

Response to review report 1

I acknowledge the great effort made by the authors in improving their manuscript and responding to my comments. The manuscript was much nicer to read and clearer but I still have some minor comments listed below following the manuscript order.

We thank the reviewer for a thorough second round review. We carefully considered all recommendations which really helped to further improve the manuscript.

Abstract, Lines 19-20: I think the authors should add here the downscaling of 9 historical simulations and of the ERAI reanalysis as well.

We agree and have complemented the information about the hindcast and historical imulations

Table 2:

- The time period 2070-2099 should be written too for the three RCP.

Done.

- In the caption, I find “the original ERAI hindcast data” a bit misleading. Could the authors replace it with the ERAI reanalysis?

Done.

Section 2.2, Line 184: The title is also misleading because the authors write about the hindcast produced with the ERAI reanalysis. I suggest to remove this small title as it is not needed.

We agree and removed the title.

Section 3.3:

- Line 206: the unit of the gravitational acceleration should be $[m\ s^{-2}]$ and not “ $[m^3/(kg * s^2)]$ ”.

Of course. This corrected now.

- The authors could also mention here the unit of the IVT as well ($kg\ m^{-1}\ s^{-1}$).

We agree and have included the unit in line 200.

- Point 8 from line 245 onwards: The authors should mention if they use an absolute value or a percentile of IVT as threshold to define the AR mask. From Fig. 3, the mask seems to be defined with IVT above the 85th percentile of each grid point.

The masks were generated by mapping the exceedance of the percentile thresholds shown in Figure 2. We have made clear this now.

Section 2.5, Line 304: I believe the word “heavy” should not appear here.

We removed the word “heavy” here.

Section 3.2:

- Lines 338-339: Maybe refer to Fig. 3 at the end of the sentence “However, ... and lifetime.”

Good idea. We included the reference to Fig. 3.

- Line 350: after “warming”, I would add “compared to the other models”.

Done.

- In the paragraph made by lines 348-351: since the authors emphasise the different behaviour of MIROC, they could maybe also add that RCA-MIROC is the model with the lowest number of AR detected in all simulations (historical and future), as far as I can see in Table 3.

We now noted that the increase for RCA-MIROC is lowest among the models in RCP8.5 (line 355).

Table 3: some white spaces are missing between the numbers and parenthesis in column 3 for RCP2.6.

Thank you. We have inserted the lacking white spaces now.

Section 3.3, Line 367: The reference to Fig. 2 is not needed here and also a bit confusing.

We removed the reference.

Section 3.4, Lines 421-425: do the authors mean that there might not be only one IVT mask for some time steps?
No. Any double entries for IVT masks were removed during the detection (as stated section 2.3, page 7, point 6)

Section 3.5:

- **Lines 438-439: “ENSM” -> MEAN. I believe the references to the figures are wrong. It should be: 0.86 for %AMP (Fig. 5b), 0.82 for %TP (Fig 5d), and 0.92 for %95P (Fig. 5c). I suggest to switch the last two to get the figures in alphabetical order (b, c, and d).**

We turned ENSM → MEAN.

Thank you for the correction. The figure references are now in the right order.

- **Line 451: Between the two sentences “...hindcast.” and “However, ...”, I would also point out that RCA-MEAN and RCA-ERA-Interim highly overestimate the number of AR in September compared to ERA-Interim but underestimate it in October. This was mentioned in the previous version of the manuscript (see lines 748-760 of the tracked-changes version). The authors do not need to provide an explanation for this behaviour.**

We re-included the following sentence at that place:

“In addition, RCA-MEAN and RCA-ERA-Interim highly overestimate the number of AR in September compared to ERA-Interim but underestimate it in October.”

Figure 7: “Note no realization are available for RCA-IPSL, RCA-CAN and RCA-CNRM.” Please add “that” between “Note” and “no” and mention that it is for rcp26 only at the end.

Done.

