Author Response to review comments for Christensen et al: Atmospheric regional climate projections for the Baltic Sea Region until 2100

Reviewer 1 Jouni Räisänen

GENERAL COMMENTS

The paper reviews regional climate model (RCM) projections of 21st century climate change in the Baltic Sea region. It is mainly based on a large ensemble of high-resolution (12.5 km) atmospheric RCM simulations produced in the EURO-CORDEX project, but also uses a smaller ensemble of simulations with a single coupled atmosphere – Baltic Sea RCM to assess the effect of the regional atmosphere-ocean coupling. Furthermore, comparison is made with the ENSEMBLES RCM simulations used in the previous Baltic Sea Basic Climate Change Assessment. Six climate variables (temperature, precipitation, wind speed, solar radiation, snow cover and Baltic Sea ice) are covered.

The main value of this paper is in the vast volume of results that are put together – in terms of both the number of model simulations and number of variables. This will make the paper a valuable resource for those needing an overview of climate change projections in the Baltic Sea area, even though there are few surprises in the results compared with earlier generations of model simulations. Naturally, the wide coverage comes at the cost that the physical mechanisms behind the projected changes cannot be discussed in much depth (although some attempts are made), and the results of individual simulations only appear as points in scatter diagrams. Nevertheless, the analysis methods are sound, and, with a couple of minor exceptions, the interpretation of the results is well justified.

The largest need of development in this paper concerns the quality of its graphics. The general approach where multi-panel figures are compared with scatter diagrams to represent the typical features and variation between model results works well, particularly in Section 3. Beyond this, however, there are many ways in which the reader-friendliness and informativeness of the figures could be improved. Suggestions for this are given below in “Comments on figures”. Other detailed comments are collected under “Comments on substance and text” and “Minor technical comments”.

The authors thank Jouni Räisänen for this exceedingly thorough and constructive review! We are not in disagreement with any of the comments made, and the manuscript will be changed according to the detailed suggestions. A few replies to specific comments can be found below.

COMMENTS ON FIGURES

Many of the figures in the manuscript could be fine-tuned for a better reader experience. In particular,

1. In multi-panel figures like Fig. 1 (and all the others in the same format), it is annoying for the reader to have to look back and forth between the figure and the caption to try to identify which panel is which. This can be improved by adding the relevant information directly into the figure. In case of Fig. 1, this can be done by adding the texts “25%”,

"50%" and "75%" above the three columns and the texts "Winter" and "Summer" to the left of the two rows.

2. The scatter diagrams (Figure 3 and other similar figures) would be easier to understand if a legend on the meaning of the different markers and colours were added directly to (at least) the first figure panel.
3. The scatter diagrams could also be improved by using coloured markers, not only for the coupled RCA4-NEMO ensemble but also for the EURO-CORDEX simulations. As it stands now, the different scenarios and data sets are difficult to separate visually, particularly in Fig. 3 where the number of data points is the largest. Use of colours would also allow a slight decrease in the symbol size, thus reducing the crowding in the diagrams.
4. Still one suggestion for the scatter diagrams: add horizontal and vertical zero lines to make it easier to count/estimate the number of simulations with positive and negative changes.
5. The map collections related to Section 4 (Figs. 10, 11, 13 and 15) need rethinking. The focus and new information in this section is the effect of the Baltic Sea – atmosphere coupling on the projected changes, not the uncertainty in the projections. Therefore, the lower and upper quartile maps appear redundant. Instead, it would seem better to show just three maps for each case: the median for the uncoupled simulations, that for the coupled simulations, and the difference between the latter and the former. Apart from focusing on the results that are of the highest relevance for this section, this would halve the total number of figure panels.

   We understand the comment and agree that the quartile maps should be removed. This has been done.

6. Figures 12 and 14 are not mentioned at all in the text, and Figure 16 is only mentioned very briefly. If there is no need to discuss these figures in the text, they should be omitted.
7. If Figures 12, 14 and 16 are retained: please use colours. Otherwise, it is very difficult to distinguish between the coupled and uncoupled simulations.

   We have revised and made figures and text consistent.

8. The colour scale in the figures that show changes in solar irradiation (Figs. 7, 15 and S19-S24) is potentially misleading. Intuitively, red and yellow colours are linked to drier conditions (hence more solar radiation) and green colours to wetter conditions (hence less solar radiation). This is just the opposite to the scale in these figures.

   The figures have been revised accordingly.

