October 14, 2026

*The document is color coded in the following way:*
*Referee comments are in* ***blue****.*
*Our answers are in* ***black****.*
Citations from the revised manuscript are in **red**.

# Response to Referee #1 Salar Karam, review round 2

Review, round 2. Evaluation of sea ice variability in three ocean reanalyses and two regional models in the Barents and Kara Seas in 1991-2020.

## General comment

The revised manuscript has been much improved compared to the original version. I think the authors have addressed my main concerns, which were that the original manuscript was too vague in its aim, that the discussion was too descriptive and there was no interpretation of the data, nor any implications for the results. The revised manuscript is more focused in the introduction now with clearer objectives, and the authors also provide much more interpretation in the discussion through discussing the relationship between sea ice and mixed layer temperature, as well as the new analysis with climate indices. Importantly, the authors also provide specific suggestions on the choice of data products depending on the region and the intended application of the user.

I also want to mention that I do not consider the specific additional analyses suggested in my previous review to be requirements in themselves, but rather that they were possible discussion points for a more interpretative analysis. I would say that the revised manuscript has now generally addressed my previous comments.

However, I have some remaining issues that I recommend addressing before publication. They are mostly related to some restructuring of the introduction, removing the remaining references to the storyline concept, and including the motivation for the selection of datasets. I do not think these changes will take long to implement, and therefore recommend minor revisions, after which the manuscript should be suitable for publication.

As a final general comment, I want to mention that the responses to several of the major comments in the response letter are very brief and do not indicate where the corresponding changes have been made, nor exactly how they have been addressed. I understand that the authors have restructured the manuscript, which can make it difficult to point to specific changes. However, given the length of the manuscript and the number of points raised in the original review, the limited detail in many of the responses made the revision more difficult and time consuming for me than necessary. While I do not think it is necessary to provide detailed responses for minor comments, I expect substantially more specific responses to major comments. For any future papers, I would strongly recommend that the authors provide specific responses on which relevant changes have been made, and their exact location in the revised manuscript.

We wish to again thank the reviewer, Salar Karam, for the time and effort he spent reading the manuscript and providing constructive comments. We also thank the reviewer for taking the time to list the minor comments, which have helped us improve the style and readability of the manuscript.

We acknowledge that some of our previous responses to the major comments were too brief. Due to the extensive restructuring and rewriting of the manuscript, we found it difficult to point to all individual changes within the time available for the revision. We appreciate the reviewer bringing this to our attention and will aim to provide more detailed responses and clearer references to the corresponding changes in future revisions and manuscripts.

## Main comments:

The introduction is overall improved, and the study objectives are clearer. However, I recommend some restructuring of the introduction. Right now the current introduction ends with a general discussion of regional modelling rather than with a clear statement of the research questions and the study structure. The study objectives (L. 36-40), appears already as the second paragraph, before discussing the relevant physical processes, any existing literature, or the motivation for comparing these products. I recommend moving it to the end of the introduction after establishing the general background and the motivation for the study.

We thank the reviewer for this comment, and have moved this paragraph to the end of the introduction. It has also been slightly modified following the comment of referee #2

I previously recommended removing the concept of storylines unless it was developed and explicitly connected to the analysis. The authors state in their response letter that the concept of storylines has been removed, and while it has been removed from the methodology and the discussion, it remains in two places in the revised introduction (L. 33-35, 73). Again, since the study does not appear to use the storyline framework in its analysis or interpretation, I recommend removing these two remaining references as well.

The sentence that was on lines 33–35 has been removed completely, and the sentence mentioning storylines on line 73 (now line 65) has been changed to :

Observations, reanalysis products, and models are complementary tools used to better understand the Arctic climate system, the processes behind recent changes, and its possible future evolution.

I do not think that the issue raised in my original comment on the selection of the datasets has been sufficiently addressed by the authors in the revised manuscript. The response letter provides an explanation for the selection of the regional models and the renanalyses, but it is still missing in the manuscript itself. The only justification I see in the manuscript are for OSI-SAF and ORAS5 as the observational reference and a global benchmark, respectively. However, there is still no clear explanation of why HIRHAM-NAOSIM and RASM were selected as the two regional coupled models, or why TOPAZ and RARE were selected as the two regional reanalyses. In the manuscript, the authors describe the configurations of these products but describing a dataset is not the same as explaining why it was selected for the study. This is especially important because the study now relies on fewer datasets. It is therefore important to understand what this comparison is intended to represent. In the response letter, the authors give very reasonable explanations, e.g. data availability, wanting to include different model components, and different sea ice assimilation approaches. I recommend including this information in a concise way into Section 2 of the manuscript.

