

Interactive comment on “Regional climate change projections for the Barents region” by A. Dobler et al.

Anonymous Referee #2

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Summary of manuscript

The study of Dobler et al. presents regional climate projections of the Barents Sea region applying the regional climate model (RCM) COSMO-CLM. The RCM is forced by the output of two Earth System Models (MPI-ESM and EC-Earth) following different representative concentration pathways to project climate scenarios for the Barents regions for the end of the 21st century. In general, they find strongest increases in temperature, precipitation and wind anomalies over the Barents Sea, which are associated with sea ice retreat. Along with precipitation anomalies, an increase of convective cloud cover is projected as well. Relative precipitation anomalies with respect to temperature changes are most pronounced at the West coast of Norway.

Review of the manuscript

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The present manuscript of Dobler et al. gives a rather “dry” description of the individual modeled output variables without into depth analysis of the underlying mechanisms. This might also be related to the structure of the manuscript, in which the authors describe one variable after the other, but they do not provide sufficient explanation about the model dynamics and how these climate parameters interact with each other. This might be related to the fact that the authors comprise both the results and discussions sections into one part. In its present form, the manuscript mainly constitutes a presentation of the scenarios including the main simulated parameters. In such a way, as a reader, I would expect a more critical evaluation of the model output (e.g., applying more statistical analysis, and additional boundary forcings derived from more global simulations) to provide robust statements. However, with the available set of few scenarios (including rcp scenarios of two gcms as boundaries) provided in the manuscript, such a presentation style is likely not possible.

Furthermore, most of the presented results do not seem to provide much novelty compared to the work cited in the manuscript (e.g., Koenig et al.). As an alternative, the authors may refocus more into depth on the underlying causes (sea ice changes, sea surface temperature changes, . . .) and how this potentially affect the dynamics in the atmosphere. For instance, there is no comment on potential changes of the sea surface temperatures provided by the boundary forcings. Can the authors give any statements on how much of the temperature anomalies are related to CO₂ changes, changes related to the sea ice albedo feedback or maybe the effect of potential ocean heat transport changes? The authors provide data on wind magnitude changes, but do not show additional analysis on sea level pressure changes or the respective wind circulation patterns (vector fields). Also additional analysis on the changes of climate variability of the different climate factors (e.g., changes in sea ice variability, occurrence of heavy rain events, storms, etc) may help to increase the potential impact of the study. The authors may strengthen their focus on regional changes, e.g. relative changes in precipitation along the west coast of Norway, which are probably not possible to resolve by more coarse global model resolution.

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Such a refocusing of the paper may help the authors to discuss their results in a more general perspective including additional literature as well (see specific comments). These suggestions to refocus the manuscript should be rather seen as a motivation than a strict guideline for a potential revised version of the manuscript. In its present form, I therefore recommend major revisions of the manuscript. Below, there is a list of some more specific comments on the manuscript:

Page-by-page list of general comments:

In general the manuscript is clearly written and easy to follow:

Lines 1-2: This is a rather general introduction, which does not really guide to the topic. Alternatively, a more straightforward introduction would be to focus on high variability in sea ice changes and associated potential changes under the warming climate scenarios.

Lines 6-7: You do not mention that warming is closely linked to the sea ice feedback. I guess you try to avoid this because the sea ice retreat of the warming scenarios is rather a model forcing than a model outcome? Still, it should be made clear that probably the sea-ice changes cause these high warming anomalies.

Line 12: Typo, please write "increase"

Lines 23-24: For the Barents Sea, one of the most important phenomena is the sea ice feedback, which is not mentioned here.

Line 30: "[...] about the aggregated effect of physical processes". You missed to mention the synergies and nonlinear processes that are related to feedbacks in the modelled climate system.

Line 56: Please write Arctic with a capital letter.

Line 73: Is there a specific reason why the authors apply only one RCP scenario of EC-Earth to force their regional model with? In such a way the present modelling strategy

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appears to be incomplete.

Line 86: Wrong placement of the bracket

Line 95: Please write "simulating" instead of "simulation"

Line 125-126: Is this due to the low resolution forcing input of the global model, which cannot resolve the effect of detailed coastlines and orographic slopes? Please clarify.

Lines 138-147: The temperature bias of the MERRA reanalysis dataset show similar patterns like for the simulation of COSMO-CLM driven by MPI-ESM. Does it mean that the MERRA dataset is not trustworthy or that these reanalysis datasets show huge discrepancies in general? In turn, how robust are the CRU observations? In general, I think this section of the manuscript needs more analysis, e.g. scatterplots with regression lines and correlation analysis between observation, reanalysis data and the simulations. I also wonder why the authors evaluate precipitation and temperature fields over land but not over sea. If they do so due to a lack in data coverage increasing the uncertainties over the ocean, they may want to evaluate their model simulations individually over land and ocean.

Lines 153-154: "[...] and the largest changes in all runs can be seen in the northern part of the domain in winter, followed by autumn and spring". Please provide an explanation for the fact that the strongest warming occurs during winter time.

Lines 170-174: I wonder how sea ice variability is changing for the model scenarios and how the atmospheric climate responds to potentially more sea ice variability and/or more open ocean surface.

Lines 193-200: Please provide vector plots showing the atmospheric circulation pattern. How does the atmospheric circulation change for the different pressure levels? I also want to motivate the authors to speculate about different mechanisms that may play a role in a warming climate (e.g., Bengtsson et al., 2004; Semenov et al., 2009). Such a discussion may help to increase the potential impact of the study and reflects

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the importance of the Barents Sea beyond regional scales.

Bengtsson, L.; Semenov, V. A. & Johannessen, O. M. The Early Twentieth-Century Warming in the Arctic-A Possible Mechanism *J. Climate, Journal of Climate, American Meteorological Society*, 2004, 17, 4045-4057

Semenov, V. A., W. Park, and M. Latif (2009), Barents Sea inflow shutdown: A new mechanism for rapid climate changes, *Geophys. Res. Lett.*, 36, L14709, doi:10.1029/2009GL038911.

Interactive comment on *Earth Syst. Dynam. Discuss.*, doi:10.5194/esd-2016-27, 2016.