Dear Reviewer,

thank you very much for your time and suggestions for improving our manuscript. Please see our responses (*in italics*) to your comments (**in bold**) below:

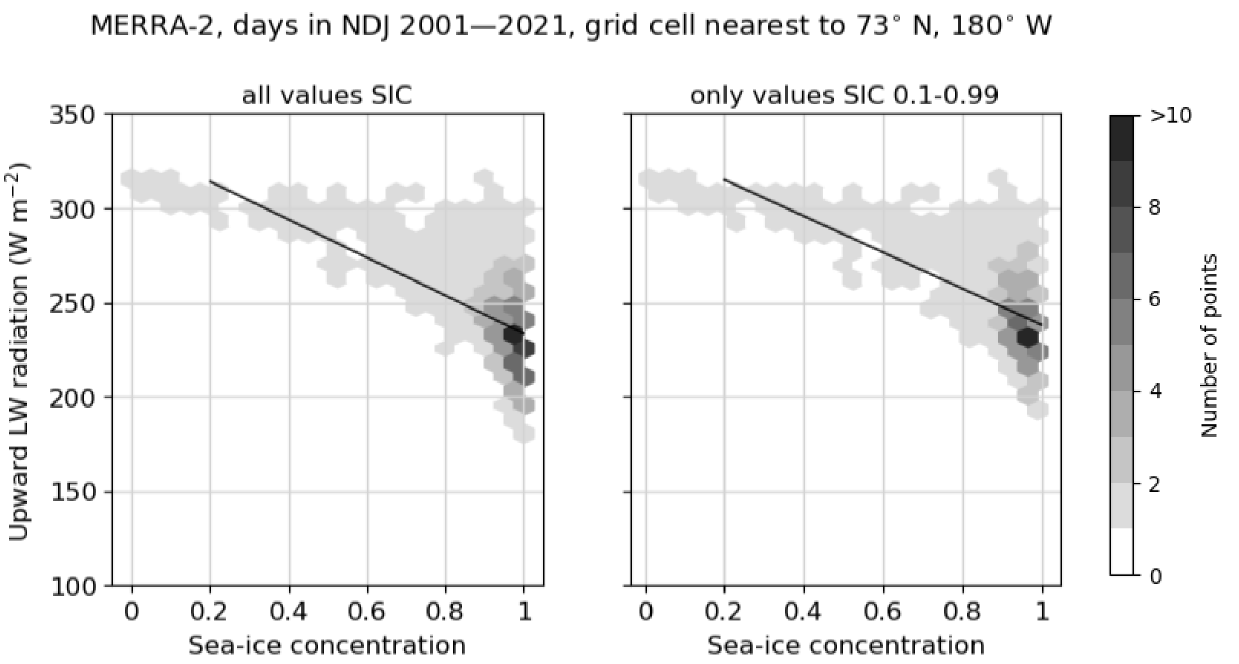
Major Comment:

1) The variance of upward LW radiation to sea ice is greatest at the 100% sic level (Fig. 2). This, however, is to be expected because this is a limiting value for sea ice, as long as it is cold enough for sea ice to remain at 100% the conditions can vary greatly, so this value should not be included in a sensitivity analysis as it no longer reflects a sensitivity to sea ice. If included in the results, it will steepen the slope artificially. I recommend the authors redo the analysis including only sic values from 1-99% and test whether this changes any of the results presented in the manuscript.

We agree that this would be the case, if SIC was only controlled by air temperature. However, SIC also depends on the wind, which can result in ice drift convergence (increase of SIC) or divergence (decrease of SIC). Hence, the ice surface temperature, controlling upward LW radiation, does not necessarily differ much between conditions of 1 and almost 1 SIC.

In Figure 2, Point 2, there are 4-5 hexagonal bins between SIC 0.95-1 and the range of ULW seems to be rather similar at least among the rightmost 2-3 values of SIC (~0.98-1).