Thank you for pointing this out. The following text have been added to Section 2, line 87->:

The regional products were selected to represent different model components, configurations, and approaches to data assimilation. In particular, the coupled models allow air–sea interactions to evolve within the model system, while the ocean reanalyses are forced by atmospheric reanalysis and observations. The two ocean reanalyses also differ in their treatment of sea ice observations: TOPAZ assimilates satellite sea ice concentration (SIC) observations, whereas RARE does not (Sec. 2.2).

## Minor comments:

L 41. Perhaps a bit redundant with "on the Siberian Shelf", I recommend removing.

Removed.

L 42. "Greenland Sea" is typically considered to be west of the deep Mid-Atlantic Ridge. Change to the "connected to the Nordic Seas" perhaps.

Thank you for pointing this out, we have changed this according to the suggestion.

L. 57. "E.g.," appears after the cited references. Change citation to "(e.g., Arthun et al [...])"

Thank you for noticing this, it have been fixed.

L 61. The colon reads a bit strange in that sentence. Maybe change to "[...] parts of the region. Dorr et al., 2024, for example, found that [...]"

This has been changed as suggested.

Figure 1. Change "Topography" and "Topographic" to "bathymetry" and "Bathymetric", respectively in the figure text. Then you write "(darker lines)" vs "(lighter colours)", I'd be consistent and write lines in both, i.e. "darker lines" and "lighter lines". Finally, I would not write a number part in numbers and part in text, i.e. "1255 thousand". And additionally, is it important to know the exact area of the two seas? In my view it doesn't add anything to the paper, and can be safely removed.

Thank you for pointing out the problems in the captions. The new caption is:

Barents and Kara Seas bathymetry, International Hydrographic Organization domains (darker lines; International Hydrographic Organization and Sieger, 2012), and regions with data available in all models (lighter lines), which are used for area-averaged comparisons. BSO stands for Barents Sea opening and BSX for Barents Sea exit. Bathymetric data from GEBCO Bathymetric Compilation Group (2026).

The areas were added to the caption, as the areas used for area-averaged comparisons depends on the models used, and having the numbers enables a reader to compare the SIA in table 2 to the whole region. The areas have been moved to the caption of table 2 to to be close to where they are relevant.

L. 114. The reference should come at the end of the sentence. I.e., "50° N (Xie et al., 2017)".

Thank you for notising this error. Fixed.

L. 114. Change the sentence starting at L. 114 to have the reference come at the end of the first clause. The result should be "[...] Hybrid Coordinate Ocean Model version 2.2 (HYCOM; Chassignet et al., 2003), and [...]".

Changed as suggested.

L 115. Reference should be at the end of the sentence to not interrupt the flow of the sentence so much. Change to "ERA5 data is used as atmospheric forcing (Hersbach et al., 2020)."

Changed as suggested.

L 124. Reference Carton and Chepurin should appear as a parenthetical citation and not a text citation here.

Fixed.

L. 126. Change the sentence to have the reference come at the end of the first clause. The result should be "[...] and Assimilation System 2.1 (PIOMAS; Zhang and Rothrock, 2003) are used to [...]".

Done.

L. 132. Can merge both parentheses and separate by comma. "(ORAS5, hereafter ORAS)".

Done.

L 135. Add comma after the reference to Gouretski and Reseghetti.

Added.

L. 141. Add appropriate reference at the end of the sentence.

Added [...], respectively (e.g. Gerland et al., 2023).

to the end of the sentence, line 144.

L. 142. Change “cover” to “covers”

Changed.

L. 167. Remove "and the depth of the ML is where the density differs from the density at 5 m by $\sigma$" since you already mention it at L. 164.

Removed.

L. 172. Write “Indices” with lower case letters.

Done.

L. 172-176. This part reads a bit strange. The authors first describe NAO as b), then AMO and PDO with no letter attached to it, then EA/WR as a), then NH OHC with no letter attached to it. Attach letters correctly going from a-e chronologically, or remove the letters.

The odd a) and b) stemmed from a problem in the references, these have now been fixed. The year in the EA/WR citation has been added and the NAO reference has been changed to a more appropriate one.

Table 2. The last sentence in the table text has “(least-squares regression)” outside of the sentence. Move the period after the parentheses.

Moved.

