Authors' Response to Reviews of

A reanalysis of the Arctic sea ice cover over the satellite era utilising summertime observations of sea ice thickness

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The Cryosphere,

RC: ***Reviewers' Comment***, AR: Authors' Response, □ Manuscript Text

Dear Editor and Anonymous Reviewers

Firstly, we would like to thank you all very much for the constructive comments and suggestions for the manuscript "A reanalysis of the Arctic sea ice cover over the satellite era utilising summertime observations of sea ice thickness". Your insights are very useful in enhancing the quality of our work. Based on the comments and suggestions, we plan to revise the manuscript accordingly.

Please find our detailed point-by-point responses to the reviewers' comments in the following sections. Below, we list each comment (Reviewer Comment, **RC**) and insert our response (Authors' Response, **AR**).

Sincerely,

Nicholas Williams
On behalf of all the authors

Anonymous Reviewer 1

The study "A Reanalysis of the Arctic sea ice cover over the satellite era utilising summertime observations of SIT" presents four different model experiments, which are compared to different observations, satellite products and other model runs, with the aim of identifying the models physical deficiencies. To me it appears like the authors are trying to do too many things at once. I could see that there are two studies in the current paper. One focusing on assimilation technic, looking at the thickness distribution and the assimilation period and one looking at the physical drivers of the marginal ice zone. Both studies however would need some additional model experiments and some heavy restructuring of the text. There are four major points that I think need to be address in relation to this:

RC: ***1) Formulate a clear question you want to answer***

Currently there is no clear question stated, nor answered. From the topics that are touched app on in the introduction I would assume that the paper answers some question in regards to assimilation technic, in that case I think the choice of model runs is questionable (see point 2), or that the study aims at finding physical drivers that are missing in the model set up to simulate a correct marginal ice zone (MIZ). For the second point I am also missing some experiments (see point 3), but also a clear definition of the MIZ and an ocean ice coupled model. Many effects in the MIZ are driven by the ocean and most state of the art sea ice modeling systems are using at least sea ice-ocean coupled models, which the authors seem to be aware of, since they are citing them in the introduction. So, to add to the current discourse it would be crucial to run a sea ice-ocean coupled model.

AR: Thank you to the reviewer for this comment. We agree that the aims of the study were not sufficiently clear in the original manuscript. The primary objective of the manuscript is to assess the impact of different data assimilation configurations, particularly the choice and temporal availability of assimilated observations, and to provide support for the development of future sea ice reanalyses. The MIZ analysis is intended as a diagnostic evaluation of how the different assimilation configurations affect the representation of the MIZ when compared with observations. It is not intended to attribute MIZ biases to individual physical processes or to identify the missing processes required to reproduce the observed MIZ. As the focus in this study is not on the MIZ processes this

We will therefore reformulate the abstract and introduction to state the research questions more explicitly. We will also revise the framing of the MIZ analysis throughout the manuscript so that it is clear that the MIZ is used primarily as a diagnostic of the impact of the assimilation configurations, rather than as a means of identifying individual physical drivers of MIZ variability or bias.

In particular, we will remove or reword language that implies that the study identifies specific physical processes responsible for differences between the experiments. Where possible physical mechanisms are discussed, these will instead be presented as possible explanations or hypotheses rather than as demonstrated causal mechanisms. This includes statements concerning the role of snow thickness in explaining the simulated sea ice volume, which we agree are too strongly stated given the evidence presented.

2) Choose the model runs accordingly

First of all, most studies cited as state of the art sea ice assimilation studies in the introduction are running at least sea ice-ocean coupled models. I am aware that a fully coupled model is heavy to run, but by running a stand alone sea ice model, forced with a ocean climatology I wonder what the missing ocean forcing does to the sea ice state. This is not discussed nor even mentioned in the discussion. Further do the model runs used in the study appear a bit random. What does CASIRA-b add to the study? Even the authors them self

seem not too interested in it, as they don't mention it at all in the discussion. Finally I am wondering why the assimilation technics vary between CASIRA-n and CASIRA-d. Using the same assimilation technic would allow to actually discuss the influence of summer vs. only winter assimilation, but like the study is set up right now the differences might originate from either the data, the assimilation technic, or the assimilation period.

AR: We agree that the rationale for the choice of model experiments, and in particular the role of the prescribed ocean forcing, should be made clearer.

