

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:

1) Line 72: What is meant by “All surface radiative fluxes were defined as positive.”? There are downward and upward surfaces flux. Does this mean both downward and upward surface radiative fluxes are defined as positive? If yes, I suggest making this explicit, as the norm is to define either upward or downward as positive and the other as negative.

Yes, it is meant that both downward and upward radiative surface fluxes are positive (they also come in this shape from all the reanalyses). We adjusted the text to: ‘All surface radiative fluxes (both upward and downward) were defined as positive.’ Line 72

2) Line 149: I suggest deleting ‘also’.

We adjusted the text accordingly. Line 150

3) Lines 217-218: I suggest changing “also reduction of DSW...” to “the reduction of DSW also played a role.”

We adjusted the text accordingly. Line 218–219

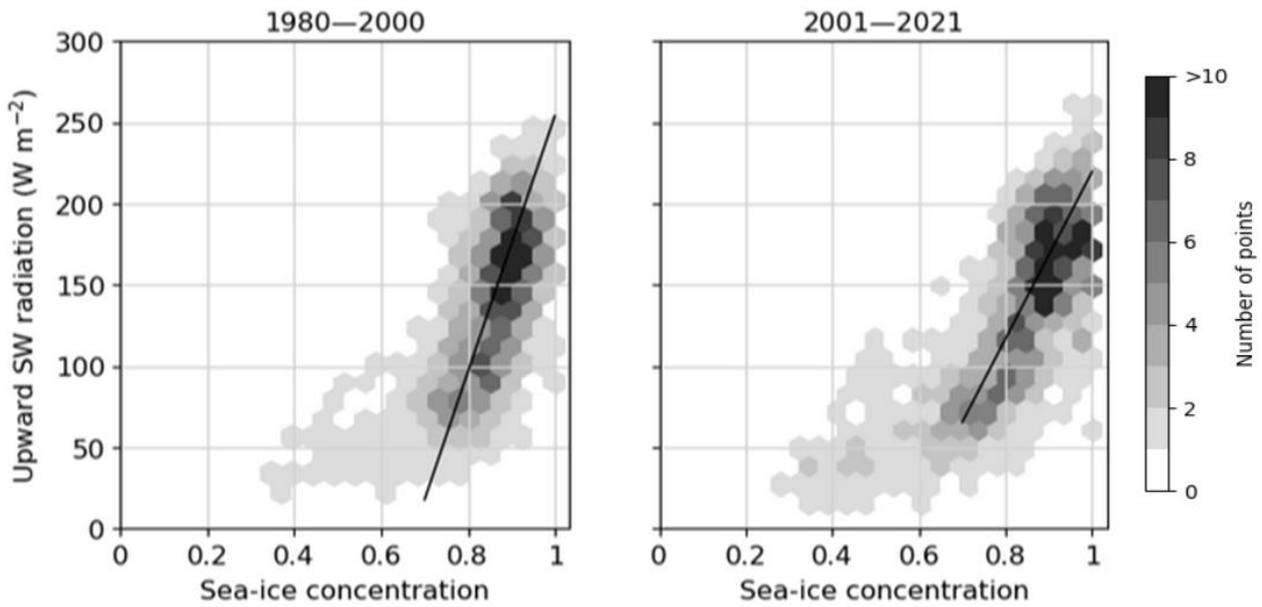
4) In Section 4.1 the authors discuss how the increase in SIC in the central Arctic leads to increased sensitivity in ULW. However, why does the increase in SIC in the central Arctic not lead to strengthening of the sensitivity in USW? This should be discussed in section 4.2 to contrast with section 4.1.

We added a specification to Section 4.1 that we are addressing the increase in decadal sensitivity of ULW to SIC due to decadal increase of SIC in November–December–January (that we saw in Fig. 1).

If we compare the decadal change in ULW sensitivity to SIC in May–June–July (Fig. S3), it shows very similar patterns to the decadal change in USW sensitivity to SIC in May–June–July (Fig. 5).

Additionally, to show the decadal change of the daily SIC and USW in the Central Arctic/Greenland Sea in this season, we present representative grid cells from ERA5 and MERRA-2 (similar to Point 2 in Fig. 2) in Fig. X below. In both reanalyses and both study periods, the daily SIC was mostly around 0.9, however, we noted an increase in SIC below 0.8 in the second study period leading to weaker effect of SIC on USW in May–June–July in 2001–2021 compared to 1980–2000. However, we do not consider the above results interesting enough to be discussed in Section 4.2. We understand the Reviewer’s point of view. Had the increased sensitivity of ULW to SIC and the decreased sensitivity of USW to SIC occurred in the same season, it would require discussion in Section 4.2. Now when it is clarified that the increased sensitivity of ULW to SIC occurred in November–December–January (when there is very little to none solar radiation), the discussion is not relevant.

