

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. In my opinion, there is an overstatement of the novelty of this work and conclusions. The introduction and discussion would need to include more engagement with the literature of work on this topic (see the suggestions in the minor comments section).

We have improved the manuscript in this respect. See our comments to the specific suggestions below.

2. Methods, Lines 68-73: please can you give more details of the skill and bias assessments of NCEP CFSR and then CFSv2 compared to other reanalyses beyond just the in-line references. I would question the use of CFSR v1 over other widely used reanalysis datasets. Studies have shown larger biases in NCEP CFSR Arctic atmospheric temperature aloft (Jakobson et al. 2012) and lower tropospheric Arctic humidity structure (Serreze et al. 2012). Lindsay et al. (2014) support the performance of CFSR over the Arctic, but their work is not discussed in this paper. Graham et al. (2019) found a moist bias in CFSv2 throughout the troposphere and Kong et al. (2022) found that CFSR has a moist bias over the Pacific sector and that moisture is more sensitive to sea ice concentration changes. Adding more details from the literature will also enable a brief comment of how the results should be interpreted and could differ if different reanalyses were used.

We agree that CFSR and CFSv2 are not free of biases, but we still consider them better than other reanalyses for a study where the representation of sea ice concentration and thickness as well near-surface air temperature and specific humidity are critically important.

In Jakobson et al. (2012), CFSR indeed showed a cold bias at heights of 300 to 900 m but in the lowermost 200 m it overperformed ERA-Interim, JCDAS, NCEP-DOE and MERRA reanalyses for air temperature, and in the lowermost 500 m it overperformed the above-mentioned reanalyses for specific humidity. Serreze et al. (2012) indeed showed that MERRA had smaller biases than CFSR in the vertical profile of specific humidity. However, Serreze et al. (2012) also detected evidence of unphysical features in the MERRA record of air temperature and specific humidity. Graham et al. (2019) indeed found ERA5 performing better than CFSv2 in the Fram Strait region, and attributed it to a more advanced data assimilation method (4D-VAR).

Kong et al. (2022) identified challenges in both ERA5 and CFSR(+CFSv2) in the Pacific Region, including a larger sensitivity of CFSR(+CFSv2) to SIC. However, these results did not demonstrate worse performance of CFSR(+CFSv2) compared to ERA5. The warm bias in ice surface and near-surface air in ERA5 has received a lot of attention (Batrak and Müller, 2019). As the focus of our study is on the effects of SIC, we prioritized the quality of presentation of sea ice, and therefore selected CFSR and CFSv2.

Using data from several reanalyses would have made the manuscript excessively long, as results were analysed from various altitudes. It was doable in Uhlíková et al. (2024; 2025), which focused on surface fluxes only.

In the revised version of the manuscript we have extended the arguments for our choice to base the study on CFSR and CFSv2, also citing Lindsay et al. (2014).

3. Methods, Lines 73-80: there needs to be a discussion of the differences in the two model versions (CFSR vs. CFSv2) and their changed physics and biases. There needs to be a discussion of how the use of two different model versions might affect the results from 1980-2000 and 2001-2021 which are being directly compared as apples to apples. This further leads me to question why this dataset was chosen compared to others that cover 1980-present with the same model version.

The NCEP/CFSR reanalysis (including both CFSR and CFSv2) was selected because, unlike other reanalyses, it explicitly models both sea-ice thickness and snow depth on top of sea ice. This representation provides a physically more realistic description of surface conditions and leads to a substantially higher sensitivity of surface latent and sensible heat fluxes to SIC (Uhlíková et al., 2024). Consequently, a stronger sensitivity of near-surface air temperature and specific humidity to SIC can also be expected, which is central to the objectives of this study.

