downscaling

done.

Line 555: “the contribution to” -> the contribution of ARs to

done.

Line 563: “climate” -> climates

done.

Line 571: “of orographic” -> by orographic

done.

Line 575: “show ARs” -> show that ARs

done.

Line 588: “favors” -> favor

done.

Line 589: “stronger” -> more strongly

done.

Line 596: “arriving Scandinavia” -> arriving to Scandinavia

done.

Reply to reviewer RC4

In the present work, the authors used downscaled versions of global climate simulations from the CMIP5 ensemble using RCA-NEMO to study the atmospheric rivers over Europe in the present and future climates and compared the same with runs using ERA-I and using different RCPs. Results show that ARs will become more frequent and stronger in the future. Furthermore, the authors highlighted the variability of precipitation in different scenarios and different model simulations.

Though the study consists of some interesting results, the presentation of results, description and quality need to be improved. Information on the methods and quality of the figures including the methodology needs to be elaborated. Many typos, Grammar, missing words are found. Therefore, I would recommend the manuscript for major revision before accepting it for publication.

We thank the reviewer for a thorough review of our manuscript and his/her valuable suggestions that really help to improve the manuscript. We regret that we were too short in the description of the methods and other parts of the text.

We have now profoundly revised the manuscript considering recommendations off all reviewers. Major changes include:

1. A “step by step” description of the AR detection method is included in section”2.3 Detection of atmospheric rivers”

2. New section “2.4 Detection in future climate” explains how ARs are detected in the future runs. The reason why we used IVT 85^{th} percentiles from the future climate are described and the consequences for the interpretation are given.

3. New section “2.5 Calculation of indices” describes precisely how climate indices in Figures 5, 8, and 12 have been derived. Figures 5 and 8 are now fully consistent (showing the same set of indices).

4. In section “5.1 Uncertainties with respect to the choice of CMIP5 models” We added a paragraph that shows that uncertainty (i.e. model spread) is generally higher in the extreme precipitation regime than in the low or mean precipitation regime.

5. Section “4.3 Influence of dynamical changes” is completely rewritten. The method has been made clear now and the interpretation is more concise.

6. All figures have been redrawn. They now contain visible lon/lat labels. Maps do show the same domain.

Please find our point to point replies below:

Major comments:

L100: Please elaborate on the purpose of the study.

The main purposes of this study are to:

- conduct the first analysis of AR over Europe using a downscaled CMIP5 model ensemble
- Investigate the added value of high resolution in representing AR in a climate model.
- assess future climate related changes in AR characteristics over Europe.
- explore uncertainties with respect to the choice of the global model and in regard to the choice of the greenhouse gas emission scenario

We have included the text to make the purpose more clear.

L105: Please rephrase the sentence and carefully check the text corrections.

We have completely rephrased the paragraph. The whole text was corrected by a native speaker after revision.

Section 2.1:

Please give more details on the model setup, spatial and temporal resolutions of climate models, comparison with

ERA-I etc.

Table 1 includes now an overview about horizontal resolution (in km & degree), vertical levels, and temporal resolution for the climate model RCA and the used ERAI reanalysis data. We have also added a short paragraph that briefly summarizes the information about the ERAI reanalysis (ERAI) and ERAI hindcast simulation (RCA-ERAI).(last paragraph in section “2.2 The high resolution ensemble”)

Also, it would be interesting to see a case study showing how later boundary forcing is influencing the AR characteristics in the RCA-climate model.

The lateral boundary forcing is extracted from global CMIP5 models/ERAI reanalysis and prescribed at the lateral boundaries. There is no feedback from RCA to outside the model domain. This means ARs enter the model domain at the lateral boundaries from the parent global model but then they develop freely and independent from the parent global model. We have added this information in the first paragraph (lines 141ff) at the end of section 2.1 to make this clear.

Section 2.2:

Table 2: Please provide a reason for leaving out the RCP2.6 cases for RCA – IPSL-CM5A-MR, RCA – CanESM2 and RCA – CNRM-CM5 despite their availability to force the RCA.

There were technical reasons why these scenarios are lacking (not all driving data necessary for RCA were available from these 3 models). We now added this information in the caption of table 2

Section 2.3: In addition to figure 2, a figure with the IVT threshold difference w.r.t historical mean in different RCPs would give a robust picture of the difference in magnitude of IVT thresholds.