9. Figure 9. Remove the titles (which are too long, and do not differentiate the coupled and the coupled simulations). Add the labels "Uncoupled" / "Coupled" to the left of the two rows, and "25%" / "50%" / "75%" above the three columns.
10. Figures S1-S24. Please label the periods ("2041-2070" and "2071-2100") to the left of the figures and the percentiles ("25%" / "50%" / "75%") on the top of the figures.
11. Figure 3(d) should represent land south of 60°N in DJF, not land north of 60°N.

This mistake has been corrected.

COMMENTS ON SUBSTANCE AND TEXT

1. L12-15. I think the focus on the 12.5 km simulations should be mentioned in the abstract.

   The abstract text has been adapted.

2. The text from L86 to L113 is difficult to follow, partly because it jumps back and forth between the EURO-CORDEX and BACC II / ENSEMBLES simulations and partly because the EURO-CORDEX part is described in somewhat surprising order. Please first describe the EURO-CORDEX simulations, proceeding from the general (scenarios and periods, plus the "pattern scaling" sentence on L97-99) to the details (notes on missing data on L86-90). After this is done, proceed to the comparison with the earlier BACC II / ENSEMBLES simulations (L91-95) and to the way of presentation of results (L113-124, excluding the first sentence that should come earlier).

   We have revised the text accordingly. Also the first sentence of Ch 2 was moved to where we start describing the EURO-CORDEX data. The statement on the global mean change in the BACCII simulations has been removed being more a result than a statement describing the data (a comparison between global means is mentioned later).

3. It would be good to repeat the definition of the baseline, mid-century and end-century periods in caption of Table 2.

   The manuscript has been revised accordingly.

4. L110. "many years" is an understatement. This is many decades.

   The manuscript has been revised accordingly.

5. L131. Mention the resolution of the RCA4 simulations.

   Done.

6. L143-144. The underestimation of the inter-quartile spread is not self-evident. If the 8 GCMs can be considered as a random sample from CMIP5, the expected value of the (n-1) variance should be the same as for the whole ensemble. The same may or may not apply to the inter-quartile spread, depending on how the quartiles have been estimated.

   This is a valid point, and the original text was much too generalizing. We have referrred to a relevant reference, writing:

   As only 8 GCMs have been used for these RCP8.5 RCM experiments, the spread between quartiles could be lower than what would have come from an exhaustive downscaling of all CMIP5 global simulations; Kjellström et al. (2016) compared 9 GCMs, including the 8 GCMs analysed here, to a larger CMIP5 ensemble and found the small-ensemble spread over Sweden to be comparable in summer, but smaller in winter.

7. L154. What does "most extreme" refer to? The simulations with the largest warming or larger warming of the highest temperatures?

   Rephrased so that it now explicitly talks about ”the upper quartile”.

8. L157. Do you mean the ice-albedo feedback mechanism over the Arctic Ocean? There is no sea ice, and only little snow in the highest mountains, left in JJA in this region even in the present-day climate.

   True. The ice-albedo feedback is seen much further to the north, over more central parts of the Arctic ocean. We have changed to ”potentially connected to the larger temperature increases further to the north in the Arctic (IPCC, 2021)”.

9. L162. summer, winter or annual mean temperature trends?

   Have added that this is both for summer and winter.

10. L164-167. This text oversimplifies the dynamics of diurnal temperature range (DTR) changes, which originate from a multitude of factors (e.g., Lindvall, J. & Svensson, G, 2015: The diurnal temperature range in the CMIP5 models. Clim. Dyn. 44, 405–421). In addition to the processes discussed in the mentioned paper, it should be noted that the genuine diurnal temperature range is very small in the middle of the winter when there is little solar radiation. However, differences between the daily maximum and minimum temperatures can still be substantial due to synoptic-scale weather variability. Factors that reduce the temperature variability on synoptic time scales (e.g., reduced temperature gradients between the Atlantic Ocean and Eurasia) therefore also likely contribute to the apparent decrease in DTR.

    We have replaced the sentence ”This is a direct consequence ...” with ”A range of factors may be responsible for this decrease in difference between minimum and maximum temperatures. This could involve changes in the diurnal temperature range (e.g. Lindvall and Svensson, 2015) or changes in the synoptic weather variability in combination with reduced large-scale temperature gradients between the Atlantic Ocean and the Eurasian continent (IPCC, 2021).”

11. L177-178. Suggested rewording: … (Norway), where the amount of precipitation is particularly sensitive to different changes in the large-scale circulation?

    The manuscript has been revised accordingly.