As we showed in Uhlíková et al. (2024), the magnitude of this sensitivity remains relatively stable throughout the period 1980–2021, showing temporal consistency comparable to that found in ERA5 reanalysis and MERRA-2 (see e.g. Fig. 4 in Uhlíková et al. (2024) for latent heat flux). In the present paper, data of seasonal means in SIC, temperature, and specific humidity over eight Arctic basins (Figs. 2, S1-S3) reveal no systematic differences between the periods before and after 2011, when the model version and data assimilation system were upgraded. The 2011 model updates primarily affected cloud processes, radiative transfer, and the land surface model (Saha et al., 2014). Changes to the land surface model are not expected to influence our results. While modifications to cloud and radiative transfer parameterizations have potential to affect snow and ice surface temperatures, and thus the temperature contrast between open water and sea ice, our analysis indicates that their impact is minimal in the marine Arctic, as no systematic differences are detected across the pre- and post-2011 periods.

Our approach is consistent with previous studies, such as Sato et al. (2018) and Kong et al. (2022), which treat CFSR and CFSv2 as a single, continuous dataset derived from the same underlying model, without emphasizing relatively minor parameterization changes.

In the revised manuscript we have added a brief description of the changes made in 2011.

4. The paper states causality results of SIC affecting the atmosphere or vice versa (e.g. Lines 179-199; Lines 350-352). Unless I missed a portion of the analysis, causality cannot be ascertained from the comparisons and statistics presented here. OLS regression identifies statistical associations and does not, by itself, imply causality. “Coefficient of determination” (R^2) is the fraction of the variance in the dependent variable that can be explained by the regression model. However, the model x and y can be flipped to generate the same relationship. The R^2 does not mean the model is correct or causal. The language therefore needs to be adjusted throughout the paper of what the results present in terms of variance and relationships, but directional mechanisms are only possible explanations and not results.

We have rewritten the text on these lines (and in abstract) so that we do not explicitly refer to causal effects but summarize the statistical findings and present interpretation on physical processes that may favour these findings.

6. In my opinion, the calculations and comparison of Q2m to T2m could be moved to supplemental as this section breaks up the flow of the discussion and paper. Some currently supplemental figures are referred to in the text and could be moved into the main manuscript instead.

We consider the calculation and depiction of R^2 between T2m, SIC and Q2m, SIC, and Q2m_{SAT} important for the explanation of why R^2 between Q2m, SIC is consistently higher than that of T2m, SIC. It complements Figs. 3 and 4 as it shows the values of SIC, T2m, Q2m in selected grid cells that were utilised to calculate the values of R^2 between these variables.

7. Section 4.1: this section explains why SIC variability changes. However, there is no discussion of why the R^2 changes. What then is related to the variability in the temperature anomalies instead in the later decades? Or when both temperature and SIC variability increase, why does R^2 increase in the later decades? This is somewhat addressed in section 4.2 but not always directly addressing the stated results in section 4.1.

Increased variability in air temperature in the northern Barents Sea in November-December-January may be attributed to two drivers: (1) cyclone tracks having become more meridional in the region, with increased cyclone activity in the northern Barents Sea and decreased one in the southeastern Barents Sea (Wickström et al., 2020) and (2) increased SIC variability in the northern Barents Sea. Cyclone activity (Kruglova and Myslenkov, 2024) and SIC variability (Fig. 2) have also increased in the East Siberian Sea. In these sea areas, which were previously characterized by limited variability of SIC in the cold seasons, increasing variability favours increasing R^2 values.

We have added adjusted text above to the corresponding paragraphs in Section 4.1.

8. In general, I don't feel that the compelling sentence of "surface heat and moisture fluxes become less sensitive to SIC in a warming climate" from the abstract is fully demonstrated or discussed within the body of the manuscript. If this is analyzed and discussed more deeply, it would be a valuable contribution to the literature.

We assessed the decadal changes in sensitivity of surface turbulent radiative fluxes to SIC in our previous paper (Uhlíková et al., 2024), which is in this context referred to in the end of Section 4.2.