Figure 8: “Difference between 2070-2099 minus 1970-1999” -> Difference between 2070-2099 and 1970-1999. The caption should also mention that the figure displays the ensemble mean difference.

Fully agree. We added this important information in the caption now.

Section 4.2:

- **Line 502: “nearly everywhere.” -> nearly everywhere for RCP45 and RCP85.**

- **Line 511: “The changes” -> The relative changes.**

Thank you for the corrections. They are included now.

Figures 9 and 10: both figures have a different projection than the rest of the maps (Figs. 5, 8, and 12).

Yes, but since the differences in the projections are of cosmetic nature and so we think this is acceptable.

Figure 9: as for Fig. 8 mention in the caption that this is an ensemble mean of the relative change.

Done.

Figure 10: same comment as for Figs. 8 and 9: this is an ensemble mean.

Done.

Section 4.3, Line 538: Yes, there is a reduction over Norway but also over many European mountainous regions, such as the Pyrenees, Massif Central, Alps, mountains in the Balkans. The authors could also mention the high absolute values in Scandinavia in panels (a) and (c) of Fig. 10 which seem to be linked to the lack of sea ice in the Gulf of Bothnia in the future.

We agree and now mentioned also the other mountainous regions. We are a bit careful pointing explicitly to a potential relationship to the sea ice retreat in the Gulf of Bothnia without further analysis. However, this definitely merits investigation in a follow up process study.

Section 5.1:

- **Line 556: What do the authors mean with “models’ solutions”? Should it be responses instead of “solutions”?**

We replaced “model solutions” by “model results”.

- **Line 566: I would remove “highly” because there is some variation among the models.**

Done.

- **Line 659: “Fig. 8a” -> Fig. 12a.**

Thank you for the correction!

- **Line 575: “along the Norwegian coast”: This statement can be extended to the whole Scandinavia.**

Done.

- **Line 580: “smaller” than when? I believe it should say smaller in the future (compared to the historical period).**
Correct. We added “in the future” at this place.

- **Line 591: add RCA-ECE here as well.**
Done.

- **Line 592: “decrease, occurring in the UK; was” -> decrease over UK was.**
Done.

Figure 12: please add in the caption that the figure is valid for scenario RCP85.
Done.

Section 5.2:

- **Line 617: “a robust climate change signal”: I would not call it “robust” but high.**
We now changed the sentence to “ a strong climate signal”

- **Lines 625-627: instead of or in addition to the low-level jet stream, I would mention the storm tracks with a reference to Zappa et al. (2013) their Fig. 2a, which I think is going in the direction of the present study’s results.**
We agree this is more specific and fits better in the context. We included the reference to Zappa et al. (2013) and storm tracks.

Section 6:

- **Line 635: “distinct N-S gradients”: to me, the Iberian peninsula rather shows E-W gradients in the ERAI reanalysis. RCA-ERAI shows gradients in both directions because of the orography.**
Fully correct. We rephrased this sentence accordingly (line 648).

- **Lines 636-637: This sentence should be rewritten as it is not clear. Suggestion: The AR imprint on the analysed indices in the ERAI dataset was weak over Iberia but stronger in distant parts of Eastern Europe compared to the downscaled RCA-ERAI (Fig. 5).**

Thank you for the suggestions. We changed the text accordingly.

- **Line 678: “is coarse” -> is still relatively coarse.**
Done.

Technical details:

Line 79: “(Albano et al., 2017;)” Is there a reference missing? Otherwise, should be (Albano et al., 2017).
Changed to (Albano et al., 2017).

Line 83: “Held and Soden ,2006” -> Held and Soden, 2006
Done.

Lines 112,113,114: put commas at the end of the three lines.
Done.

Line 220: “bin specific” -> bin-specific
Done.

Lines 230 and 231: remove the commas in the two references (after Villarini and after al.).
Done.

Line 322: “equator to pole” -> “equator-to-pole”
Done.

Line 327: delete one “and the”.
Done.

Line 390: “Downscling” -> Downscaling

Done.