Regarding the issue of sea ice-ocean coupling, we agree that the use of prescribed ocean forcing is an important limitation that should be considered when interpreting the results. However, we do not consider an ocean-coupled experiment necessary to address the primary question of this study, which is focused on the impact of different sea ice data assimilation configurations. Standalone CICE simulations forced by prescribed ocean conditions have been widely used to investigate Arctic sea ice evolution. For example, Hunke (2010) demonstrated that CICE forced with a one-dimensional ocean mixed-layer model can reproduce the interannual variability of Arctic sea ice, while Archibald et al. (2024) compared standalone CICE simulations with coupled NEMO-CICE simulations and found comparable performance for a number of sea ice characteristics.

To further assess the importance of the ocean configuration in our particular model set-up, we have additionally tested our standalone CPOM-CICE model, using the 1D mixed-layer ocean climatology, against the same model coupled to the NEMO ocean model (NEMO v3.6). The results are shown in Figure R1. The two configurations exhibit differences in their simulated sea ice state, particularly during summer. While the NEMO-CICE configuration produces a March sea ice extent that is closer to the satellite observations, it also exhibits substantially lower September sea ice extent and volume, particularly during the latter part of the simulation. In contrast, the standalone CICE configuration maintains a more persistent summer ice cover and greater sea ice volume.

Importantly, the differences between the standalone CICE and NEMO-CICE configurations are generally smaller in magnitude than those between the CASIRA-b and CASIRA-n reanalyses. This indicates that, within our model framework, the choice of assimilated SIC product has a larger influence on the simulated sea ice state than the choice between the standalone and coupled ocean configurations. This comparison supports the use of the standalone configuration for the primary objective of this study, while we acknowledge that ocean coupling may be important for other applications. We will nevertheless add a discussion of the limitations associated with the prescribed ocean forcing, including its potential influence on the simulated MIZ, to the revised manuscript.

We also agree that the role of CASIRA-b was not sufficiently clear in the original manuscript. CASIRA-b provides an additional experiment in which the assimilation methodology is held fixed while a different SIC observation product is assimilated. This allows us to isolate the sensitivity of the resulting reanalysis to the choice of SIC observations. The comparison demonstrates that the choice of SIC observations can substantially influence the resulting sea ice thickness and volume, despite the otherwise identical assimilation configuration. We therefore consider CASIRA-b important for demonstrating the sensitivity of a reanalysis to the choice of assimilated SIC observations. We will make this experimental motivation clearer in the revised manuscript and will discuss the role of CASIRA-b more explicitly in the Results and Discussion.

Finally, we would like to clarify that the assimilation technique is the same in all of the model experiments. The assimilation algorithm and assimilation settings are unchanged between CASIRA-n and CASIRA-d. The difference between these experiments is the temporal availability of the assimilated SIT observations: CASIRA-n assimilates SIT throughout the year, whereas CASIRA-d assimilates SIT only during winter. Thus, the experimental design allows us to investigate the effect of the period over which SIT observations

are available without introducing a change in assimilation methodology.

In our previous study (Williams et al., 2023), we also found that, using the same assimilation technique, assimilating the SIT distribution primarily affected the thickest sea ice category, with much smaller impacts elsewhere including on mean SIT. This provides additional support for interpreting the differences between CASIRA-n and CASIRA-d primarily in terms of the different periods of SIT assimilation. We will revise the description of the experimental design to make this distinction explicit.

3) Support your findings with the results from the model runs

The study of the MIZ is interesting and highly relevant, however I am currently missing the support for the findings. The discussion for example states that the SIV overestimation is due to too little snow (line 472). But I can not see any results supporting this findings. I could also see other factors than the snow thickness effecting the SIV (for example the drag parametrization, or the ocean forcing). I would expect some sensitivity study or a like. As the study is build up right now the physical drivers might be a good guess, but I am not seeing any evidence for the findings in the study. Furthermore I would expect the ocean drivers to be quite important for the formation of the MIZ, but I am currently missing a thorough discussion of what the missing ocean forcing might do to the modeled MIZ.

AR: We agree with the reviewer that some of the physical interpretations in the original manuscript were stated more strongly than is supported by the experiments. In particular, we agree that the statement attributing the overestimation of sea ice volume to insufficient snow thickness is not directly demonstrated by the model results. Other factors, including the drag parametrization and the prescribed ocean forcing, could also contribute to the simulated sea ice state. We will therefore remove or reword such statements and present these mechanisms as possible explanations rather than demonstrated causes inside the discussion.