We have changed the part of the abstract to ,...even though previous studies showed that surface heat and moisture fluxes become less sensitive to SIC in a warming climate... ‘

9. Overall, I would suggest reorganizing the paper so that the results are paired with the discussion and explanation. Without this restructuring, there is repetition throughout the manuscript, and the reader must move around sections to find and glean the discussion and explanations of the stated results. Furthermore, some results are never properly explained. For example, moving the discussion of temperature inversions and wind to the relevant discussion of the relationships would be very helpful for the reader's understanding and change the feeling that this section was just added on. The conclusions can then be used to summarize the main findings. I can defer to the editor if this is not the suggested format of Cryosphere manuscripts. However, whatever the overall structure of the main sections, I do think some refining could still be done to improve the readability and understandability of the results and their discussion.

We have considered the text again in terms of readability and tried to improve it where applicable, keeping the original main sections.

10. The conclusions and value of this paper would greatly benefit from a discussion of what this paper brings to the current literature and understanding. Are there any takeaways that could inform our understanding of the future of the changing Arctic?

We have added discussion on what this study brings to the current literature and understanding. However, we prefer not to speculate about the future of the changing Arctic in the paper, because we cannot assume that the trends observed between 1980-2000 and 2001-2021 will continue quantitatively similar in the future.

11. Line 19: this is an abrupt sentence to end the abstract. I suggest moving it to lines 3-4 when summarizing the study.

We have moved the sentence accordingly.

12. Line 46: please include some other relevant references here. E.g. Rinke et al. 2006; Parker et al. 2022.

We have added citations to these important references and expanded the discussion surrounding them.

13. Line 47-57: Please see and consider additional references that do examine some of these same relationships and discuss how they inform or supplement your work throughout the manuscript. E.g. Screen et al. 2013; Boisvert and Stroeve 2015; Boisvert et al. 2015; Taylor et al. 2018; Liang et al. 2021; Yu-Fang et al. 2022; Boisvert et al. 2023.

We have added citations to these important and relevant references and expanded the discussion surrounding them.

14. Introduction overall: I find the flow to be quite awkward. The introduction starts with a strong emphasis and a lot of detail, on the relationship between leads and the atmosphere. Given the methods of examining SIC and the atmospheric variables at coarse resolution of 0.5 degrees from the NCEP/CFSR data, there should be a broader and deeper discussion of the interaction of general surface characteristics with the atmosphere (both top-down, bottom-up, and the feedbacks).

This is an important perspective. We maintain that it is essential to give significant attention to the relationship between leads and the atmosphere. Although the spatial resolution of NCEP/CFSR data is 0.5 x 0.5 degrees, SIC is provided for each grid cell, representing the subgrid-scale partitioning between sea ice and open water within that cell. Given the relatively low number of polynyas in the Arctic, most subgrid-scale open water within the inner ice pack can be attributed to leads. In contrast, the marginal ice zone often consists of a heterogeneous mixture of sea ice and open water, where open water areas are not necessarily leads but are more directly connected to the open ocean.

We have expanded the introduction to better address atmosphere-surface interactions and associated feedback mechanisms.

15. Line 75: is the native resolution 0.5 degrees for CFSR?

The most detailed spatial resolution for some variables in CFSR is 0.312 x 0.313 degrees, but some of the variables aloft were only available in 0.5 x 0.5 degrees, so we downloaded and worked with all variables in this spatial resolution.

16. Table 1 and 2, please use a marker or formatting to denote which of the numbers are statistically significant.

We have marked statistically significant results in Tables 1 and 2 (and Tables S1-S6) in bold.

17. Table 1, consider including the standard deviation values so that we can assess the decadal trend against variability.

We have added standard errors of the slopes to Table 1 (and Tables S1, S3, S5) in order for the reader to be able to assess the interannual variability of the seasonal means (excluding trends). This variability is also depicted in Fig. 2 for November-December-January (and S1-S3 for other seasons).

18. Lines 122: please give a short reason why temperature is increasing without large changes in SIC.

We have added a sentence explaining that even a rapid warming in the Central Arctic during November-April does not result in major decrease in SIC, because also in the recent warmer climate, the air temperatures remain well below the melting point for most of the time.