Line 442: “occurs” -> occur

Done.

Line 538: “a~20%” -> a ~20%

Done.

Line 539: “Fig.10a” -> Fig. 10A

Done.

Line 588: “standard deviations were” -> standard deviation was. The authors write about only one standard deviation here, the one displayed in Fig. 12c.

Done.

Line 627: “Pasquier et a., 2018” -> Pasquier et al., 2018

Response to review report 2

Many thanks to the authors for addressing the review comments carefully and revising the manuscript accordingly. I strongly believe that the manuscript has been improved a lot through changes and additions made by the authors. Despite the changes made, I would recommend authors to make a minor revision to improve the article further and make it suitable for publication in the journal Earth System Dynamics.

We thank the reviewer for a thorough second round review. We carefully considered all recommendations which really helped to further improve the manuscript.

Comments:

1. Ln-23: Please write full form or expand ERAI at first instance.

We now wrote the full form at line 23.

2. Authors may discuss more about interpolation techniques used in comparing downscaled RCA-ERAI with ERAI reanalysis data.

We have added a note in line 188 that we use the bilinear remapping technique provided by “climate data operators” package (Schulzweida et al. 2021) where more details can be found. However, a robust discussion about interpolation techniques would require more analysis which is not the scope of the present paper.

Schulzweida, U.: CDO User Guide (Version 2.0.0). Zenodo. <http://doi.org/10.5281/zenodo.5614769>, 2021.

3. Ln-25: Please improve the abstract by providing reasons for the results discussed in the abstract.

We added the discussed changed storm track activity over the North Atlantic and/or changed weather regimes as potential driver of the found local changes in the AR pathway over Europe (last sentence of the abstract; see also point 4 below).

However, we would like to avoid to make the abstract too long and too detailed but rather provide a summary of the main results. A condensed text summarizing main results and the underlying reasons is already provided in section 6 “Summary and conclusion” at the end of the article.

4. Ln-42: Authors may discuss these results in connection with the North Atlantic winter storm activity and frequency changes.

Good idea. We now added the sentence here:

“The found changes in the local AR pathway are probably driven by larger scale circulation changes such as a change in dominating weather regimes and/or changes in the winter storm track over the North Atlantic”

5. Ln-78: Is it local groundwater recharge?

Basically yes, in particular in semi-arid regions. We mentioned this explicitly in the next sentence (line 81).

6. Ln-136: I would assume that authors used HIRLAM based Rossby Centre regional atmospheric climate model in which atmospheric model RCA and ocean model NEMO were used. However, in the text, it is mentioned that the Rossby Centre regional atmosphere model, which might be confusing to the reader. Please make necessary corrections carefully.

We slightly adapted the sentence to be more precise:

“The atmospheric part of the regional climate model (Wang et al., 2015; Gröger et al., 2015; Dieterich et al., 2019) is based on Rossby Center regional atmosphere model RCA (Samuelsson et al., 2011; Kupiainen et al., 2014) version 4.”

The RCA model version 4 was then coupled to the NEMO3.3 high resolution model (North Sea + Baltic Sea) as introduced by Wang et al. (2015) and Dieterich et al. (2019).

7. Ln-186: Please detail the interpolation techniques if any are used.

We used the bilinear remapping technique. This is now mentioned in the last paragraph of section 2.2 “ the high resolution ensemble (line 190).

8. Ln-225: Though it is a well-known approach for Europe, I suspect a drawback back in it. Here authors are comparing the 85th percentile along 10 degrees west with westward and eastward grid cells. This approach might suites well for the westward grid cells where in many instances, moisture transport occurs from the west or southwest. But assuming the same threshold for the eastward grid cells might eliminate a few grid cells from the actual AR imprint. This is because the AR landfall at the western Europe boundary could decrease IVT along the eastern landmass due to the loss of IVT in the form of precipitation and the moisture cut-off from the ocean. Hence the less IVT values during the subsequent time steps in a given AR. As pointed out in line 366, Instead, authors may consider taking the 85th percentile at each grid specially and find the AR.