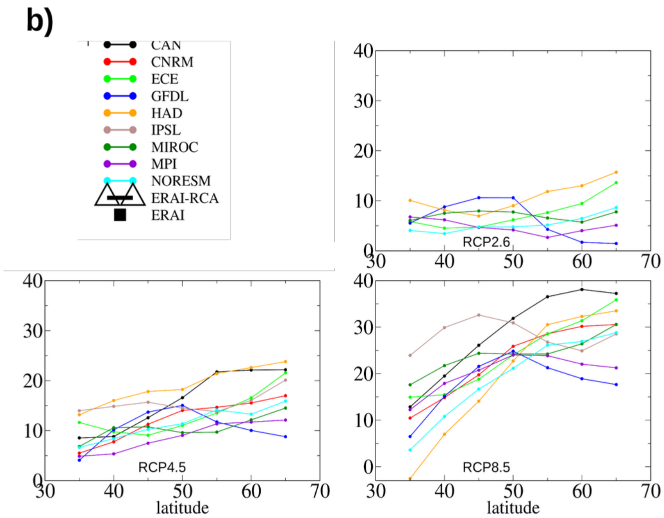


Figure 2b (revised version): change [%] of IVT thresholds in the future scenarios relative to the respective historical period.

We have added the difference plots as Figure 2b in the revised version (see above).
The plot gives indeed a more robust picture and demonstrates the presence of dynamical changes. We added a short paragraph at the end of the new section “3.1 Differences in IVT thresholds in historical, and hindcast simulations”:

“There was no evidence of a linear relationship between climate warming and the and the increase in IVT thresholds (Fig. 2b). For example, in RCA-HAD the IVT at 35°N in the low and moderate warming scenarios (RCP2.6 and RCP4.5) increased by ~10% and ~15%, respectively (Fig. 2b). In the strongest warming scenario (RCP8.5), however, the IVT decreased by ~5%. This suggests that, over the long term, dynamic changes influence the IVT.

How do the discrepancies in IVT thresholds from different models and different RCPs are attributed to? Please give a brief note on the reasons for the biases in the IVT thresholds. Could it be due to bias in RCA runs or due to bias in

the lateral boundary conditions from the GCMs?

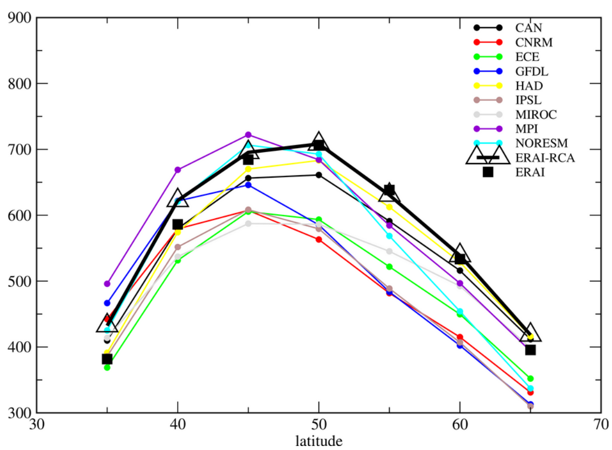
The discrepancies are most likely related to the different climates provided by the global models at the lateral boundary. First, all parent global models have a different thermodynamical equilibrium state and with this air temperatures and moisture load will be different. In connection with this, the large scale atmospheric circulation will differ (e.g. Brands, 2021) among the global models as the equator to pole temperature gradients are likewise different (Harvey et a., 2013). Consequently, the large differences in the IVT thresholds are not so surprising but he magnitude of difference is noteworthy. We will add a short paragraph to discuss these discrepancies.

Brands, S.: A circulation-based performance atlas of the CMIP5 and 6 models for regional climate studies in the northern hemisphere, Geosci. Model Dev. Discuss. [preprint], https://doi.org/10.5194/gmd-2020-418, in review, 2021

Harvey, B.J., Shaffrey, L.C. & Woollings, T.J. Equator-to-pole temperature differences and the extra-tropical storm track responses of the CMIP5 climate models. *Clim Dyn* **43,** 1171–1182 (2014). https://doi.org/10.1007/s00382-013-1883-9

This is now discussed in the new section 3.1 Differences in IVT thresholds in historical, and hindcast simulations.

Figure 3: How do authors justify the large difference in IVT of ARs in hindcast and reanalysis? Though both reanalysis and hindcast data use observations, I would assume that the model parameters such as lead time, assimilation methods etc might be causing the bias here. Please discuss the same.