The comparison between the standalone CICE and NEMO-CICE configurations described above provides an additional indication of the potential role of ocean forcing. The two configurations produce different sea ice states, particularly during summer, demonstrating that the prescribed ocean forcing can influence the simulated sea ice evolution. We will therefore expand the discussion of the limitations associated with the prescribed ocean forcing and explicitly acknowledge that ocean processes may also influence the simulated MIZ.

We will consequently revise the Discussion to distinguish between (i) differences that are directly demonstrated by the assimilation experiments and (ii) possible physical explanations for those differences. Where the experiments do not provide sufficient evidence to identify a specific physical driver, we will state this explicitly rather than presenting the mechanism as a conclusion of the study.

RC: ***4) Choice of data***

I think it would be beneficial to actually use more independent data for the validation. The BGEP data is commonly used, but also spatial limited. An addition of, for example the Fram Strait observatory ULS data, would add a lot to the discussion. And also for the SIE analysis I would recommend another more independent data set, as for example the MASIE sea ice extent, which could be used to calculate the integrated ice edge error from the different experiments.

Finally I am missing a discussion of the short come of the used data sets. Which benefits/short comes do ULS vs. air born data have? Which questions can we answer with them, which not? How reliable is PIOMAS? What are the differences between AYR-CS2 and WIN-CS2? What effect might this differences have on the assimilation?

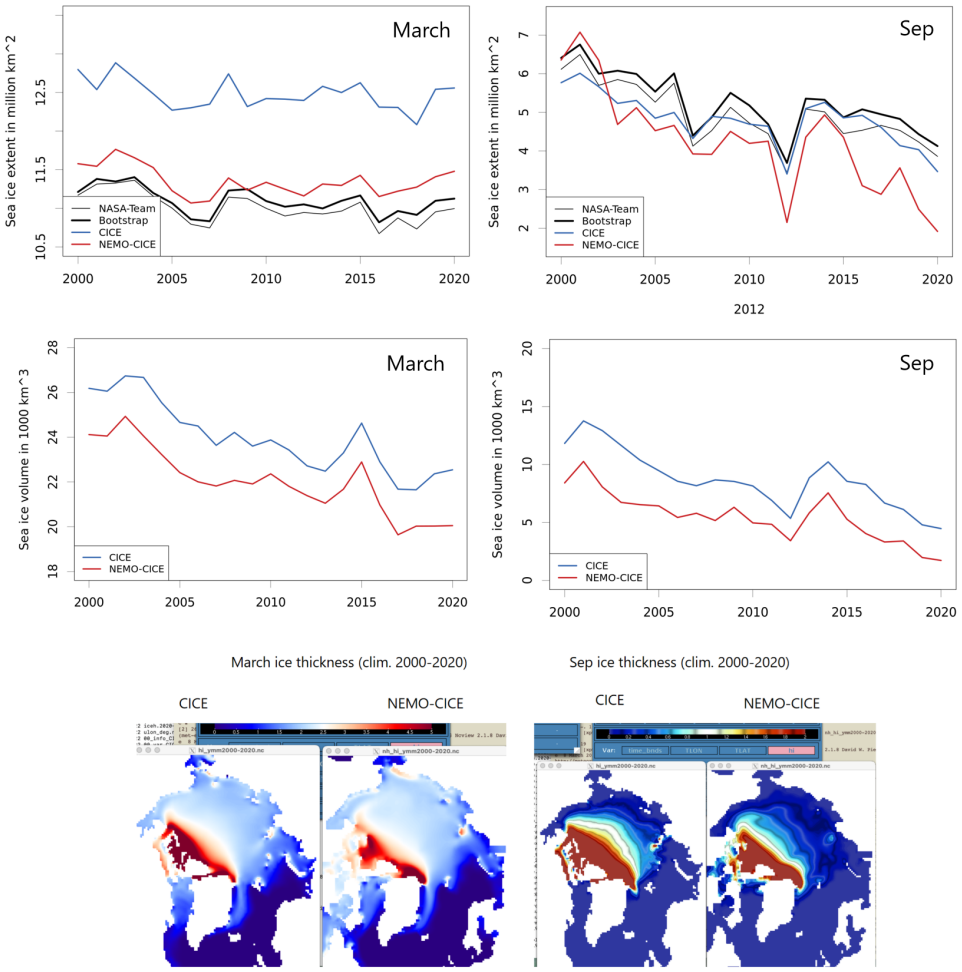


Figure R1: Comparison of Arctic sea ice simulated by CICE with climatological 1D ocean layer and by the fully coupled NEMO-CICE configuration over 2000–2020. (top left) March sea ice extent from the NASA-Team and Bootstrap satellite products, CICE, and NEMO-CICE; (top right) corresponding September sea ice extent. (middle left) March sea ice volume from CICE and NEMO-CICE; (middle right) corresponding September sea ice volume. (bottom) 2000–2020 climatological sea ice thickness in March and September from CICE and NEMO-CICE.