12. L209. Apparently, this should be “squared correlation coefficients of 0.5 to 0.6.

    Thanks for pointing this out. Has now been corrected.

13. L216-129. This is not true for temperature change in summer (for the total region, warming of ca. 2.9 K in BACC II and 3.6 K in EURO-CORDEX).

    Correct, thanks for spotting this mistake! We have changed the text by adding ”generally” and ”apart from land areas in summer where the BACC II change is only about 80% of

the RCP8.5 result (+6.5% vs. +8.2%)". It has also led us to rephrase the conclusions (see also your point #25 below).

14. L285-294. This text does not fit well in Section 3.3 on "Extreme precipitation". Rather place it in the end of Section 3.2.

    The text has been revised.

15. L319-324. When discussing the geographical distribution of wind speed changes, also refer to Figs. S13-S18.

    We have added the reference.

16. L367-369. It seems that the aerosol issue should already have been mentioned when discussing simulated temperature change in Section 3.1.

    We have added "A potential source of difference between GCMs and RCMs is the different treatment of aerosols in these models. Many of the RCMs do not include time-varying anthropogenic aerosols leading to weaker future warming compared to GCMs (Boé et al., 2020)." also to the temperature chapter (where we describe similarities between GCMs and RCMs). Here, in the chapter on solar irradiation we repeat the message about different treatment of aerosols in GCMs and RCMs without explicitly talking about differences in warming.

17. L379-381. This article, based on the EURO-CORDEX 12.5 km RCMs, might also be cited: Räisänen, J., 2021: Snow conditions in northern Europe: the dynamics of interannual variability versus projected long-term change, The Cryosphere, 15, 1677–1696, https://doi.org/10.5194/tc-15-1677-2021. The conclusions are largely the same as in Räisänen and Eklund (2011).

    Thanks for pointing to this study! We have added "Räisänen (2021) found a widespread future decrease in northern Europe for snow water equivalents also for a set of EURO-CORDEX RCMs. It was shown that a smaller snowfall fraction together with larger reduction of snow on ground more than compensated for increasing precipitation as seen in several of the RCMs. In relative numbers the decrease was found to be larger in southern warmer parts of Scandinavia while changes in absolute numbers are larger in the north. Similarly, the results were ambiguous for the most high-altitude parts of the Scandinavian mountains where some models indicate increasing snow water and others a decrease."

18. L387-388. This is not only, and perhaps not primarily, about orography. The baseline climate in the northern areas is colder due to the smaller amount of solar radiation as well.

    We have revised the text and now starts with "... the generally colder climate and smaller amount of solar radiation" before bringing up the orographic part.

19. L390-391. This might also be affected by the larger increase in winter precipitation in the BACC II simulations, at least north of 60N (Figure 3c and Tables S9-S10).

This last sentence has now been rephrased so that it reads ” This is consistent both with the fact that the RCP8.5 scenario on average projects larger warming than the SRES A1B scenario used in BACC II and that the precipitation increase is smaller in the RCP8.5 scenario than in SRES A1B, at least north of 60°N (cf. Fig. 3c)”.

20. L394. The increase in temperatures has an impact on snow cover even in high-altitude areas. Even if temperature generally remains below zero in the middle of winter, the frost season starts later in fall and therefore the accumulation of snow starts later. See Räisänen & Eklund (2011) or Räisänen (2021) (as cited in comment 17 above).

    Added to the end of the paragraph ” However, also in these high-altitude regions, the warmer future climate results in a shorter snow season with accumulation starting later and spring melt starting earlier that acts to reduce the total amount of snow (Räisänen et al., 2021).”

21. L434-435. This might also be because the coupling has a similar effect on temperature in both the baseline and the future periods.

    We agree. Without further analysis our statement:

    "This is probably due to the fact that air temperature anomalies generated locally over the open sea disperse rapidly in the atmosphere." has to remain quite speculative. We have removed this sentence.

22. L442-443. Based on Figure 9, many of the uncoupled simulations had no sea ice over the northernmost parts of the Baltic Sea, and thus no decrease in sea ice. It is therefore not surprising that the warming is larger in the coupled simulations in which the ice cover decreases (as it must as the climate warms).

    We agree and have added the explanation to this paragraph.

23. L458-475. Please refer to Fig. 13 when discussing the wind speed changes. Also, the main point of interest should be the effect of the coupling on the wind speed changes over and near the Baltic Sea. What happens at the Norwegian coast must be an artefact of the resolution difference, and uninteresting as such. Similarly, the discussion (as well as Figs. 10-11 and 13) could focus just on the median changes, because the uncertainty range is not the primary point of interest in this context.

