

Response to Reviewers

Note: In this response, the reviewers comments have been left untouched, and we added our responses in bold.

Reviewer 2:

This paper has been made corresponding revisions based on the previous round of review. I have no further substantive comments here, only some specific suggestions:

We thank the reviewer for the helpful suggestions. We respond to them below.

1)The results of this study are all based on relatively short (40 years) satellite remote sensing data. Although the author believes that the low-frequency signal recognition method used can reduce the impact of short time series, I consider it is still necessary to further discuss some results that may be affected by insufficient time series, and its possible degree or extent or intensity.

We thank the reviewer for this suggestion. We already discuss the limitations of using this method on a short time series in Lines 320–345 and Lines 366–375. We have furthermore added the following sentence to Line 370-371: 'The results may therefore overestimate the forced contribution to sea ice changes, and underestimate the internal contribution.'

2)Figure 1 “sea ice edge based on 50% sea ice concentration” : Why do you use a 50% threshold instead of the usual 15%?

Since this figure is showing seasonal average sea ice cover, we think it is more useful to use a 50% threshold to not skew the cover to one month. In the figure caption, we have changed 'sea-ice edge' to 'sea-ice cover' to more accurately describe what we are showing.

3)Figure 2b: Corresponding to LFP2 in summer, Beaufort Sea and East Siberian Sea have an obvious dipole structure, which needs further explanation. It may be related to the leading (some months) Beaufort High, which can adjust the zonal advection of sea ice in the southern Beaufort Sea Chukchi Seas.

Yes, there is a dipole structure, and this ties into the discussion on the effect of the previous winter AO on summer sea ice through ice motion and subsequent thinning. We have added a sentence in Line 196-197, specifically mentioning that this also leads to a dipole between the Beaufort and East Siberian Seas.

4)Line 199 “The third LFP in summer is connected to a dipole in geopotential height over western Greenland and the Canadian Archipelago on the one side, and the Nordic Seas on the other side”: Can this be considered as an Arctic dipole, the so-called DA.

Yes, it is reminiscent of that. We have added a sentence, and some references in Lines 202–203.

5)Line 369 “However, summer sea ice is projected to disappear in the coming decades (Notz and SIMIP community, 2020; Årthun et al., 2020; Bonan et al., 2021b), setting a limit to the length of the observational record of summer sea ice”: The threshold for defining the Arctic ice free is 1 million square kilometers by the climate models, and it can be considered as ice free with a temporary appearance, not for the whole sum-

mer. Thus, interannual changes in summer sea ice extent will still exist. In the time scale of the next 50-70 years, there should be a pseudo-proposition of “no statistical change of summer sea ice because of ice free”. This proposition may only be established in some very outer sea areas, such as the Barents Sea and Baffin Bay.

We have removed the last part of the sentence and replaced 'summer' with 'September' in Line 372. We now use the same argument than for winter, that patterns may change over time.