Figure 2. Since both panels share the same colorbar, I recommend moving the colorbar to the center of the figure rather than keeping it under the left panel only. This makes it clearer that both panels use the same colorbar.

The colorbar has been moved.

L. 242. Any appropriate references to add for where initial states lead to different results in the Arctic? Or perhaps more generally?

Added reference (Rinke and Dethloff, 2000).

TODO: if someone have some better or additional reference please feel free to add

L. 260. The line “[...] even though some change can be seen [...]” is a bit too vague. Please clarify exactly what change is being referred to (e.g., changes in SIA departures? In which direction? By how much?).

Thank you for pointing this out, we hope that the new version is clearer. This sentence now reads on line 262:

Departures from the climatological mean SIA in the datasets are low in winter and spring (January–April), when the Kara Sea is practically completely ice-covered, even though some SIA anomalies can be seen during this period, being strongest in OSI SAF and ORAS (Fig. 2c, d).

L. 297. Avoid relative terms such as “is discussed below”. I recommend specifying “S. 4.3.1 and 4.3.2”.

Specifying the sections as suggested.

L. 356. Change “indicating” to “indicates”.

Changed.

L. 370. Change “( Fig. 5h)” to “(Fig. 5h).

Changed.

L. 370. Change “but he maximum [...]” to “but the maximum [...]”.

Changed.

L 445. Change “(Sec. 2.2)Carton and Chepurin (2023)” to “(Sec. 2.2; Carton and Chepurin, 2023)”

Changed.

L. 471, 475, 477, 479, 481, 483, 491, 494. Separate the letters in the references to the appended figures by a space. E.g., change “(Fig. A7a,b,c)” to “(Fig. A7a, b, c)”.

Changed.

# Response to Referee #2 review round 2

In summary, this paper notes the importance of Arctic processes in understanding sea ice and temperature changes. Those processes are baked into coupled models and ocean reanalysis, so this study looks at how well these datasets re-produce observations of spring and autumn sea ice area and sea ice concentration. The aim is to evaluate several datasets. The datasets chosen, have a range of resolutions from 1/4 degree to 1/12 degree. They calculate correlations between the ice variables in observations and models. The mixed layer temperature and large-scale climate indices from observations are correlated with sea ice area in the coupled models and reanalysis. These correlations are then compared between the various products. As expected, the reanalysis preforms better than the coupled models in most cases. There is limited analysis into the processes.

We wish to thank referee #2 for their hard work in reviewing our manuscript.

## 1 General comments

The aim of the study ("evaluates several reanalysis products...") is broad and uninspiring. I would suggest reframing it with a more specific question like this and adjusting the rest of the paper to answer the question: How well do coupled models perform against the reanalysis and observations? Or Does the coupled atmosphere enhance processes that lead to sea ice area decline?

We thank the reviewer for this suggestion. We have revised the research question paragraphs and moved it to the end of the introduction (as suggested in lines 36 to 40 minor comment) to more specifically frame the question of how well the reanalysis products and regional ocean models represent observed sea ice and mixed layer temperature. We retain the broader comparison, as specifically attributing sea ice decline to atmosphere–ocean coupling is beyond the scope of this study.

The last paragraph of the Introduction on line 78 is now:

This paper evaluates several reanalysis products and regional climate models and focuses on assessing their skill in representing observed sea ice and mixed layer temperature in the Barents and Kara Seas. The associated biases and trends in each of these products and models are used to evaluate their strengths and weaknesses. This allows us to provide concrete guidance to users on the appropriate use of such products and models for different purposes. Understanding the performance of reanalysis and models in these regions is crucial due to their role as hotspots in the current Arctic climate change.

Overall, the coupled model results seem a bit muddled. The correlations between observational climate indices and sea ice in coupled models does not seem like a good choice given that the coupled model is unconstrained (within the domain) and evolve differently to the observed world so it is unclear if the lower correlation is the coupled model not representing the real-world index well or the processes linking the index to the sea ice are not well represented in the coupled model. It would make more sense to correlate the observations with their equal in the model. Then to understand the processes in the model better, a correlation between model variables or region to another model variable or region.

We thank the reviewer for this comment. The regional coupled models do not cover the complete regions required to calculate the climate indices, so equivalent indices cannot be calculated directly from the model output. However, the models are forced at their lateral boundaries by reanalysis, which constrains and steers the large-scale circulation. Calculating the indices from these reanalyses would give the same or very similar indices to the observations due to data assimilation, and we therefore consider this additional analysis to provide little benefit. If the global coupled models were still included, as in the previous version of the manuscript, we agree that climate-index equivalents should instead be calculated directly from those models.

