Response to the editor comments

Thank you very much for helpful and constructive comments. Please see below for a point-by-point response on your comments (with our replies marked with blue colour and indentation).

Both reviewers pointed out potentials of this study but also provide constructive comments which I believe are valuable for authors to improve the manuscript. In addition to comments from two reviewers, I have a few comments for authors to consider during the revision:

I got impression authors have done a lot work in this study and presentation of results were comprehensive. However, the reasons of "why" seems a bit weak. The authors claims the biggest improvement was the surface temperature calculation: "The strongest improvement was observed for winter months over the Central Arctic, and the Greenland and Barents seas where a 4.91°C median ice surface temperature error of ERA5 is reduced to 1.88°C in CARRA on average." In winter months, the stable boundary layer (SBL) may dominate the surface energy balance. CARRA seems tackle SBL better than ERA5, yet the stable boundary layer was not mentioned in the manuscript at all. I would like to see authors give some discussion on it.

> A similar comment was given by the second referee and we agree that the original manuscript provides only limited discussion on the mechanisms and links between the observed verification results and differences in representing sea ice in CARRA and ERA5. In fact, for ice surface temperature CARRA's implementation relies on the findings of an earlier study (Batrak and Müller, 2019) that assessed the impacts of utilising snow cover in the sea ice parameterisation scheme of HARMONIE-AROME, therefore we find it somewhat excessive to repeat that study once again. For other variables, namely ice thickness and snow depth, ERA5 simply does not represent their evolution and any improvement/degradation found in CARRA is purely because of the additional prognostic formulations included in the regional reanalysis systems. Nevertheless, we agree that our paper can benefit from extending the discussions and we will update the manuscript accordingly.
>
> Concerning your comment about the representation of winter-time SBLs in CARRA compared to ERA5. Indeed, a snow layer with low thermal conductivity allows for more effective radiative cooling of the ice surface and stronger inversion. This should aid in a better representation of SBLs, however in absence of reliable observational data we can not confirm that with certainty. In general, applying a more detailed sea ice scheme impacts many atmospheric variables from two metre temperature and turbulent fluxes directly over sea ice to cloud cover and precipitation tens and hundreds kilometres away from the ice edge during cold air outbreaks. Assessing all the impacts induced by the ice cover in one study is simply impossible. Therefore, we decided to focus only on the sea ice variables and leave investigations of atmospheric parameters to future studies.

Several places in the manuscript, I would like to see some discussion on "why" e.g.

L630 "The sea ice cover in CARRA adequately represents general multiyear trends towards thinner and warmer ice cover, connected to the ongoing climate change in the Arctic. Comparisons against the satellite-based and in situ sea ice observations show generally improved representation of sea ice in CARRA (using ERA5 as a baseline), although this improvement is not universal". Why?

> We are somewhat puzzled by this comment. This sentence simply says that CARRA is not always better than ERA5 and we do not see how we can further extend on this.

"The main difference between the sea ice schemes in ERA5 and CARRA is the presence of an explicitly resolved snow layer, which allows for much lower ice surface temperature in the CARRA system, therefore reducing the warm ice surface temperature bias found in ERA5" see my comment on SBL above. Can we say that the presence of a snow layer can better tackle the SBL?

> Well, indeed, having a snow-covered ice surface results in strong radiative cooling in winter-time clear-sky conditions thus allowing for much stronger inversion compared to snow-free ice surface. However, it is difficult to draw conclusions on how much SBLs are improved in CARRA compared to ERA5 without any reference data. Therefore, a dedicated study would be needed to thoroughly investigate the representation of the atmospheric boundary layer in CARRA. We will update the manuscript to mention potential positive impacts of lower ice surface temperature on representation of SBL in CARRA.

" However, for the area covering Baffin Bay and the Davis Strait the verification scores suggest that a warm winter-time bias of ERA5 is replaced with a cold bias in CARRA." Why?

> We believe that for Baffin Bay and the Davis Strait snow depth and ice thickness in CARRA are overestimated which leads to a cold bias in the ice surface temperature. We will update the manuscript to explicitly state that.

"No improvement over ERA5 was found in the ice surface albedo with spring-time errors in CARRA being up to 8% higher on average than those in ERA5 when computed against the CLARA-A2 satellite retrieval product." Why?

> This result is a simple consequence of different albedo parameterisation schemes applied in CARRA and ERA5. We will update the manuscript to note that.

L95: "Surface albedo of the sea-ice covered grid cells in CARRA is computed by applying simple parameterisation schemes. For snow-free ice cover, a temperature-dependent broadband albedo scheme is applied (defined as HIRHAM in Liu et al., 2007), and when ice is covered by snow an adapted version of the broadband snow albedo scheme by Douville et al. (1995) is used. When computing albedo of cold dry snow covering sea ice in the CARRA system, the albedo scheme of Douville et al. (1995) is modified to increase the value of the lowest possible albedo in the dry albedo degradation term from the original 0.5 to 0.75. Sea ice albedo schemes applied in CARRA do not distinguish between direct and diffuse components of surface albedo. The HARMONIE-AROME NWP system does not produce grid-cell averaged albedo as an output variable, therefore in the CARRA product the surface albedo field is computed from the hourly accumulated downwelling and upwelling shortwave radiation fluxes and available only from the model integration output."

Are you saying for each CARRA grid, in ice covered area, the surface albedo was calculated by simple parameterization schemes. For ice free area, the surface albedo was calculated by the ratio of upwelling /downwelling shortwave radiative flux, right? Then what is the final product of surface albedo for each CARRA grid cell? Is this (albedo in CARRA cell) better to be called "CARRA surface albedo field"? Can this ice surface albedo and ice-free surface albedo played any role on no improvement of CARRA surface albedo over ERA5 product?

> No, we meant something different there. Surface albedo in the HARMONIE-AROME NWP system is always computed by parameterisation schemes for all surface types found within a grid cell, be it ice-covered or ice-free sea, or land. Then, a grid-cell average albedo is also computed and reported to the atmospheric components of the model. However, even though HARMONIE-AROME technically has a grid-cell average albedo as one of its internal parameters, the NWP system does not write it to the output files. So, to have this field available in the CARRA product it had to be reconstructed from the downwelling and upwelling shortwave radiation output fields produced by HARMONIE-AROME. This is a purely post-processing procedure and it has no effect on the performance of the model itself. We will update the text to make it more clear.

References

Batrak, Y. and Müller, M.: On the warm bias in atmospheric reanalyses induced by the missing snow over Arctic sea-ice, Nature Communications, 10, 4170, https://doi.org/10.1038/s41467-019-11975-3, 2019.