ERA5, days in May—June—July, grid cell nearest to 81° N, 0° W



MERRA-2, days in May—June—July, grid cell nearest to 81° N, 0° W

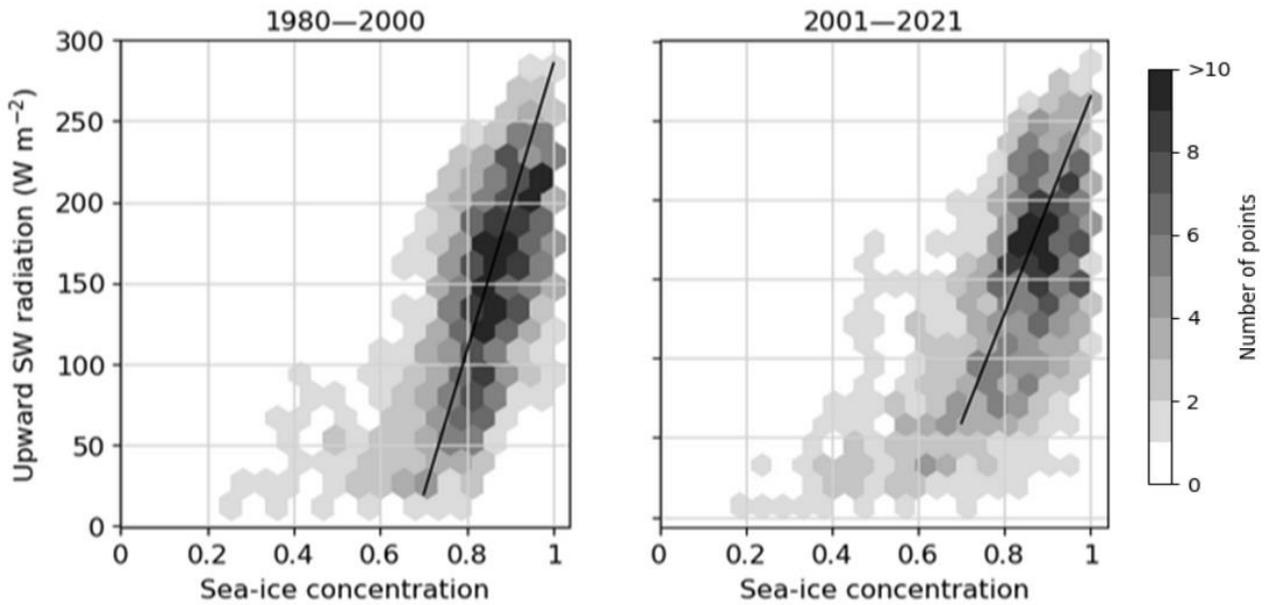


Figure X. Daily sea-ice concentration (SIC) and upward shortwave radiative flux (USW) in selected grid cell (nearest to 81° N, 0° W). ERA5 and MERRA-2 data, days in May–June–July (1932 days). Black solid lines depict (a part of) the regression lines and illustrate their slope.

5) Lines 298-299: Why doesn't the large spread in clouds have an impactful effect on the solar radiation? Hypothesis or additional insight should be added.

Text in question:

'In our study, we only calculated decadal seasonal differences in mean CCC, but even by using this simple calculation and just one cloud parameter, we noted a large spread in values between the reanalyses (row i in Figs. 7, S11, S13). However, in all seasons, the magnitude of changes in USW explained by changes in DSW (ΔUSW_{DSW}) was very similar among reanalyses (panels i-l in Figs. 6, S10, S12), so from the point of view of solar radiation, clouds did not seem to be a key factor for the inter-reanalysis differences in decadal seasonal changes.'

Thank you for pointing out this part of the text. We noted the confusing phrasing.

Large spread among reanalyses in decadal changes in cloud condensate content (CCC, for May–June–July shown in Fig. 7 row i) is reflected in the spread in decadal changes in downward solar radiation at the surface (DSW, Fig. 6a–d). It seems that we meant to point out that the effect of CCC on USW (via ΔUSW_{DSW} , Fig. 6i–l), however, does not show a very large spread. After reconsidering, we do not see any special reason why this should be mentioned and decided to not include the text on Lines 297–299 in the revised manuscript.

The key message regarding the effect of CCC (via ΔUSW_{DSW}) on USW should be that, according to our results, it is smaller than the effect of decadal changes in surface albedo (ΔUSW_b) on USW as shown in Fig. 6i–l and m–p. Such message was already mentioned in the Section 3.3 of the Results and we do not consider it necessary to mention it again in the Discussion.