After mentioning my main concerns I would also like to mention that I do think that there is some potential in the paper, as already stated in the introduction paragraph. Furthermore, I liked the approach of using assimilation studies to evaluate sea ice models, appreciated the detailed description of the validation methods and the fact that the authors both evaluated the model sea ice volume and thickness.

AR: Thank you to the reviewer for these helpful comments and suggestions. We agree that additional independent observations would strengthen the validation and provide a broader assessment of the reanalysis experiments.

We will add observations from the Fram Strait Observatory ULS to provide an additional independent verification of sea ice thickness. The Fram Strait Observatory ULS provides a long-term record extending from 1990 to the present, although its spatial coverage is limited to two adjacent grid cells in the Fram Strait. We consider this complementary to the BGEP ULS observations, which provide observations at several locations within the Beaufort and Chukchi Seas. We will also include a comparison with ICESat-2 sea ice thickness observations. ICESat-2 observations are only available from November 2018 and therefore provide a relatively short validation period within our 1980–2020 reanalysis. Nevertheless, they provide an important independent evaluation of the spatial distribution of sea ice thickness and complement the longer-term ULS observations, which provide point measurements at fixed locations. We will therefore use ICESat-2 to assess the agreement between the reanalysis experiments and observed sea ice thickness during the period of overlap. RMSE and Bias comparisons with the reanalysis experiments will be added to Table 2 in the manuscript.

For sea ice concentration, we will use the independent AMSR2 sea ice concentration dataset to provide an additional verification of the reanalysis experiments. AMSR2 is based on independent satellite observations compared with the NASA Team and Bootstrap products used in the assimilation, and provides spatially complete sea ice concentration observations. We therefore consider it a useful independent verification of the simulated sea ice concentration and ice edge. We will use AMSR2 as the independent dataset for the sea ice concentration and to assess the integrated ice edge error.

In response to the reviewer's final point, we will expand the discussion of the limitations, uncertainties, and characteristics of the datasets used for validation in the datasets for validation section. This will include a discussion of the advantages and limitations of ULS, airborne, and satellite observations, their respective spatial and temporal coverage, and which aspects of the sea ice state each dataset can constrain. We will also discuss the uncertainties associated with PIOMAS and the differences between the AYR-CS2 and WIN-CS2 products, including how these differences may affect the evaluation and assimilation experiments.

We thank the reviewer again for these suggestions. We agree that explicitly discussing the characteristics and limitations of the independent datasets is important for interpreting the validation results and for understanding the extent to which the different observations provide complementary information about sea ice thickness, volume, and concentration.

Minor comments:

RC: ***How is the assimilation state vector set up? Are they similar for all assimilated runs?***

AR: The assimilation state vector is set up the same for all assimilated runs and the same as Williams et al. (2023). The state vector includes all variables used in the CICE restart files. These include ice concentration and volume per thickness category per unit grid cell, ice and snow enthalpy per enthalpy layer, melt pond area, depth and lid thickness, sea ice age. We will make this more clear in the revised manuscript, thank you for your helpful suggestion.

RC: ***Color bars: Make them colorblind friendly***

AR: We will modify the colorbars accordingly, thank you for your helpful suggestion.

RC: ***Figure 2g: It looks like the data sets are averaged over different periods and you compare the much shorter ULS with the longer model runs. This is misleading.***

AR: Apologies for this oversight. We will update the figure with the correct averages over the ULS C period.

RC: ***Figure 8: Why is WIN-CS2 not included?***

AR: It was initially not included for consistency, as we show both winter and summer sea ice thickness, and WIN-CS2 is not available in summer, however we will add the March WIN-CS2 SIT to this plot for further reference. Thank you for your suggestion.

1. Anonymous Reviewer 2

General comments

RC: ***The manuscript "A reanalysis of the Arctic sea ice cover over the satellite era utilising summertime observations of SIT" presents an interesting sensitivity study with the potential to demonstrate the value of assimilating year-round Sea Ice Thickness (SIT) observations to improve sea ice reanalyses and better understand sea ice model biases.***

The intended purpose of the reanalyses presented in the paper could, however, be clarified. Is the primary objective to assess the impact of different assimilation strategies and support the development of future sea ice reanalyses, or to provide a dataset for broader scientific applications such as climate change and Arctic variability studies?