    We have revised the text, only discussing winter change, and as mentioned elsewhere, we will remove quartile plots from this part of the manuscript as suggested.

24. L479-483. The earlier text gives the impression that the three columns in Fig. 15 and other similar maps represent the 25th, 50th and 75th percentiles of time mean changes in the ensemble - i.e., variation between simulations and not from day to day. Please check this text and revise what is needed.

    This is indeed our definition of a quartile. In accordance with comment 2 on Figures, we have removed the quartile plots from the discussion about the coupled simulations. Therefore the misleading text has been reformulated.

25. L569. Based on Fig. 3, this applies in winter but not in summer.

    We have changed the text here so that it is clear that it addresses winter. The last sentence of the paragraph has been changed to: ”For summer, the differences are larger and it cannot be generally concluded if the regional sensitivity to global climate change is different from what it was in BACC II.”

26. L582-584. Could the decrease in winter also be related to reduced snow cover? Lower surface albedo reduces multiple reflection between the surface and clouds, thereby attenuating the gross downward solar radiation flux. See the suggestion on p. 2472 in Ruosteenoja, K., & Räisänen, P. 2013: Seasonal changes in solar radiation and relative humidity in Europe in response to global warming, Journal of Climate, 26(8), 2467-2481.

    This is an interesting idea. We haven’t investigated it in our results but we add a statement with a reference to the suggested paper at the end of the chapter on solar irradiation: ”It has also been suggested that reduced snow cover (see Ch. 3.6 below) could contribute to attenuate gross downward solar radiation flux as the reduced surface albedo reduces multiple reflection between the surface and the clouds (Ruosteenoja and Räisänen, 2013).” Here, in the conclusions we added ”and potentially also less snow”.

27. L592-593. Suggesting rewording: “… terrain, likely as an artifact of different model resolution”. I would not call this an uncertainty, because it is obvious that higher resolution is better.

    We agree with this point, and the text has been revised.

MINOR TECHNICAL COMMENTS

1. L18-20. Suggested rewording of sentence: “In simulations with a coupled atmosphere-ocean model, the climate change signal is locally modified relative to the corresponding stand-alone atmosphere regional climate model”. The text this far has not defined the coupled atmosphere-ocean model in question, which makes its definite article confusing.
2. L24. coupled model inter-comparison projects (CMIPs) OR model inter-comparison projects (MIPs)
3. L38. Keuler et al. (2016) is missing from the list of references. Please also check the list for other possible omissions.
4. L148. Nikulin et al. (2011) *used* an ensemble
5. L179. Delete the first ”winter”.
6. L270. … higher resolution, which allows them to avoid?
7. L283-284. simpler language: the increase in precipitation extremes is strongly dependent on moisture availability?
8. L310. Suggested rewording for the beginning of the sentence: “Donat et al. (2011) analysed the annual 98th percentile”. As it stands now, the beginning and the end of the sentence are not consistent.
9. L326. Fig. 15 should be Fig. 13
10. L377. Typo in “becausesnow”

11. L421. "a more detailed look at the five driving GCMs" should be reformulated, because no results for the GCMs themselves are shown.
12. L473. < 2% (2 m/s would be a huge change)
13. Caption of Table S20. Standard deviation of precipitation change, not temperature change.

We have corrected the text following all these minor points.

Reviewer 2

The authors thank anonymous reviewer 2 for the very useful comments. In general we agree with the suggested changes. A few remarks can be found below.

The manuscript updates former climate assessment reports for the Baltic Sea region with respect to the discussion of climate projections. They use simulated projections by an ensemble of atmosphere-only regional climate models (RCM) within the Euro-CORDEX (cf. www.cordex.org) initiative and additionally they use an ensemble of simulations by a coupled RCM (with RCA4-NEMO) which has been discussed in literature before. It would be nice to see a model ensemble of coupled simulations, but they are not available or at least not as easily available as the Euro-CORDEX simulations. In my opinion, the assessment update is worth to be published because it discusses causes of changes in the projections too. However, the manuscript needs some improvements before.

In the abstract the authors write “in this review paper, we will concentrate”. The manuscript does not properly review, but in put the new simulations in context. I suggest to change the wording.

This paragraph has been reformulated, as it does not describe properly what the paper contains. We will take this comment into account.

Line 140: “higher warming is expected for land areas” – the given arguments (a) miss references and (b) miss additional causes (e.g. as water vapor which is mentioned by the authors later at line 166). Line 143: “the spread between quartiles is lower” – here too references are missing. The manuscript tends to lack supporting references.

