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Reply on RC1

Ole Bøssing Christensen et al.

Author comment on "Atmospheric regional climate projections for the Baltic Sea region until 2100" by Ole Bøssing Christensen et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-51-AC1, 2021

In the following we quote the reviewer in ordinary typeface and set our replies in boldface italics.

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,

- 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.
- 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.
- 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.
- 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.
- 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.

- 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.
- 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 will revise and make figures and text consistent.

- 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 will be revised accordingly.

- 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.
- 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.
- Figure 3(d) should represent land south of 60°N in DJF, not land north of 60°N.

This mistake will be corrected.

COMMENTS ON SUBSTANCE AND TEXT

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

The abstract text will be adapted.

- 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).

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

The manuscript will be revised accordingly.

- "many years" is an understatement. This is many decades.

The manuscript will be revised accordingly.

- Mention the resolution of the RCA4 simulations.

Will be done.

- 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.

- 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".

- 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)".

- summer, winter or annual mean temperature trends?

Have added that this is both for summer and winter.

- 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)."

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

The manuscript will be revised accordingly.

- Apparently, this should be "squared correlation coefficients of 0.5 to 0.6.

Thanks for pointing this out. Will be corrected.

- 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).

- 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.

We will revise the text.

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

We will add the reference.

- 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.

- 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. The decrease was found to be larger in southern warmer parts of Scandinavia and larger in high-elevated parts in the north."

- 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.

- 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)".

- 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)."

- 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 will therefore remove this sentence.

- 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 will add the explanation to this paragraph.

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

- 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 will remove the quartile plots from the discussion about the coupled simulations. Therefore the misleading text will be reformulated.

- 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.”

- 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”.

- 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

- 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.
- coupled model inter-comparison projects (CMIPs) OR model inter-comparison projects (MIPs)
- Keuler et al. (2016) is missing from the list of references. Please also check the list for other possible omissions.
- Nikulin et al. (2011) *used* an ensemble
- Delete the first ”winter”.
- … higher resolution, which allows them to avoid?
- L283-284. simpler language: the increase in precipitation extremes is strongly dependent on moisture availability?
- 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.
- Fig. 15 should be Fig. 13
- Typo in “becausesnow”
- "a more detailed look at the five driving GCMs" should be reformulated, because no results for the GCMs themselves are shown.
- < 2% (2 m/s would be a huge change)
- Caption of Table S20. Standard deviation of precipitation change, not temperature change.

We will correct the text following all these minor points.