

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

Anonymous Referee #1

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The authors aim to provide an assessment of future climate change over the Barents region via a set of experiments using the COSMO-CLM regional climate model. There is also a limited intercomparison with a mini-ensemble of regional downscalings from previous studies (Koenigk et al. 2015). A range of future scenarios, which roughly correspond to low (rcp2.6), medium (rcp4.5) and high (rcp8.5) emissions trajectories, are explored. Their findings are consistent with previous work over the region and exhibit strong and robust temperature increases consistent with enhanced Arctic amplification. These changes are also closely linked to sea-ice decline in the region. However, it must be noted that sea-ice fields are provided as a lower boundary from the driving earth system models so the RCMs are not able to demonstrate any added value with

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respect to the representation of this field. Positive precipitation changes are highest along the western coast of Norway in summer and fall while some parts of Scandinavia experience drying. Consistent with other regional modeling experiments the COSMO simulation exhibits reduced cloud cover over the ocean areas, in contrast with a limited ensemble of driving global earth system models.

It is a bit difficult to identify the contribution made by this study as the findings presented largely confirm what is already known. Further, since they are based on single model simulations they are not robust. Evaluation of the COSMO model is severely lacking due to the absence of perfect boundary experiments. As such it is impossible to properly validate the RCM (a task already made difficult due to the scarcity of observation, as the authors acknowledge), constrain the future changes and assess the added value of the RCM vis-à-vis the driving ESMs. The authors argue that validating historical simulations against observations is appropriate but need to provide a more convincing argument. There are substantial recent trends in precipitation over the Barents region and Scandinavia, which are due at least partly to internal variability, that the historical simulations cannot be expected to reproduce.

The study is also limited by the fact that it presents canonical results that are better suited to an ensemble approach. On the topic of future changes in precipitation, for example, the range of changes from a rather limited ensemble (10 members, maybe only 5 of which are independent) of high resolution downscalings from CORDEX is -2% - +33% in winter and -12% - +23% in summer over western Norway. While the COSMO simulations presented here certainly fall within these bounds they do not add any new information. With respect to sea-ice the present day bias in the EC-Earth simulation renders the changes indicated by the future simulation highly suspect as they are so clearly linked to this field. This reviewer also struggled to find the added value described in the last sentence of the abstract. NorESM is clearly an outlier but since it is not downscaled with COSMO so it cannot exhibit any added value with respect to this driving model. Once it is removed the RCMs and ESMs largely agree

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on cloud cover decline in both sign and magnitude. In the text the authors themselves state that COSMO exhibits a similar reduction in cloud cover as the driving MPI-ESM. So where has the added value come from exactly? Koenigk et al. (2015) showed that there was little added value obtained by the regional models for canonical variables and processes due to the strong dependence on the large-scale features of the driving ESMs. This study does little to change that.

Single model experiments suffer from a lack of robustness. Their best application is to test model performance and the simulation of relevant processes. Might it not be more useful then, given the limitations of a single model experiment design, to investigate processes where the RCM might be expected to show improvement, such as surface fluxes and how they relate to the changes in cloudiness for example? Or assess cold air outbreaks, confirm that COSMO can reproduce these events and then say something about future changes. Or even better actually explore the dependencies between variables as described in the introduction?

Despite its shortcomings this work has potential. However, the authors must make an effort to provide something more than simple an addition to the Koenigk et al. (2015) ensemble. As it stands this is the only contribution made, as the added value claimed is simply not supported by the results. Major revisions are needed with new and perhaps a restructured analyses. While I would like to see an evaluation simulation I do not believe it is necessary if the authors can expand the scope of their evaluation (CRU is too limited and questionable in this region). Given the scope of the revisions required I am supplying only major comments at this time. Minor comments will follow once a fully revised manuscript is submitted.

Major Comments: 1. In the introduction the authors propose a number of potentially fruitful avenues of investigation but then fail to follow up on them. The possible exception might be the cloud cover issues but this proved not to be very informative (see comment X). The paper begins by stating that the dependencies between e.g., cloud cover, precipitation type and wind speeds are under-investigated. This is a promising

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line of investigation. However, it is not pursued as the authors then go on to present and discuss the changes in said fields in isolation. Some deeper investigation (such as covariation of winds and convective precipitation, temperature and pressure, temperature and precipitation, etc.) would greatly improve the quality of the manuscript. This is also where analysis of a single model set of experiments is most useful.