The sentence starting on L140 has been revised and now reads ”Larger warming than the global average is generally expected for land areas, which warm more quickly than sea areas where also enhanced evaporation tends to reduce warming (e.g. Sutton et al., 2007)); it is most clearly seen in winter in the eastern part of the area.”. We’ve also added a reference to the sentence starting on L143.

Line 179: Is it possible that the simulated differential changes, change patterns in precipitation are biased by the selection of GCMs or the over-representation of GCMs (looking at Tab. 1, 3 of 8 GCMs are more often applied than average)?

This is indeed possible, but the net effect is small due to the already large ensemble. A direct average, as also done here, has been compared to a ”democratic” matrix with emulated values used for missing GCM-RCM combinations in a Deliverable report, D1.4.2 from the H2020 PRINCIPLES project. The technique is described in Christensen and Kjellström (2021). The effect of the incomplete combination matrix is negligible for the area and fields described in the current manuscript.

Christensen, O.B., and Kjellström, E. 2021: Filling the Matrix: An ANOVA-Based Method to Emulate Regional Climate Model Simulations for Equally-Weighted Properties of Ensembles of Opportunity, Clim. Dyn. In press. https://doi.org/10.21203/rs.3.rs-366374/v1

Minor issues:

- Line 25: There is AR6 now using SSP scenarios. Should be mentioned, even with no RCM ensembles available yet.

  Here, we have added " The most recent, sixth assessment report (IPCC 2021; AR6) build on the successor CMIP6 (Eyring et al., 2016) that involves a new set of Shared Socioeconomic Pathway (SSP) scenarios (O'Neill et al., 2017). This has, however, not been addressed here as, at this point, downscaling activities based on CMIP6 projections are still lacking."

- Lines 27 - 100: I have difficulties to understand these sentences and they are probably better placed in the Results section.
- Line 248: ".."
- Line 272: The lower limit of grey zone grid-spacing is ca. 3-5 km.

  We agree. This has been revised.

- Line 377 "becausesnow"

  This has been corrected.

- Line 393-4: Two times "may". Are there other possible causes?

  We have revised the text in the manuscript

- Line 415: This is not true for all atmosphere-only models. Some have their own sea-ice parameterizations which were used, I guess, in their Euro-CORDEX set-up.

  We have revised the text in the manuscript

- Line 421: How many ensemble members? Only 5 members in total?

  Only 5 ensemble members are represented in both ensembles, coupled and atmosphere-only. There are, unfortunately, known errors in the CNRM GCM simulation.

- Line 572: The sensitivity of the regional models' newer versions decreased? Is there any idea why?

  We have revised this statement on regional sensitivity. Differences between the ensembles are very small in winter but more in line with the differences in the global mean temperature for summer.

- Some of the Figs. titles are cut off (e.g. 11, S26, 27)

  This has been corrected, replacing the panel titles with column and row labels in the figure.

Reviewer 3

The authors would like to thank the anonymous reviewer for very useful comments. Most of the suggested changes will be carried out in the revision. Some specific rebuttal comments can be found below.

The manuscript is about an analysis of the results of a large number of simulations with several different atmospheric regional climate models. In addition the authors include 22 simulations performed with the coupled atmosphere-ocean model RCA4-NEMO. The climate changes of the quantities 2 metre temperature, precipitation (including extreme precipitation), wind speed, and solar irradiation are taken into account.

The manuscript is well structured and the figures are acceptable but the different symbols in figures like Fig.3 are hard to distinguish (e.g. the scenario means are not easy to spot).

The figures have been revised for several reasons, this being one of them.

While publications of analyses over the Baltic Sea catchment area for the above mentioned quantities are numerous throughout the last decade, the present analysis adds value by including a large ensemble of simulations of different regional climate models and for regional atmosphere-ocean coupled models a number of 22 climate simulations is also outstanding.

One critical comment from my point of view is that the authors did not took the opportunity to investigate a few additional quantities (e.g. sunshine duration, daily temperature range etc.) The ESGF provides a lot of these additional quantities and it is a pity that these data are used only very rarely.

This is a very good suggestion by the reviewer. However, the aim of the paper is to compare the latest results of scenario simulations with earlier simulations used for the assessment in BACC II and these additional quantities have not been analyzed previously. In addition, the paper is already quite long.

I would recommend the manuscript to be published after taking into account the few comments below.