We have to apologize for the bad quality of Figure in the old manuscript version which made it difficult to properly distinguish RCA-ERAI and ERAI. We replotted Figure 2 and marked both curves with symbols (see above). Hence, notable differences between ERAI (black curve triangles) and ERAI-RCA (black curve filled squares) occur only south of 40°N. The largest difference, equals ~11% (at 35°N). That's not so much but nevertheless notable. Due to the rotated grid of RCA, the positions 35°N and 40 °N at 10°W are closest to the lateral boundary of RCA. Thus, the most likely explanation is, that we see here an effect of the errors associated with the lateral boundary coupling which are well known in regional models with one-way coupling to the driving coarse resolution models (e.g. Davies, 2013; Chikhar and Gauthier 2017).

Furthermore, the southern model boundary at 35°N lies in the transition zone from dry air masses of the subtropics and the wet air masses of the humid westerlies i.e. where high gradients in moisture content over short distances can be expected. Here, small differences in the mean position of the transition zone can cause large differences in local moisture content. We will discuss this in a potentially revised version.

An explanation has been added to the new section "3.1 Differences in IVT thresholds in historical, and hindcast simulations."

Davies, T. (2014), Lateral boundary conditions for limited area models. Q.J.R. Meteorol. Soc., 140: 185-196. https://doi.org/10.1002/qj.2127

Chikhar, K., & Gauthier, P. (2017). Impact of Lateral Boundary Conditions on Regional Analyses, *Monthly Weather Review*, *145*(4), 1361-1379.

Section 3.1:

L235: I would agree with the authors that the RCA impact on detecting the number would be less. However, it is to be noted that RCA could impact the strength of the ARs including the length to width ratio, footprint over a region, persistence etc. Please discuss these issues here.

Good point. We agree RCA leaves it's own footprint on ARs within the domain and with this it influences geometry, duration and intensity.

We have added this point in the first paragraph of section "3.2 General statistics":

"...This indicates that the number of AR in the RCA is primarily controlled by the parent global model at the lateral boundaries. This is not surprising since AR develop in open ocean regions far outside the model's domain. However, within that domain the RCA develops freely, leaving its own fingerprint on AR, by controlling their intensity, geometry, and lifetime. The RCA fingerprint is likely to become stronger with growing distance from the lateral boundaries…."

Table 3: Why is RCA-MIROC showing a different trend in frequency expressed as the number ARs detected in a 30 year period of different climate scenarios? I suspect the value 445 8 from RCP45 in RCA-CAN. Please verify it.

This was a mistake RCA-CAN has a value of 445. Thank you for the correction.

RCA-MIROC shows indeed different trends. The reason for that is difficult to analyze and would need further analysis on the global model MIROC5 which is beyond the scope of the present study. We added the following remark in section "3.2 General statistics"

"Compared to the historical period, RCA-MIROC increased by 15% in RCP 4.5 but only by 5% in the strongest warming scenario (RCP8.5, Table 3). Hence, there was no linear scaling with global mean warming. This can be explained by the changes in the large-scale circulation in the parent global model, such as induced by shifts in the eddy-driven jet (Gao et al., 2016)."

Section 3.2:

L290: I would recommend authors include more details on rainfall variability by describing the dynamics and synoptic conditions responsible, rather than simply showing some stats.

Our main point here was to demonstrate that even despite ARs are a very rare phenomenon (< ~3% of all days in a 30 year period are impacted by ARs) they can contribute significant amounts to the local yearly rainfall amount particularly in dry regions. We have rephrased section 3.2 (3.3 un the new version) to make this more clear.

We agree that more details on the dynamics of synoptic weather system would be interesting. However, this would need much more statistical analysis and would require to develop algorithms for the detection and classification of synoptic weather systems in the RCA model output. This would go beyond the purpose of this paper. However, there is literature about relationship between ARs and synoptical weather systems (Pasquier et al., 2018). We made a reference to the Pasquier et al paper at the end of section 5.2.

Pasquier, J. T., Pfahl, S., & Grams, C. M. (2019). Modulation of atmospheric river occurrence and associated precipitation extremes in the North Atlantic Region by European weather regimes. Geophysical Research Letters, 46, 1014– 1023. https://doi.org/10.1029/2018GL081194

Section 3.2.3:

L325: Please verify the Figures numbers cited here. Through precipitation patterns are modulated by stochastic processes and further modulated by topography, the magnitude of IVT is affected by the small scale processes such

as fluxes, SST and winds etc. Please go through the literature and discuss the same here.

We have strongly revised this section (section "3.4 Effect of downscaling" in the new version) which now mainly focuses on the effect of resolution. Therefore, in this context we would like to avoid a review on small scale processes like winds surface temperatures and air temperatures etc. that influence the magnitude of the IVT. Instead, we added a reference to Feser et al. (2011) Hoheneger et al. (2020) and Stevens et al. (2020) and Gomez-Navarro et al. (2011) that discuss the added value of high resolution on convection processes and the representation of topography in high resolution models. (Line 399ff).