2. The introduction goes on to mention placing the current study within the scope of empirical statistical approaches such as those employed by Benestad et al. (2016). This also was not explored further. Setting the COSMO simulations within the scope of the ESD work could provide an interesting space to explore the strengths and weaknesses of both approaches. Are there things we learn from the COSMO experiments that we do not learn from the Benestad et al. (2016) downscalings? Obviously the benefit of the ESD approach is that it is statistically robust. But is it physically plausible?

3. On lines 52-54 the mini-ensemble from Koenig et al. (2015) is introduced. If the authors intend to “sell” this study as an additional ensemble member they need to be clear on this issue. The additional ensemble members need to be introduced and the reasons for, and scope of, their inclusion described much earlier in the text (Introduction and Methods sections). It is quite confusing when in the discussion of results to suddenly shift from the single model perspective to the ensemble perspective. Since one of the stated aims is to increase the ensemble size of available CMIP5 downscalings, I would suggest the entire analysis (including Pr, T and sea-ice) be performed on the existing ensemble (Koenig et al. (2015) + COSMO). In this way the analysis will at least be consistent throughout although in practice the simulations likely have quite different skillful scales as the Arctic-CORDEX runs are at .44 and the COSMO runs at .22 resolutions. Some discussion on the appropriateness of the comparison is required.

4. As stated previously, the lack of robustness is an issue. That the COSMO simulations are similar to other simulations is not terribly illuminating. Since robustness cannot be fully evaluated here I suggest addressing the following questions: Are the

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changes presented outside the confidence intervals of the historical simulation? Are they statistically significant (preferably with a non-parametric Monte Carlo-type approach)?

5. The lack of realism in the present EC-Earth sea-ice renders the changes described highly suspect. It is also unclear why only the rcp4.5 simulation is included when rcp2.6 and rcp8.5 are performed for MPI-ESM. The entire experiment design has the feeling of incompleteness. It comes across as though this was an exercise in downscaling rather than to address specific scientific questions. As a result it is difficult to discern what it obtained through the inclusion of the EC-Earth. The authors claim on L261-264 that the EC-Earth runs provide important insights but decline to provide any details as to what these are. Something much more specific such as the demonstrated effect of the sea-ice bias on the future changes is needed else just take out the EC-Earth simulation.

6. The evaluation of the COSMO simulations over the historical period is also puzzling. Why is CRU, a land-based, interpolated, gridded dataset, used? Given that the domain of interest is mostly over water this decision is baffling. The precipitation from interpolated datasets with sparse observations is highly suspect. The temperature might be more reliable but again will suffer due to station sparseness. Inclusion of the MERRA reanalysis is welcomed but the way it is included is not terribly informative as it is compared against CRU with CRU held up as ground truth. I would recommend comparing the COSMO simulations directly against more reanalysis products (ERA-Interim, MERRA, Arctic System Reanalysis (<http://polarmet.osu.edu/ASR/>)) and provide considerably more detail and explanation as to why CRU is in any way a reliable ground truth for the Barents region. My argument would be that none of these products are sufficient in and of themselves but that taken collectively one can cobble together a reasonable picture of model performance. As mentioned previously, just looking at modeled temperature and precipitation biases is not so informative and has been done before. How about looking at processes that COSMO, or any model, has to get right in order to

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produce a plausible reproduction of climate over the Barents region?

7. The one aspect where the authors claim to demonstrate added value of the COSMO simulations is cloud cover. However this proves not to be the case. COSMO simply reproduces the cloud cover trend shown by the driving MPI-ESM (Figure 10). If the argument instead is that collectively (i.e., the Koenigk et al. ensemble + COSMO) these experiments exhibit added value then this should be demonstrated quantitatively. Just claiming they add value because they agree is not especially convincing. Especially when they largely reproduce the ESM trends with one outstanding exception (NorESM).

8. The partitioning of clouds and convective/non-convective precipitation is potentially interesting and again would benefit greatly from a more detailed analysis focused on the relevant processes (e.g., cold air outbreaks, winds, fluxes). Given the conclusion that the increase in convective clouds goes along with a decrease in total and low level cloud the exclusion of the plot for COSMO showing changes in the lower levels is unwarranted (L215-217).

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