19. Figure 2: consider adding trend lines for the series that have a significant trend over time.

We think that adding the trend lines would make the plots too busy and harder to read considering the already high number of lines in each plot of Fig. 2. The values of decadal trends are listed in Table 1 (and Tables S1, S3, S5).

20. Line 170: please rephrase grammatically.

We have rephrased the sentence as follows ,When examining mean seasonal and decadal values of SIC, temperature, and specific humidity, a substantial amount of information about their true variability is lost. ‘

21. Section 3.2 and throughout: please include confirmations within text of when a relationship (R^2 value) is statistically significant.

All the R^2 values mentioned in the text are statistically significant, referring to Figs. 3, 4, S4, S5, where 'only statistically significant results at the confidence level $p < 0.05$ are shown (insignificant ones are masked in white)'.

22. Line 205: please include discussion of how the large-scale circulation and climate may have a stronger relationship with the atmosphere aloft.

We have added a sentence on the dominant role of large-scale circulation on temperature and specific humidity aloft.

23. Lines 206-238: I think it is still valid to discuss the physical mechanisms of sea ice as a buffer between the ocean and the atmosphere and the possible effects of sea ice loss on latent heat fluxes, evaporation, and humidity. Not only the relationship to T2m.

The text on lines 206-238 included equally much attention to T2 m and Q2m. We have added text on the role of the surface fluxes of sensible and latent heat on T2m and Q2m, respectively.

24. Lines 251-272: please include some brief discussion of what could be causing the changing relationships here.

This discussion takes place in Section 4.1.

25: Figure 7: this figure requires a lot of comparison by eye from a) to b) and c) to d). You could consider summarizing the differences in a table for easier and quicker interpretations of the similarities and differences.

To make the comparison of the values of SIC and temperature between 1980-2000 and 2001-2021 easier, we have plotted the values from parts a, b and c, d together and distinguished them by colour (values from 1980-2000 are black and those from 2001-2021 are blue or red depending on whether R^2 decreased or increased between the study periods, colour-coded with Fig. 6).

26. Line 277: please rephrase (towards close to SIC 1) – this is hard to understand.

We rephrased as 'a decadal increase in SIC (to almost 1)'.

References:

- Kong, B., Liu, N., Fan, L., Lin, L., Yang, L., Chen, H., ... & Xu, Y. (2022): Evaluation of surface meteorology parameters and heat fluxes from CFSR and ERA5 over the Pacific Arctic Region. *Quarterly Journal of the Royal Meteorological Society*, 148(747), 2973-2990.
- Kruglova E. E., Myslenkov S. A. Increased storm activity in the eastern sector of the Russian Arctic. *Arctic: Ecology and Economy*, 2024, vol. 14, no. 4, pp. 522—535. DOI: 10.25283/2223-4594-2025-4-522-535. (In Russian).
- Saha, S., Moorthi, S., Wu, X., Wang, J., Nadiga, S., Tripp, P., Behringer, D., Hou, Y.-T., ya Chuang, H., Iredell, M., Ek, M., Meng, J., Yang, R., Mendez, M. P., van den Dool, H., Zhang, Q., Wang, W., Chen, M., and Becker, E.: The NCEP Climate Forecast System Version 2, *Journal of Climate*, 27, 2185 – 2208, <https://journals.ametsoc.org/view/journals/clim/27/6/jcli-d-12-00823.1.xml>, 2014.
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- Uhlíková, T., Vihma, T., Karpechko, A. Y., and Uotila, P.: Effects of Arctic sea-ice concentration on turbulent surface fluxes in four atmospheric reanalyses, *The Cryosphere*, 18, 957–976, <https://doi.org/10.5194/tc-18-957-2024>, 2024.
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- Wickström, S., Jonassen, M., Vihma, T, and Uotila, P. (2020). Trends in cyclones in the high latitude North Atlantic during 1979-2016. *Q. J. R. Meteorol. Soc.*,146, 762–779, DOI: 10.1002/qj.3707.