The reanalysis results on their own seem quite incremental. RARE does not preform as well but the reader is left wondering why, as there is limited analysis and discussion on this.

We think that the reason why RARE does not perform as well as the other reanalyses is mainly due to the differences in the assimilation and the datasets used as shortly discussed in Sections 4.3.1 and 4.4.

TOPAZ4b and ORAS5 both assimilate SIC from satellite measurements, while RARE is only by the heat-flux relaxation based on PIOMAS SIT. PIOMAS itself assimilates SIC, but from a different source (NSIDC near-real time product). Additionally, the Resolution of PIOMAS is less than that of for example OSI-SAF used in TOPAZ, which probably also affects the results. (40 km average over the domain, north of 49°$N$ (Schweiger et al., 2019), and using an even coarser atmospheric forcing). We have added the following sentence to 4.3.1, line 345 to expand on this:

In addition, the PIOMAS data used for the sea ice constraint have a coarser spatial resolution than the satellite observations assimilated in TOPAZ and ORAS, which may further contribute to the larger discrepancies between RARE and OSI SAF.

Introduction mentions processes but the analysis always stops short of digging into the processes in the models that might be enlightening on why one is performing better than another.

We thank the reviewer for this comment. The addition of the climate-index correlations is our attempt to investigate the role of large-scale patterns and processes using the data available to us. A more detailed investigation of the internal model processes would require substantial additional analysis and data that is not readily available to us. We have therefore limited the process-oriented analysis to the climate-index correlations. Please also see our responses to minor comments 445–447 and 474–475 for further discussion.

## 2 Minor comments

Lines 36 to 40. This should be the last paragraph of the introduction, and should frame the problem and science question or hypothesis that you are answering or testing in the rest of the paper.

See answer to general comment.

Line 85 – 94. Why are these datasets picked? Have they previously been assessed in the Arctic?

All datasets have previously been assessed in the Arctic, but not necessarily focusing on the Barents and/or Kara Seas.

When choosing the regional models and reanalyses, the aim was to choose models and reanalyses that use different model components to get a variety of models, while still keeping the amount of products relatively small. For the regional coupled models, the availability of model data within the PolarRES project also played an important role.

OSI-SAF is the observational reference for sea ice, and the global reanalysis ORAS5 is a global benchmark that is generally known to perform well (e.g. Zuo et al., 2019), and it also provides ocean boundary conditions for HIRHAM-NAOSIM, making it a good addition as a reference, as observational data from the ocean are sparse.

Meanwhile, the marine reanalyses are evaluated less than some others, but as they are used by the community, their evaluation and reporting are valuable. Topaz4b is a regional analysis assimilating SIC data, while RARE does not assimilate SIC data and instead constrains the sea ice with PIOMAS SIT. This difference in assimilation gives us variety between the two regional reanalyses chosen.

HIRHAM-NAOSIM and RASM are two regional climate models which were involved in the PolarRES project from which the present study has been funded. The HIRHAM-NAOSIM model runs were specifically made as part of the PolarRES project. The two model systems have different model components, giving us a variety of models. Both models have been evaluated on the Arctic Ocean before.

The following paragraph has been added to the manuscript section 2, line 87:

The regional products were selected to represent different model components, configurations, and approaches to data assimilation. In particular, the coupled models allow air–sea interactions to evolve within the model system, while the ocean reanalyses are forced by atmospheric reanalysis and observations. The two ocean reanalyses also differ in their treatment of sea ice observations: TOPAZ assimilates satellite sea ice concentration (SIC) observations, whereas RARE does not (Sec. 2.2).

Line 105-11. What about the atmospheric model resolution?

The atmosphere and land share a polar-stereographic 50 km horizontal resolution. This has been added to the end of the paragraph.

Line 445-447. Why not look at the mechanism?

A detailed investigation of the mechanism is beyond the scope of this manuscript. Furthermore, we do not have all the necessary information to investigate it, for example, details of the heat flux relaxation based on PIOMAS or access to the RARE model code.

Line 474-475. Why not check if this is because of winds and/or currents?

We thank the reviewer for this suggestion. Investigating this mechanism would require extending the analysis beyond the scope of the present study. In particular, wind data are not included in the datasets considered here and would require adding an atmospheric reanalysis. While current data are available from the ocean reanalyses, currents have not been analysed in this study, which focuses on sea ice and ocean temperature. We have therefore retained this only as a possible explanation.

# References


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