L335: Please describe the reasons for the bias in RCA ARs inland propagation.

This is difficult to explore. It could be related to different model physics between RCA and the ECMWF IFS model (which produced the ERAI reanalysis with data assimilation). It could also be related to the fact that in RCA the North Sea and Baltic Sea is interactively simulated with a high resolution ocean model, while in the ECMWF-IFS model these two seas are implemented only as a fixed surface scheme. This means basically that SST, sea ice boundary fields are prescribed from ERA data while in RCA4-NEMO they are calculated during simulation.

We now discussed possible reasons in an own paragraph of section "3.4 Effect of Downscaling" (lines 411ff).

Section 4.2:

L385: Not sure if the figures cited here are relevant to the context. Please verify
Section 4.2 "Spatial changes" was substantially revised and Fig. 8C in the old version was removed. This was done to harmonize Figs 5 an 8 which now show the same parameters. We rephrased the text body and likewise checked references to figure as well.

L400: Increase in AR forced heavy precipitation over Eastern Europe may also associate with changes in absolute path of the ARs, increase in IVT/moisture availability and duration/persistency of the ARs over the land. Please discuss these issues.

This comment refers to Fig. 8c of the old version which showed high relative increases (%) of AR events over eastern Europe. We have removed this Figure 8c to harmonize Figures 8 and 5 to show the same parameters (as recommended by other reviewers). The aspect of changed absolute AR paths / moisture availability is discussed now in section "4.3 Influence of dynamical changes"

Section 4.3:

It is a good practice to present figures with lat-long labels which are missing for almost all spatial figures in the manuscript. Please redraw them in the revised manuscript.

We have redrawn all the maps and added longitude/latitude labels.

L450: The approach selected by the authors in finding the source region of ARs raises many questions. For example, earlier the authors mentioned that As originate from open oceans. But taking 10W as a reference for finding the source region does not line up with the earlier statement and may induce errors in results. Furthermore, it is not customary to find the source region according to the AR incidence/landfalling.

Sorry, this approach was described insufficiently in the previous version. We did not want to evaluate the source region (unfortunately we used this wording) but we meant the latitudinal bin where ARs strike the 10°W meridian. We agree this doesn't say anything about the source region where the ARs formed (outside our model domain). As you mentioned in your previous comment (L400), this says something about the absolute path of the AR after entering Europe at 10°W.

In the revised version, the Section "4.3 Influence of dynamical changes" has been substantially revised to more accurately describe the analysis method, it's interpretation and the conclusions.

L460, 465, 470: Please rephrase the sentences with proper citations to the figures.
We have rephrased the two paragraphs and corrected some mistakes in the reference to figures.

L475: " we can conclude that ARs from the southern Atlantic sectors are more present over most land regions in a warmer climate" is a strong statement in this context.
This statement is misleading. We removed it in the new version.

Section 5.1: Please describe how and why the higher latitudes experience increasing precipitation despite the decrease in ARs.

This was poorly described. ARs do also increase in the high latitudes >60°N but ARs <45°N increase even more. Consequently the relative contribution on land from ARs entering Europe north of 60°N lowers.

We described this now more clearly in the revised section “4.3 Influence of dynamical changes”. We also added a note in section “6 Summary and conclusions” (line 663).

P495: How the decreased AR impact over Norway can be explained with the decrease of ARs arising from >60N. Earlier authors claim that Scandinavian ARs originate from the south.

In the future climate AR increase at all latitudes, but the relative contribution from <45° increases while the relative contribution from >60°N decreases. This is described now in the revised section “4.3 Influence of dynamical changes”

L515: A figure showing regional mean precipitation change in the historical/future scenarios from individual models with error bars would give a better idea of the magnitude of precipitation changes.

That’s true. However, in the context here we aim to explicitly emphasize the strengthened AR influence on heavy precipitation. The mean changes even on a seasonal resolution have been investigated in Gröger et al. (2021, https://link.springer.com/article/10.1007/s00382-020-05489-8, Figs 2 & 3).

Section 6:

L590: Figure 9 shows the opposite result. Please explain the same.

This sentence is removed in the new version.

Minor comments:

1. Please carefully go through the text and sentences and correct the typos, values, and grammar.

2. Authors are requested to re-check the references as some of them are not matching the context they are cited.

3. Please improve the quality of the figures

We have strongly revised the manuscript and the text was revised by a native speaker. Also the figures quality has been improved.