The manuscript would also benefit from a stronger focus on CASIRA-n. In particular, the behaviour of CASIRA-b prior to 2010 is not sufficiently explained, making it difficult to interpret some of the comparisons presented later in the paper.

AR: Thank you to the reviewer for taking the time to review our manuscript. We clarify that the primary objective of the manuscript is to assess the impact of different data assimilation strategies and to provide support for the development of future sea ice reanalyses, rather than to develop a reanalysis dataset specifically for broader climate applications. We will restructure the abstract, introduction and Results section to make this objective clearer and to place greater emphasis on CASIRA-n as the principal reanalysis experiment.

We will also explain the behaviour of CASIRA-b prior to 2010 in greater detail. This will clarify its role in demonstrating the sensitivity of the reanalysis to the choice of SIC observations and provide the necessary context before shifting the focus to CASIRA-n in the remainder of the Results section.

RC: ***Results and evaluation***

The Results section could be better organized to strengthen the overall narrative. More specifically, the evaluation of the reanalysis should not rely solely on comparisons with in situ SIT observations but should incorporate the broader set of variables and diagnostics presented throughout the Results section.

One possible structure could be:

Demonstrate that assimilating year-round SIT observations, independently of SIC assimilation, improves the behaviour for all parameters and eventually improves the agreement with independent in situ SIT measurements. Show how the improved reanalysis helps identify and understand the underlying model deficiencies, thereby providing guidance for future developments of sea ice reanalyses, potentially including fully coupled ocean-sea ice systems. Demonstrate that the combined assessment of observation fit and model behaviour contributes to a more comprehensive evaluation of the reanalysis and increases confidence for future users.

AR: We thank the reviewer for their comments and suggestions on how to reorganise and reevaluate the Results section. In response, we will reorganise the Results section to strengthen the focus on the development of sea ice reanalyses and the assessment of different assimilation strategies:

Comparison of the reanalysis experiments and assimilated observations - We will first compare the reanalysis experiments with each other and with the assimilated observations. This section will explain the behaviour of CASIRA-b prior to 2010 and clarify its role in the study, after which the analysis will focus primarily on

CASIRA-n and CASIRA-d.
Evaluation of the impact of year-round SIT assimilation – We will then compare CASIRA-n with CASIRA-d to evaluate the effect of assimilating SIT observations throughout the year, rather than only during winter. The evaluation will consider a broader set of independent observations and model diagnostics, including SIC from AMSR2 and SIT from OIB, BGEP ULS, Fram Strait Observatory ULS, ICESat-2, and CryoSat-2. In addition to assessing the agreement with independent SIT observations, we will examine how year-round SIT assimilation affects the broader sea ice model state and its behaviour. This will demonstrate whether the improved fit to independent observations is accompanied by improvements in other aspects of the reanalysis and will help identify remaining model deficiencies.
Comparison with the CPOM-CICE control after 2010 - Finally, we will evaluate the behaviour of CASIRA-n relative to CPOM-CICE during the period when the full set of observations is available, allowing us to assess the impact of the assimilation system relative to the unassimilated model.

As also noted in our response to Reviewer 1, we have previously shown that assimilation of the sea ice thickness distribution (SITD) has only small effects on total mean sea ice thickness in our system (Williams et al., 2023). The assimilation technique used in the present study is identical to that used in Williams et al. (2023). The SITD assimilation primarily modifies the thickness of the thickest two categories and only has a small effect on mean sea ice thickness. We will clarify this point in the revised manuscript so that differences between the experiments can be interpreted in terms of the relevant differences in their assimilated observations and assimilation periods.

RC: ***Forcing and validation datasets***

The choice of the forcing and validation datasets deserves further justification.

Why were additional available in situ observations not included in the validation? Why were independent datasets, such as ICESat-2 observations, not used for evaluation? A spatial density map of the OIB observations would also be useful to assess the representativeness of the validation dataset. Similarly, the choice of ocean forcing is not sufficiently justified. The rationale for using this dataset should be explained in more detail. In addition, I was unable to locate the reference corresponding to Ferry et al. (2011), and this should be verified.

AR: Thank you to the reviewer for their suggestions. We agree that additional independent observations would strengthen the validation of the reanalysis experiments.

