Authors' comments on anonymous Referee #2

We also would like to thank the anonymous Referee #2 for the review and the very useful input to a possible revision of our manuscript. Please find below a point-to-point reply on the issues raised by Referee#2.

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.

In the revised manuscript, we will put more emphasize on interactions between single variables and include more spatial fields (e.g. winds at different height levels, cold air outbreaks, pressure and fluxes). Although the robustness has been partly addressed by the comparison to the Koenigk et al. ensemble and the DMI-HIRHAM run, a more critical evaluation of the model output will be included. This also includes a more detailed evaluation based on an ERAInterim driven COSMO run.

Furthermore, most of the presented results do not seem to provide much novelty compared to the work cited in the manuscript (e.g., Koenigk 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?

Further analysis of the causes will be included in the revised manuscript. Preliminary results show that the maximum of ca. +20K changes in winter 2m temperature are due to ca. +5K SST increase, +20K surface temperature increase due to replacement of ice surfaces with open ocean and -5K due to changes from an inversion over ice to an unstable lower atmosphere over open water.

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).

Changes of pressure and wind fields (incl. vectors) will be included in the revised manuscript.

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. 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).

To assess climate variability, past and future sea-ice variability and cold air outbreak frequencies will be considered in the revised manuscript.

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 mansucript. 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.

This is a very valid point. We will restructure the manuscript as well as the introduction with more focus on the high variability in sea-ice cover, and on the projected changes not only in the mean signal but also its variability.

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.

The fact that the sea-ice retreat is a model forcing and not a model outcome was not the reason we did not mention the sea ice feedback. We simply didn’t focus on the single processes involved but followed more an impact oriented approach. However, we now did some more detailed analyses and will include more information on the relevant processes and their impact.

Line 12: Typo, please write “increase”

Will be changed.

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

More information on the relevant processes like sea-ice feedback will be included here and in other parts of the manuscript.

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.

More information on the relevant processes like sea-ice feedback will be included here and in other parts of the manuscript.

Line 56: Please write Arctic with a capital letter.

Will be changed.

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 appears to be incomplete.

The idea was to show the influence of the GCM selection mainly. However, we will skip EC-EARTH in the revised manuscript.

Line 86: Wrong placement of the bracket

Will be changed.

Line 95: Please write “simulating” instead of “simulation”

Will be changed.

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.

The reason for this is not clear, but the low resolution forcing should actually be corrected by the RCM. This may thus be a systematic COSMO bias or problems with the gridded observational data in this areas.

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.

We agree that an evaluation based on CRU only is not appropriate in the region. This was also the argument to introduce MERRA: It was meant as an alternative "truth" in the region as CRU can not be considered the ground truth. However, we can see now that the limitation of MERRA to land points and the comparison of CRU to MERRA is more puzzling than clarifying and this will be changed in the revised manuscript.

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.

More information on the single processes and their impact will be included. The strongest warming occur during winter time due to the largest sea-ice retreat in winter.

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.

To answer this question, projected changes in sea-ice variability and responces will be included in the revised manuscript.

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 the importance of the Barents Sea beyond regional scales.

Changes of wind fields (incl. vectors) at different levels will be included in the revised manuscript.