

Response to reviewer 2

December 1, 2025

Thank you to the reviewer for taking the time to provide excellent constructive comments which should help improve the manuscript.

- "Why is OSISAF sea ice concentration chosen over other available observations? The choice of OSISAF data over other datasets needs to be justified in the text."

As in response to RC1:

On the choice of OSISAF observations: there are a number of reasons why we assimilate the OSISAF L3 data. We move to L3 because the assimilation system can cope with missing data, something the old system could not do and therefore required L4 data (gap filled). OSISAF provides a climate data record from 1978 onwards which seamlessly transitions into a near real time product. This near real time product is suitable for the operational schedule for NWP production. Moreover the data is available with operational levels of support. More information on the choice of observational data will be part of the overarching ORAS6 paper, currently in preparation.

- "It should also be considered how the observational uncertainty of the sea ice concentration observations effects the results particularly at the ice edge."

The uncertainty associated with the product is not something that we currently account for. This will be addressed in future updates, but was not part of the production system due to the need to have finished production with tight time constraints. Observations are perturbed, as mentioned at the end of Section 3.1. However we realise this is a deficiency of the system and results in underestimation of the uncertainty in the ice edge in the ORAS6 ensemble.

- "In figure 4 the authors only show the difference between the Gamma splitting and background splitting, but I think it would also be informative and useful for the discussion to show Peterson splitting against background splitting or Gamma splitting against Peterson splitting. This would be useful to see if there are further differences at the ice edge."

Thank you for asking for this plot - this has highlighted that we included the incorrect plot in our manuscript. Below we show the erroneous Figure 4, noting that it was actually showing the difference in performance in the Peterson splitting and the background splitting.

Figure 4 CORRECTED, shows now gamma splitting in comparison to background splitting. We see that the same pattern exists - a large degradation in the Atlantic sector. We will happily replace the figure with corrected version, and show both in a new version of the manuscript.

Moreover we will include Figure [NEW](#) to show verification in sea ice concentration against the ESA CCIv3 SIC product of [Embury et al. \[2024\]](#) . We think this will significantly help the reader understand the choices made regarding the splitting methodology.

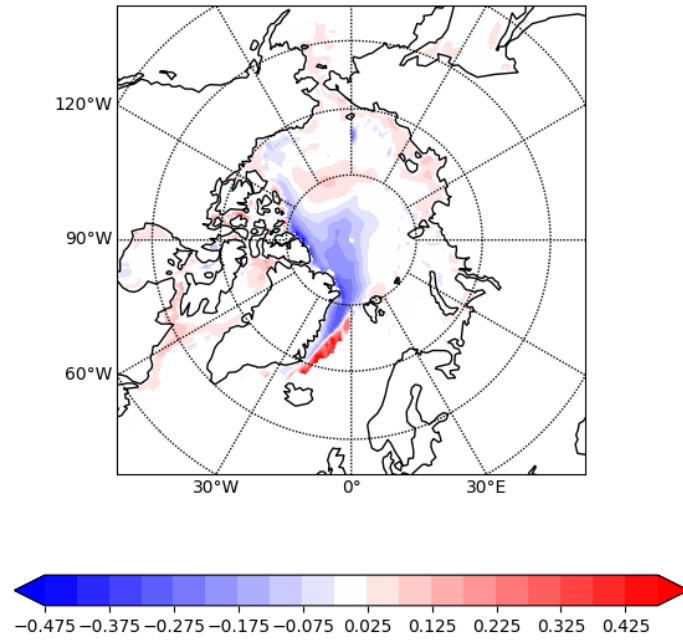


Figure 4: Errors in analysis of sea ice thickness comparing to *CS2SMOS* merged *CryoSat-2* and *SMOS* ice thickness. Shown is the difference in RMSE using the ~~gamma~~ **Peterson** splitting compared to background splitting. In red are regions of worse sea ice thickness, which is notable around the ice edge in the Atlantic.

O. Embury, C. J. Merchant, S. A. Good, N. A. Rayner, J. L. Høyer, C. Atkinson, T. Block, E. Alerskans, K. J. Pearson, M. Worsfold, et al. Satellite-based time-series of sea-surface temperature since 1980 for climate applications. *Scientific Data*, 11(1):326, 2024