We will add observations from the Fram Strait Observatory ULS, which provides a long-term record extending from 1990 to the present and therefore provides an important complementary constraint on the temporal evolution of sea ice thickness. Although its spatial coverage is limited to two adjacent grid cells in the Fram Strait, it complements the BGEP ULS observations, which provide measurements at several locations in the Beaufort and Chukchi Seas. We will also include a comparison with ICESat-2 sea ice thickness observations. Although ICESat-2 observations are only available from November 2018 and therefore cover a relatively short period of our reanalysis, they provide an independent spatially distributed measurement of sea ice thickness. We will use the period of overlap to assess the agreement between the reanalysis experiments and ICESat-2 observations, and add the resulting RMSE and bias comparisons to Table 2 in the manuscript.

Additionally, we will add the locations of the OIB observations to one of the figures in the paper to provide a clearer indication of their spatial representativeness.

The Ferry et al. (2011) dataset has been extensively used as the ocean forcing in the CPOM-CICE model since its early development (Flocco et al., 2010; Tsamados et al., 2013; Schröder et al., 2014, 2019; Williams et al., 2023; Frew et al., 2025), which is why we have continued to use it in this study. The sea ice model,

when forced with this ocean reanalysis, is able to provide good estimates of the interannual and intra-annual variability of both Arctic and Antarctic sea ice (Schröder et al., 2019; Frew et al., 2025). We will add further justification for the use of this dataset in the methodology section with the above references. The ocean reanalysis described by Ferry et al. (2011), known as "MYO-WP4-PUM-GLOBAL-REANALYSIS-PHYS-001-004" is a legacy ocean reanalysis project from the MyOcean project, which was a precursor to the Copernicus Marine Service (CMEMS). This legacy product has now been removed from the Copernicus website.

RC: ***Model and reanalysis description***

The description of the model configuration and reanalysis setup could be made more precise.

Spatial resolution: the reference to the ORCA1 grid may not be sufficiently clear for all readers. A brief explanation and/or an appropriate reference would be helpful. Temporal resolution: the temporal frequency of the reanalysis products should be stated explicitly (e.g., monthly means or another output frequency).

AR: Thank you to the reviewer for this helpful suggestion. The temporal resolution of the reanalysis products are monthly means. We will add information about both the spatial and temporal resolution in the text as follows:

> We run CPOM-CICE on the ORCA 1 degree tripolar grid over a pan-Arctic domain. This is a tripolar curvilinear ocean grid developed for the NEMO Ocean model (Madec et al., 2015) with a nominal 1-degree horizontal resolution which avoids a singularity in the northern pole by placing its two northern poles over land. The analysis is based on monthly mean outputs across all reanalysis experiments.

RC: ***Data assimilation methodology***

The data assimilation methodology should be described in greater detail.

The system description could provide more information on the control vector and on the volume conservation constraints applied when only SIC is assimilated. Section 2.3 appears to mix the description of the observations with that of the observation operators, which makes the workflow difficult to follow. For example, SIC appears to be assimilated daily, whereas SIT is assimilated monthly. Separating the descriptions of the assimilated observations and the observation operators would provide a clearer understanding of the methodology and the associated processing steps.

AR: Thank you to the reviewer for their suggestion which will help to clarify the methods used. We will expand the description of the data assimilation methodology to explicitly describe the control vector, the volume conservation constraints applied following SIC assimilation, and the temporal frequency at which each observation type is assimilated. We will also separate the description of the observations from that of the corresponding observation operators to make the assimilation workflow clearer. The methodology is identical to that described in Williams et al. (2023), and we will provide explicit references to the relevant sections of that study.

RC: ***Presentation of the figures and results***

The way the figures are discussed in the text could be improved.

It may be easier for the reader to first describe the characteristics and behaviour of the observations and then discuss how the reanalysis reproduces, explains, or deviates from these observational features. A stronger focus on CASIRA-n throughout the Results section would also help improve the clarity of the narrative.

AR: Thank you to the reviewer for the suggestion which will help improve our manuscript. We will include further

discussion concerning the observations used for validation in that section, and additionally increase the focus on CASIRA-n, particularly in the section for validation of the reanalysis.

RC: ***Ocean coupling***

The absence of coupling with the ocean should be discussed more explicitly.

The current framework does not account for variations in ocean temperature and heat transport associated with ocean circulation, which may influence sea ice evolution. This limitation, together with its potential implications for the interpretation of the results, should be clearly acknowledged in the Discussion section.

AR: As also discussed in our response to Reviewer 1, we agree that the absence of an interactive ocean is an important limitation of the modelling framework and that its potential implications for the interpretation of the results should be discussed more explicitly. We will therefore add a discussion of this limitation to the revised manuscript, including the potential influence of prescribed ocean temperature and heat transport on the simulated sea ice evolution and MIZ representation.

