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:

This study aimed to compare the affects of sea ice concentration on the surface radiative fluxes from 4 reanalysis and this sort of a companion paper to their previous study looking at turbulent fluxes from these reanalysis. They found that the upward longwave radiation was most sensitive to SIC in the winter, and upward shortwave radiation was most sensitive to SIC during the summer. They found that the relationship between SIC and upward longwave radiation has decreased from the first 20 years compared to the last 20 years in the 1980-2021 record and attributed this to the thinning ice and warming surface temperatures.

I found this study to be very insightful, and the methods and results were clearly explained and easy to follow. The figures were also clear and easy to understand. I feel like these types of analysis are essential to the scientific community to better understand the uncertainties and limitations posed by these global reanalysis products when studying the Polar regions like the Arctic Ocean. I feel like this paper should be published after my minor comments are addressed.

9) Sentence beginning on line 30: Needs a citation

We added a citation to Persson (2012).

10) Line 70: why do you not also include surface downward longwave radiation? I am sure you would want to do this to account for the full radiative fluxes.

Downward longwave (as well as shortwave) radiation depends on cloud formation and properties, and emisivity of the atmosphere more than on SIC (which is in the center of attention of our manuscript). SIC can and does contribute to some degree to a cloud formation by a flux of moisture to the atmosphere in areas of recent sea-ice decline, and vice versa to lower cloud formation in areas with increased SIC (indicated in our Figure 6 and Lines 250–253 in Discussion of the original manuscript). However, as shown e.g. in Nygård et al., 2020 (their Figure 6), the moisture from evaporation in the areas with sea ice present contributes only very little to cloud water and total column water vapour compared to horizontal moisture transport, which dominates the regional moistening pattern in the Arctic.

For these reasons and as the manuscript is already rather long, we decided to exclude analyses regarding DLW radiation and include the DSW radiation only as a part of the explanation of changes in USW radiation between our two study periods.

11) Table 1: can you elaborate more on how the albedo is parameterized for NCEP/CFSR
Is it coupled to the ocean or atmosphere or both? Please elaborate. Any information on the sea ice model that they use? It might be more specific if you actually spelled out

what the albedos were for the other reanalysis rather than just stating the citations, and include this information in the table.

The albedo in CFSR and CFSv2 comes from Sea Ice Simulator 1 (SIS-1) by Geophysical Fluid Dynamics Laboratory (GFDL). Sea ice plays the role of a general interface between the atmosphere and the ocean in this model.

CFSR and CFSv2 use both atmospheric and ocean model (GFS and MOM4).

We added this information to Table 1.

To concisely describe the seasonal cycle or parameterization of albedo in a table is a rather difficult task. Hence, we decided to use an example and calculate the average albedo for the grid cell nearest to the North Pole for month of June in the years 1990 and 2010 to offer a comparison of this variable between the reanalyses. We present the results in the revised manuscript.

12) Figure 3, I think that makes sense because we can probably assume that the ocean surface temperature in the marginal ice zones is likely above -1.8C and the ice is less compact so the ice temperature that was calculated was probably off some.

This comment seems unfinished, but if it was meant as a possitive comment, we thank the reviewer.

13) Line 194: Yes this makes sense that the CCC would be so different between all of the reanalyses because they all have differing cloud schemes (one moment, two moment, etc). It might be nice to reference these differences or add their parameterizations to the table 1.

Clouds in our study are only touched rather briefly for the purpose of explaining part of decadal changes in upward SW radiation by decadal changes in downward SW radiation, which depend on cloud formation and properties.

Cloud parameterization in reanalyses is a very komplex subject and as mentioned in the response to comment 10, our main focus for this manuscript ended up being mainly surface upward radiative fluxes, therefore we do not believe that going further into cloud parameterizations fits the predominant topic of the study.

We do consider the subject of cloud parameterization in reanalyses crucial for understanding their differences in the representation of the Arctic climate system (as indicated in the subsection 4.3 of Discussion in the original manuscript).

14) Figure 6: Any idea why MERRA2 has such a large change in CCC, especially in the North Atlantic, compared to other reanalysis?

One reason for larger cloud condensate content between 1980-2000 and 2001-2021 in MERRA-2 in the North Atlantic south and southwest of Iceland (which we found to some extent also in February-March-April and August-September-October; Figures S10, S12) may be that MERRA-2 assimilates aerosol observations, while the other reanalyses only apply climatological aerosol concentrations.

Other reasons are probably related to different parameterization of cloud microphysics between MERRA-2 and other reanalyses.

As our study area of focus was limited to the ice-covered seas, we generally did not assess areas more to the south even though they are depicted in some of our figures.

General Comment:

15) Since the SIC is so important for the energy budget of the lower atmosphere and ocean in the Arctic, it might be good to compare the SIC with passive microwave SIC observations to determine which SIC is most realistic? Then what conclusions can be made towards your other results which are so highly SIC dependent/driven?

Comparison of reanalyses to observations is undoubtedly necessary in order to assess their accuracy. However, SIC in all four reanalyses in our study is based on information from satellite passive microwave sensors.

There is no consensus on which passive-microwave-based SIC data set is most realistic as different passive microwave sensors are utilized and even for thr same set of raw data, SIC depends on the processing algorithm applied (such as NASA-Team, Bootstrap, and ARTIST algorithms). Hence, we cannot answer the second question.

In principle, the suggested comparison could be based on Synthetic Aperture Radar (SAR) data, which has a very high resolution and identifies each pixel as either sea ice or open water, allowing calculation of regional sea-ice concentrations. However, such comparison would require a major work (of the order of several months) and also SAR data have challenges in the pixel identification.

References:

Nygård, T., Naaka, T., and Vihma, T.: Horizontal Moisture Transport Dominates the Regional Moistening Patterns in the Arctic, J. Climate, 33, 6793–6807, https://doi.org/10.1175/JCLI-D-19-0891.1, 2020.

Persson, P. O. G.: Onset and end of the summer melt season over sea ice: Thermal structure and surface energy perspective from SHEBA, Clim. Dyn., 39, 1349–1371, 2012.