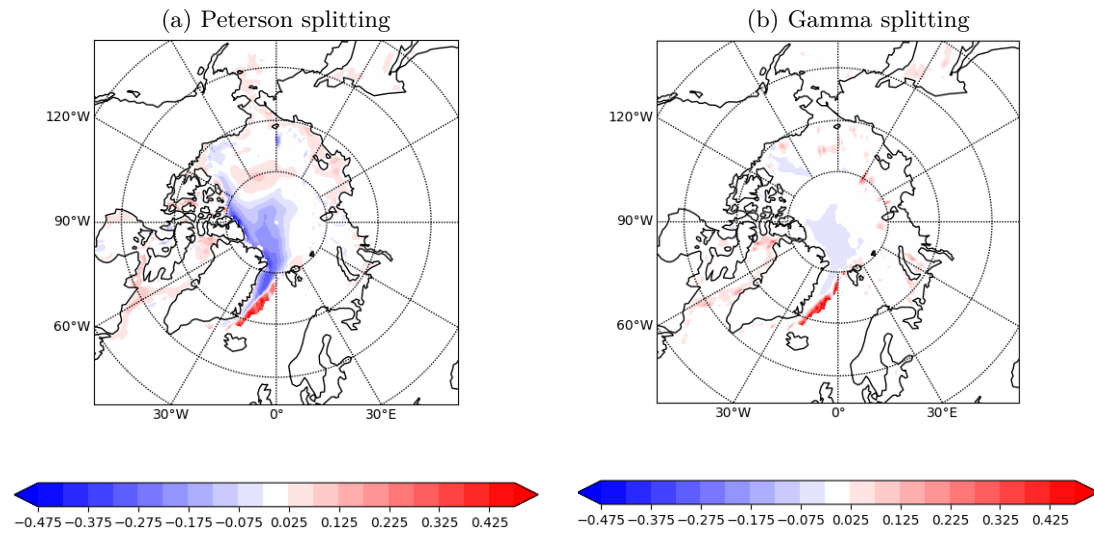


Figure 4 CORRECTED: Errors in analysis of sea ice thickness comparing to *CS2SMOS merged CryoSat-2 and SMOS ice thickness*. Shown is the difference in RMSE using (a) the Peterson splitting, and (b) the Gamma splitting, compared to background splitting. In red are regions of worse sea ice thickness, which is notable around the ice edge in the Atlantic.

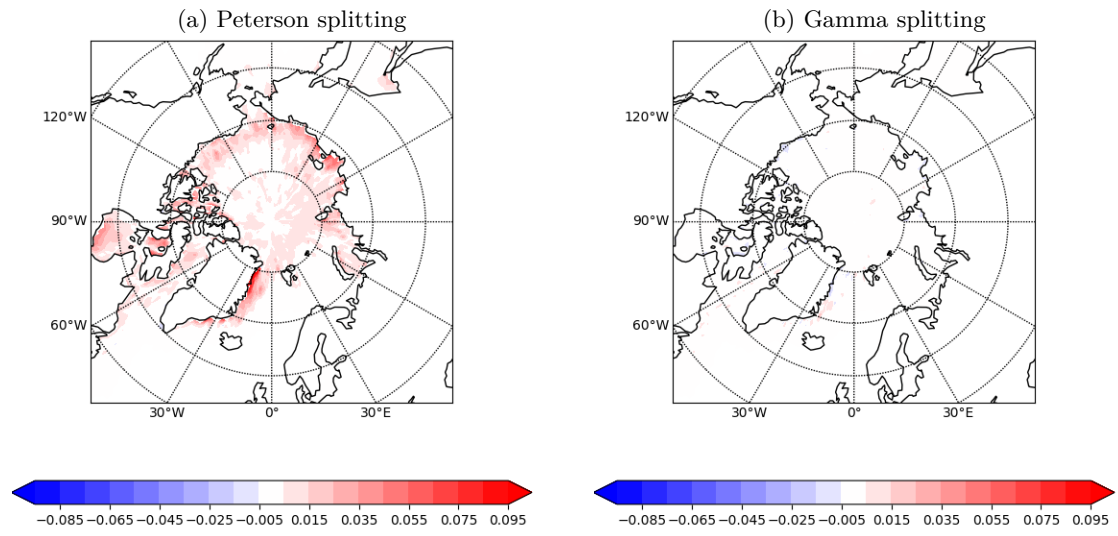


Figure NEW: Errors in analysis of sea ice concentration comparing to *ESA CCIv3*. Shown is the difference in RMSE using (a) the Peterson splitting, and (b) the Gamma splitting, compared to background splitting. In red are regions of worse sea ice concentration, which is notable across large areas of the Arctic domain when using Peterson splitting.

- "The ice induced temperature increment approach for the assimilation is interesting. I am wondering why the authors only chose smaller values of α than the control for their experiments in Section 6.3, and none larger? From the current analysis it looks like an α larger than five may lead to lower RMSE."

As in response to RC1:

This is a very fair question. We are conservative in the approach and do not want the ice induced temperature increments to be larger as they could be compensating for errors in the forcing and not in the ocean state. Following equation (5), if δa is, say, 0.5 (a 50% change in ice concentration - large but not unexpected near the ice edge), then with $\alpha = 5$ this gives a 2.5K temperature increment to the ocean. These are regions where there are limited in situ temperature measurements at depth, and so we fear making even larger temperature increments could have negative consequences on the 3D ocean state. We do hope to improve on the simplicity of this scheme in the future, and very much recognise the need for and welcome suggestions for future developments to this approach.