Standalone CICE simulations forced by prescribed ocean conditions have nevertheless been widely used to investigate Arctic sea ice evolution. (Hunke, 2010) showed that CICE forced with a one-dimensional ocean mixed-layer model can reproduce the interannual variability of Arctic sea ice, while Archibald et al. (2024) compared standalone CICE simulations with coupled NEMO-CICE simulations and found comparable performance for a number of sea ice characteristics. In addition, as described in our response to Reviewer 1, we have performed a direct comparison between our standalone CPOM-CICE configuration and the coupled NEMO-CICE configuration. Although the two configurations show differences, these are generally smaller than the differences between CASIRA-b and CASIRA-n resulting from the choice of assimilated SIC product. This supports the suitability of the standalone configuration for the primary objective of evaluating the impact of different data assimilation configurations, while acknowledging the limitations associated with the prescribed ocean forcing.

Detailed Comments

RC: ***L125: The cited reference is no longer accessible, making it difficult to identify the ocean reanalysis that was used. Please clarify which product was employed and explain why more recent reanalyses (e.g. TOPAZ `https://data.marine.copernicus.eu/product/ARCTIC_MULTIYEAR_PHY_002_003/description`, GLORYS12, GREP) were not considered.***

AR: The ocean forcing used in this study is the ocean reanalysis described by Ferry et al. (2011), known as "MYO-WP4-PUM-GLOBAL-REANALYSIS-PHYS-001-004", this is a legacy ocean reanalsysis project from the MyOcean project, which was a precursor to the Copernicus Marine Service (CMEMS). This reanalysis provides the climatological ocean forcing used by our CPOM-CICE configuration. This product was a legacy product which has now been removed from the Copernicus website. We use this product because it has been the established ocean forcing for this version of CPOM-CICE and has been used consistently in previous CPOM-CICE studies (Flocco et al., 2010; Tsamados et al., 2013; Schröder et al., 2014, 2019; Williams et al., 2023; Frew et al., 2025). We did not replace it with more recent products such as TOPAZ, GLORYS12 or GREP because CPOM-CICE has been developed with the use of this ocean reanalysis for more than a decade, and is able to produce the intra-annual and inter-annual variability of the Arctic and Antarctic sea ice well using this ocean reanalysis product (Schröder et al., 2019; Frew et al., 2025). We will clarify this in the revised manuscript.

RC: ***L293: WIN-CS2 does not appear to be displayed in Figure 1. Please verify or clarify.***

AR: Apologies for this, WIN-CS2 is not meant to be in this figure but referenced towards Table 2. We will clarify the text in this paragraph.

RC: ***L294: The text refers to a linear fit, but this metric is not reported in Table 2. The table includes a linear regression indicator, which is not described in the methodology or results. In addition, the best-performing metrics are highlighted in bold. Does this also apply to the regression metric? Both the text and Figure 1 suggest that CPOM-CICE provides the best fit, but this is not immediately apparent from Table 2.***

AR: Apologies we have interchangeably used linear fit and linear regression by mistake. We will change the references to linear fit to linear regression and also clearly define the metric in the methodology in the validation metrics section. The best-performing values for all metrics are in bold, yes, however we additionally found a mistake with the regression values in Table 2 (CPOM-CICE should be the best performing) and will update it accordingly.

RC: ***L295: What is the impact of assimilating SITD in addition to SIT? Introducing SITD at this stage tends to blur the main message of the paper. It would be helpful to show SIT distributions for all experiments alongside the AYR observations to better assess the respective impacts.***

AR: We agree that the role of SITD assimilation should be clearer. The assimilation methodology used here is identical to that of Williams et al. (2023), where we showed that assimilating the SIT distribution primarily modifies the thickest two categories of the SITD and only a small effect on the grid-cell mean SIT. In the revised manuscript, we will clarify this point and include the SIT distributions for the relevant experiments to demonstrate the effect directly. This will also help distinguish the impact of SITD assimilation from the effect of the temporal availability of SIT observations, which is the main focus of the present study.

RC: ***L296: The reported correlation and RMSD values appear to vary depending on sea ice thickness categories (e.g. >3 m and <3 m). These differences should be quantified to support the discussion.***

AR: The correlation and RMSD values are not calculated separately for different sea ice thickness categories. Rather, the distinction refers to grid cells with different mean sea ice thicknesses, with 3 m used as the threshold. We will clarify this in the text to avoid confusion.