Lines 137-138

„Higher warming than the global average is generally expected for land areas, which warm more quickly than sea areas“. I agree, however, this cannot clearly be seen in Fig.1 where the North-South gradient is more dominant.

We have added a modification; the text now reads: ”Higher warming than the global average is generally expected for land areas, which warm more quickly than sea areas where also enhanced evaporation tends to reduce warming (e.g. Sutton et al., 2007); it is most clearly seen in winter in the eastern part of the area.”

Line 361-362

„This attributes to more extensive cloud cover (not shown) in most models for the future.“

Can you add a sentence about the reason for the increase in cloud cover over the Baltic Sea Catchment area? Does the moisture in the atmosphere increase, i.e. does the precipitable water results in the ESGF archive show this? If this is not the reason what then? Maybe there is another publication which I am not aware of that covers this. If so please cite this publication.

We have revised this sentence so that it now reads ”This has been proposed to be linked to the more extensive cloud cover in northern Europe in most EURO-CORDEX RCMs for the future (Coppola et al., 2021).”

Line 391

„Also the fact that increasing temperatures may not reach the melting point is significant.“

How do the number of frost days change over the Baltic Sea catchment area? Can you give some numbers or point to a publication that covers this, please?

We have added a relevant reference and revised the text to:

”It is only in high-altitude parts of central and northern Scandinavia that changes are limited with relatively large amounts of snow also in the future. At high altitude, the increase of winter precipitation may be compensating for the increase in melting with higher temperature. Also the fact that increasing temperatures may not reach the melting point is significant; see, e.g., Gröger et al. (2021a) Fig. 12b. However, also in these high-altitude regions, the warmer future climate results in a shorter snow season with accumulation starting later and spring melt starting earlier that acts to reduce the total amount of snow (Räisänen et al., 2021).”

Figure 3

The sub figure (second column, second row) should be „land south“ not a redundant „land north“

This mistake has been corrected; thanks for noting this.

Editor comment, Marcus Reckermann

The paper is well written, scientifically sound and well documented. I think it is a very good and useful paper, which belongs to this collection of papers 100%. Apart from that, I support the reviewer´s comments.

There one aspect which the reviewers have not taken up but which is important in my view.

The authors state that they have decided to show in the main paper only RCP8.5 results, but have not given an indication why they did so. It has been discussed that the exclusive use of this scenario gives rather unrealistic results and misleading conclusions by non-experts (e.g. Hausfather and Peters, 2020). Why did the authors choose to show results from that scenario in the main paper? It may be that in their view RCP8.5 are best comparable to the SRES A1B scenarios shown in BACC II? There may be good reasons to use these very hot scenarios, but it should be made clear or at least discussed that these are not more likely than results from the less hot scenarios. If only plots of the hottest scenario are used, the reader who is not a very expert may come to the conclusion that the plots shown are more likely than the others. It is good that plots from the other scenarios are available in the supplementary material, but it should be expressed more clearly that those plots are not less likely than the RCP8.5 ones displayed in the man paper. If the authors are not of that opinion, it should be discussed.

It is perfectly understandable that the authors do not want to delve into a discussion which scenario is more likely that another as this goes beyond the scope of this paper, but it is not clear why priority was given to the most extreme scenario.

I would recommend that the authors in the text, wherever they give a number (e.g. on warming, or % increase in precipitation etc.) resulting from the RCP8.5, they should also state that number for the lower scenarios. That will not add much text but it makes clear that RCP8.5 is not the most likely. Also they may refer to the supplementary material more often.

We agree with the Editor that we should explain better why we have used RCP8.5, and text will be added. We will also point more clearly to the scatter plots and the Supplementary Material that include all available scenarios: RCP2.6, RCP4.5 and RCP8.5. Although RCP8.5 is probably an extreme scenario, we have focussed in some of the figures on RCP8.5 because the ensemble under the RCP8.5 scenario contains the largest number of members and linear scaling will approximately provide figures corresponding to the other RCPs, at least as far as average fields are concerned (e.g., Christensen et al., 2015). A direct comparison between the available data of all three RCPs is only possible by comparing the largest common subset which is much smaller (16 members) than the number of members within RCP8.5 (72 members). The availability of a large number of ensemble members enables a reduction of the impact of natural variability.

Christensen, O. B., S. Yang, F. Boberg, C. F. Maule, P. Thejll, M. Olesen, M. Drews, H. J. D. Sørup, J. H. Christensen 2015: Scalability of regional climate change in Europe for high-end scenarios. Climate Research 64 (1), 25-38 doi: 10.3354/CR01286