- "Line 70: Are these the default bounds for the categories in the SI3 sea ice model or do the authors choose unique ones? Why are they chosen?"

These are the bounds provided by the configuration with 5 categories which we take as our default, yes. It is entirely configurable - we know for instance at Mercator Ocean International they run with 11 ice categories with different bounds [Chenal et al., 2024]. There is some evidence that 5 categories are sufficient for different use cases, for example "It is also found that the current default discretization of the NEMO3.6-LIM3 model is sufficient for large-scale present-day climate applications.", [Massonnet et al., 2019].

"With five to seven categories the errors due to finite resolution of the thickness distribution are much smaller than the errors due to other sources. These results suggest that seven categories are sufficient for climate modeling. Five categories are adequate if we accept errors of 0.1 W m⁻² in the surface fluxes, 10 kN m⁻¹ in the ice strength, and 2 cm in the thickness range." [Lipscomb, 2001].

A. Chenal, G. Garric, C.-E. Testut, M. Hamon, G. Ruggiero, F. Garnier, and P.-Y. Le Traon. Assimilation of radar freeboard and snow altimetry observations in the arctic and antarctic with a coupled ocean/sea ice modelling system. *EGUsphere*, 2024:1–39, 2024. doi: 10.5194/egusphere-2024-3633. URL <https://egusphere.copernicus.org/preprints/2024/egusphere-2024-3633/>

F. Massonnet, A. Barthélemy, K. Worou, T. Fichet, M. Vancoppenolle, C. Rousset, and E. Moreno-Chamarro. On the discretization of the ice thickness distribution in the NEMO3.6-LIM3 global ocean-sea ice model. *Geoscientific Model Development*, 12(8):3745–3758, 2019. doi: 10.5194/gmd-12-3745-2019. URL <https://gmd.copernicus.org/articles/12/3745/2019/>

W. H. Lipscomb. Remapping the thickness distribution in sea ice models. *Journal of Geophysical Research: Oceans*, 106(C7):13989–14000, 2001. doi: <https://doi.org/10.1029/2000JC000518>. URL <https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2000JC000518>

- "Line 126: Sea ice concentration and thickness are often treated as independent variables, but they can be correlated. For example during the melt season thinner ice will melt more quickly than thicker ice from the ocean and atmospheric forcings."

As in response to RC1:

We understand the reviewer’s strong view here, and agree with all their scientific points. However we think this is partially a miscommunication on our part, as we have used the term “orthogonal” in the geometric sense to mean perpendicular. We absolutely agree that the quantities are related and correlated, especially with a multcategory model, but they remain representative of spatial extent (SIC) and vertical extent (SIT). This we attempt convey in Figure 1.

We would propose to change the wording such that our statement “Noting that sea ice concentration is orthogonal to sea ice thickness” would become “Noting that sea ice concentration represents the spatial extent and sea ice thickness represents the vertical extent”

We recognise the remainder of the minor comments (below). We will happily correct/act on them in the manuscript.

- Sometimes used sea-ice with hyphen and sometimes sea ice without hyphen, need to be consistent.
- Line 24: NEMOVAR not defined
- Line 28: Many others have applied Ensemble Kalman filter approaches which are relevant to this study:
- Fritzner et al., 2019 assimilates SIC, SIT and snow depth in this approach
- Williams et al., 2023 Assimilation of thickness distribution
- Fiedler et al., 2023 Assimilation of Freeboard
- As another method: Yang et al., 2014 assimilates sea ice using SEIK filter
- Line 34: “Other developments on sea-ice assimilation including attempt to constrain” should be “include attempts to”
- Line 50: remove “the” before SI3
- Line 106: “We have seen that” can be removed.
- Line 126: Sea ice concentration and thickness are often treated as independent variables, but they can be correlated. For example during the melt season thinner ice will melt more quickly than thicker ice from the ocean and atmospheric forcings.
- Line 201: should be “first appeared”
- Line 216: “Is used” is repeated
- Line 230: The sentence is unclear and quite long. It should be reworded or broken down for readability.
- Line 274: The sentence “Ice induced temperature increments give the largest impacts of all developments.” should be expanded upon/explained further to conclude clearly.
- Lines 282-292: The work of Bocquet et al., 2024 which produced a longer time series of freeboard and ice thickness from 1994 for both Arctic (winter only) and Antarctic (year-round) should be mentioned also. Numerous other studies which have also assimilated ice thickness/freeboard alongside Mignac et al., 2022 could also be mentioned.

- Figures Generally, in many figures the colour bar is not labelled and units are not provided.
- Figure 1: The authors used “melt pond” in the caption, but “meltpond” without space in the figure, need to be consistent.
- Figure 5: It’s difficult to see the differences with the current chosen colour scale.
- Figure 7: “sea” is repeated in the caption.