RC: ***L324: The statement referring to "11 out of 14 cases" is unclear. I am unable to identify how this conclusion is derived from Table 2.***

AR: This refers to the 14 individual metric comparisons shown in Table 2, considering the metrics across the different validation cases. CASIRA-n performs best in 11 of these 14 comparisons. We will clarify how this number is derived in the text.

RC: ***Figure 2: Can the authors provide an explanation for the behaviour of CASIRA-b? The reanalysis shows a substantial positive bias relative to CPOM-CICE before 2010. If this behaviour is related to the assimilation of Bootstrap SIC, this connection should be stated explicitly and discussed when commenting Fig2.***

AR: This behaviour arises from the correlations between sea ice concentration and volume in the assimilation. The Bootstrap SIC dataset generally gives higher sea ice concentrations within the ice pack than the NASA Team product, partly because the Bootstrap algorithm accounts for the effect of melt ponds, which can lead to lower retrieved SIC. Assimilating the higher Bootstrap SIC therefore produces a positive increment in ice volume through the positive SIC-volume correlations in the data assimilation update, leading to an increase in sea ice thickness. This effect is less pronounced when assimilating the NASA Team SIC product. We will explain this behaviour when introducing Figure 2, as suggested.

RC: ***L359: This statement appears to repeat the discussion already provided at L346 and could probably be***

shortened or consolidated.

AR: We will consolidate this into the previous discussion, thanks for your suggestion.

RC: ***L370: Trends have already been discussed in relation to Figure 4. Consider specifying that this paragraph focuses on trends in the seasonal cycle of SIE to avoid repetition.***

AR: We will clarify this in the text, thank you.

RC: ***L377: The discussion refers to years 1988 and 1981, but these years are not clearly identified in the Figure 5 legend. While the 1988 curve can reasonably be inferred from the abrupt January-February change, the 1981 curve is difficult to distinguish. Please indicate more clearly that the 1981 feature is visible in CASIRA-b.***

AR: Thanks, we will indicate this more clearly in the figure and text.

RC: ***L388: The pronounced increase in SIT observed in CASIRA-b, which is also evident in Figure 2, is not sufficiently explained. This behaviour should be discussed when introducing Figure 2, rather than only being mentioned later.***

AR: As per your comment above and the other reviewer, we will explain this behaviour when Figure 2 is introduced more clearly, thanks.

RC: ***L388: Typographical error: "an" instead of the current wording.***

AR: It will be corrected in the revised manuscript, thanks.

RC: ***L405: CPOM-CICE is an model experiment, not a reanalysis. Consider replacing "in all reanalyses" with "in all model experiments" (or equivalent wording).***

AR: Apologies for the poor wording, we will replace wording of this kind with model experiments.

RC: ***L497: The sentence may be missing a verb.***

AR: Apologies, this sentence should say:

"As well as too-thick sea ice in the CAA, but too-thin around the north pole peaking in September, there may be a mismatch in the sea ice transport, ..."

RC: ***L500: Is this interpretation supported by ice transport diagnostics? If such diagnostics are available, they should be presented or referenced to support the argument.***

AR: We agree that this interpretation is not directly supported by an ice transport diagnostic in the present study. This statement was intended as a hypothesis that could explain the spatial pattern seen in the model experiments, rather than as a demonstrated mechanism. We will therefore rephrase the statement to make its speculative nature explicit and avoid implying that the transport contribution has been established from our results.

RC: ***L507: Does the December SIT distribution of CASIRA-n correspond to the SITD signal derived from CryoSat-2? Clarifying this relationship would help interpret the results.***

AR: We thank the reviewer for highlighting this. The broader December SIT distribution in CASIRA-n is broadly consistent with the SIT distribution derived from CryoSat-2, which also indicates a wider range of ice thicknesses and less very thick ice compared with the CPOM-CICE simulation. We have clarified this relationship in the text and now explicitly note that the broader distribution in CASIRA-n is consistent with

the CryoSat-2 observations.

Editor

RC: ***According to iThenticate similarity report, your manuscript contains 34% similarity with existing publications. That is exceptionally high number. Commonly it's 2-3 %. I noticed that most of the similarities arises from the your thesis and your previously published article. That is certainly an issue which you can easily solve by written those parts slightly differently.***

AR: We thank the editor for bringing the similarity report to our attention. We have reviewed the sections identified as having substantial textual overlap with Dr. Williams' thesis and previous publication. We will revise these sections and rephrase the overlapping text where appropriate.

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