We agree the standard approach may be subject to some inaccuracies especially over distal land land parts when the AR becomes weaker. However, we are hesitant with the proposed solution, i.e. taking the local 85 percentile, which we think would more robust empirical evidence and tests against the standard method. This would require much more research and is definitely beyond the scope of the present paper. Therefore, we here decided to rely on the established approach as the other studies did for Europe.

9. Ln-274: Should it be RCP 8.5? Please check Figures 7 and 8 labels as well.

Thank you. We corrected the typo. We also harmonized the format of the labels in Figures 7 and 8 with the labels in the text and in the other figures.

10. Fig-3: It would be nice to see an additional figure displaying a special difference between reanalysis and hindcast.

We principally agree. However, the results discussed in the text are already clear and well supported by comparing the middle column (RCA-ERA1) and the right hand column in Figure 5. Therefore, we think an RCA-ERA1 - ERA1 difference plot is not necessary here in the context of the text.

11. Ln-285: It is not entirely clear whether authors consider AR days with precipitation or without precipitation in computing ARF.

We did not distinguish this. The analysis of ARF is based on IVT only. We added the sentence “No distinction was made whether an AR day was associated by precipitation or not.”

at line 293 to make this clear.

12. Ln-327: Please check and correct the sentence.

Done. A double “and the” was removed from the sentence (see also the above response to report 1 (line 327)).

13. Ln-330: Please try to elaborate on the dynamics influencing the IVT decrease in RCA-HAD in the RCP 8.5. For example, you may consider citing the recent paper from Venugopal et al., (2021), who have studied the decadal changes in specific humidity and wind components leading to changes in IVT over the North Atlantic using reanalysis data.

Thank you for the reference. We have added a note on the potential role of decadal variation in wind components and specific humidity with a reference to the study of Venugopal Thandlam (line 337):

“Also decadal variations in wind components and specific humidity have to be considered in this context (Thandlam et al., 2021).”

14. Ln-438: Is it RCA-MEAN in place of RCA-ENSM?

RCA-ENSM has been changed to RCA-MEAN now.

15. Ln-445: Very interested to see that RCA-MEAN failed to reproduce the precipitation but IVT. Please consider including more details and causes for the same for the reader's benefit.

We can only speculate on that and can not give a final explanation based on our results. However, we added the following note (line 454):

“We note that the exact reproduction of precipitation pattern is difficult in coarse resolution models due to insufficient description in cloud physics and/or the treatment of convective energy (e.g Prein et al., 2013; Hohenegger et al., 2020)”

Prein, A.F., Gobiet, A., Suklitsch, M. *et al.* Added value of convection permitting seasonal simulations. *Clim Dyn* **41**, 2655–2677 (2013). <https://doi.org/10.1007/s00382-013-1744-6>

Hohenegger, C., L. Kornblueh, D. Klocke, T. Becker, G. Cioni, J. F. Engels, U. Schulzweida, and B. Stevens, 2020: Climate statistics in global simulations of the atmosphere, from 80 to 2.5 km grid spacing. *J. Meteor. Soc. Japan*, 98, 73-91

16. Ln-547: The statement is valid if the North Atlantic SSTs are strong enough to permit the convection in summer. But in winter, the scenario mostly depends on the Arctic cold air outbreaks and convergence of the moisture from the adjacent areas into the AR.

Yes. We made this more clear by adding the sentence:

“This is in particular the case during the warm season when SST are high enough to allow convection or during winter when moisture convergence advects moisture from adjacent areas.”

17. Fig-12: Does the inter-model standard deviation means the average standard deviation obtained from different RCA ensembles?

No. It is the standard deviation across all 9 models (RCA-HAD, RCA-ECE...). We made this more clear in the figure caption by adding “...across all 9 models (CAN, CNRM,...NORESM)”

18. Ln-635: Please check the sentence as it is contradicting.

Done. See our response to review report 1 on the same issue.