From Figure 2, Point 1, we used MERRA-2 data from NDJ 2001-2021 as an example, and compared the slopes of the regression lines in the original case (all values of SIC) and only days with SIC 0.1-0.99. In the Figure below, we show, that the slopes of the regression lines differ only very little between the two cases.



We note that for the comparison depicted above, we utilized ordinary-least-square regression analysis to obtain the slope and intercept of the regression line, because the ODR model did not converge in Point 1 when using SIC 0.01-0.99 data.

Because of the different method used, the slope of the regression line in original data (left panel in the figure above) is somewhat different (less steep) than in our original manuscript (fifth panel in Figure 2, Point 1). However, as we used the same method for both cases in the figure above, it does not affect the results of the comparison.

2) Furthermore, the sensitivity results (e.g. Fig. 2) reflect a sensitivity that is larger at high values of sic than low values. This makes sense as low values of sic tend to have thinner sea ice and a greater percentage of open ocean. For smaller values of sic, the open ocean serves as a moderating influence that lowers the variance of the sea ice temperature and upward LW flux due to its high thermal inertia. For thinner sea ice, the ocean below also moderates the variance. The greater the sea ice thickness (SIT) the weaker the influence the ocean below has on the surface temperature and upward LW flux. Therefore, I suggest the authors attempt to include an analysis of SIT for the sensitivity analysis, which admittedly might be cumbersome as SIT observations are still lacking but a reanalysis such as PIOMAS might be helpful. This would provide a more insightful analysis of the sensitivity of upward LW fluxes to sea ice, while providing for a more complete physical explanation of the results.

We added a new figure to subsection 3.1 of the revised manuscript to address this comment. We used February data from the SHEBA campaign and carried out calculations on the effects of sea ice thickness and snow depth on the conductive heat flux from the ice base to snow surface, which further affects the surface temperature and upward LW flux. The results are presented in the new figure and interpreted in the revised text.

Additional Comments:

3) The results section 3, discussion section 4 and conclusions section 5 have some repetitive information. I suggest a revision to streamline the paper.

We reviewed sections 3, 4, and 5 for repetitive information and reduced their amount whenever possible.

4) Lines 88-89: Sentence is unclear. Improve the clarity of the definitions for R1 and R2, respectively.

We rephrased the part of the text as: '*...T stands for number of days in one sample (in our case days in seasons in the periods of 1980-2000 or 2001-2021), R_1 for correlation coefficient of lag 1 auto-correlation of SIC, and R_2 for correlation coefficient of lag 1 auto-correlation of surface radiative flux (ULW or USW).*'

5) Lines 100-101: Why is the open ocean 'usually' and not always warmer than the sea ice surface? If open ocean is colder than sea ice, wouldn't we expect the ocean to freeze into sea ice?

Open ocean in the Arctic can be colder than the sea ice during the melting season by the following mechanism: Water under the sea ice is usually close to the sea-water freezing point -1.8 °C (unless e.g. warm upwelling of ocean water present). Then, when a lead opens due to divergent ice motion, this temperature becomes the ocean surface temperature, while the sea-ice surface next to the lead may be close to the melting temperature of snow and ice (0 °C).

6) Lines 135-136: Did the authors mean "cannot be applied where no SIC is present"? I suggest a revision.

We rephrased the part of the text as: '*This assumption cannot be applied in the warm season (May-October) in the majority of adjacent seas outside the Central Arctic, because the surface temperature of the ocean is likely often higher than -1.8 °C, therefore we focused on the cold season (November-April) in these analyses. We are also aware, that in the Greenland and Barents seas, even cold-season ocean temperature may be warmer than -1.8 °C due to the North Atlantic Current carrying warm Atlantic water.*'

7) Line 206: Suggest change to "we also noted".

We changed the text accordingly.

8) Line 224: Suggest change to "plays an undeniable role" and from "also decadal …" to "decadal changes in DSW must *also* be…".

We changed the text accordingly.