Bounded & categorized: targeting data assimilation for sea ice fractional coverage and non-negative quantities in a single column multi-category sea ice model

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Responses to Referee 2

The authors would like to thank the editor and Referee 2 for the time and effort that have gone toward providing feedback on this manuscript. Please find below, in blue, our responses to referee comments, questions, and concerns. All page numbers refer to the revised manuscript.

General Comments

The authors present a framework for hypothesis testing in sea ice data assimilation (DA). Sea ice DA is complicated by the bounds on the sea ice variables, i.e., SIC and SIT should be greater than zero and SIC should be less than or equal to one. The single column sea ice model Icepack and the Data Assimilation Research Testbed (DART) are used. Non-Gaussian error covariances are tested for SIC, SIT, and category-based assimilation.

The paper is well written, relatively easy to understand (given the relatively complicated topic), and definitely deserves publication. I thank the authors for this nice work! I enjoyed reading it.

However, any paper can be improved. Below I have listed a few points that I would like to address. Overall, I rate them as minor revisions.

Author response: We thank Referee 2 for their kind feedback and are glad to hear the paper was an enjoyable read! Our responses to individual comments are included below.

Detailed Comments

I miss a statement in the introduction (and the abstract) that no 'real' observations are used, but perfect model studies, i.e. Observing System Simulation Experiments, are performed. This should be made clear from the start.

Author response: We are grateful to the referee for highlighting this omission! Clarification that the simulations performed for this study are a type of OSSE is a valuable addition to the manuscript that avoids additional confusion about the synthetic nature of the observations. We have added phrasing to the Abstract (Lines 5-7) and to the Section 2 introduction (Lines 86-89) that clarify our motivation for (and use of) the OSSE framework.

Line 76: I would add that DART will be explained in section 2.2.

Author response: References to further discussion of Icepack in section 2.1 and DART in section 2.2 have been added to the manuscript (Lines 83-84).

Line 77: To call Icepack a "single column version of the CICE sea ice model" sounds a bit strange, because the Icepack documentation says "The column physics package of the CICE sea ice model, 'Icepack' ...", i.e. Icepack is not a specific version of CICE, but an integral part of CICE. That might cause confusing as well as calling the data assimilation framework "CICE-SCM-DART".

Author response: We thank the referee for highlighting the potential for confusion here. We have altered the introduction of Icepack from

> *"The data assimilation framework used in this study couples the Data Assimilation Research Testbed (DART, Anderson et al., 2009) to Icepack (Icepack, 2020), a single-column version of the CICE sea ice model; the latter is widely used as the sea ice component of several Earth system models and in stand-alone sea ice studies. In keeping with naming conventions developed in coincident work (Riedel et al., 2023), this framework is referred to as CICE-SCM-DART."*

to

> *"The data assimilation framework used in this study couples the Data Assimilation Research Testbed (DART, Anderson et al., 2009) to Icepack (version 1.3.1, Icepack, 2020), the column-physics package of the CICE sea ice model, which is widely used as the sea ice component of several Earth system models. Icepack can be run in a stand-alone configuration as a sort of single-column model and is reviewed in Section 2.1. DART is discussed in more depth in Section 2.2. In keeping with naming conventions developed in coincident work (Riedel et al., 2023), the collective assimilation system is referred to as CICE-SCM-DART."*

in Lines 80-85.

We have retained the use of CICE-SCM-DART as a name for the framework to be consistent with other work (Riedel et al., 2023) and to reflect our use case of Icepack in its standalone setting.

Line 88: Explain briefly what CAM6 is.

Author response: In Line 88 of the original manuscript, we are referring to an ensemble reconstruction product that was run in CAM6 using DART, not to CAM6 itself. To avoid unnecessary detail or confusion, we have revised this sentence from,

> *"For use in the CICE-SCM-DART framework, 30 instances of Icepack are forced by unique atmospheric conditions extracted from randomly selected members of the CAM6 + DART reanalysis product (Raeder et al., 2021)."*

to

> *"For use in the CICE-SCM-DART framework, 30 instances of Icepack are forced by unique atmospheric conditions extracted from randomly selected members of a recent large-ensemble reanalysis product (Raeder et al., 2021)."*

in Lines 93-94.

A later reference to CAM6 (in relation to models DART can be integrated with) is qualified with explanatory phrasing in Line 116,

> *"... the Community Atmosphere Model (CAM6), the atmosphere component of the CESM2 climate model."*

Line 89: It would help the reader if a little bit more was said about the consequences of "setting the snow grain size parameter to a value of -2". Why does this choice "prevents discontinuous behavior in ice concentration related to ice-albedo feedback during the melt season"?

Author response: Please see our response to a similar comment from Referee 1 (related to Line 90 of the original manuscript).

Line 93: What does "are consistent" mean? I assume it means that the values are the same for all 30 members, right?

Author response: The referee is correct—the ocean forcing values are the same for all 30 members of the sea ice ensemble. For clarity, "*consistent*" has been replaced with "*identical*" in the revised manuscript (Line 103).

Line 126: “The use of bounded normal rank histogram (BNRH) distributions in state-space regression is addressed in (Anderson, 2023)”: I would prefer to read here a few sentences about the main findings of Anderson (2023).

Author response: We have added sentences to this effect to the end of Section 2.2 (Lines 138-141). The paragraph now concludes,

> *“The use of bounded normal rank histogram (BNRH) distributions in state-space regression (step 4) of the QCEFF enforces appropriate bounds by way of a series of transforms in probit and probability integral space. This aspect of the QCEFF also more deftly handles nonlinear relationships between observed quantities and modeled state variables and is addressed in depth for idealized cases in Anderson (2023).”*

Line 145: Table 2 is repeating many information four times (i.e. obs. Kind and obs. Error). I suggest to split the table into two – one naming the experiments and the other describing the obs. Error (which is the same for all experiments).

Author response: Per the referee suggestion, we have split Table 2 into Table 2 and Table 3. The former lists the experiments, and the latter outlines the associated observation uncertainty for each kind of observation.

Table 2: The obs. Error of SIT (10% of SIT value) is unrealistic low at least when compared to obs. Errors from altimetry (see e.g. Figure 2b in https://tc.copernicus.org/articles/11/1607/2017/tc-11-1607-2017.pdf). Any comment on that?

Author response: For a more in-depth discussion of the reasoning behind our definitions for observational uncertainties, including for SIT, please refer to our previous response to a similar comment from Referee 1 (related to Lines 137-142 of the original manuscript). Briefly, our SIT uncertainties are motivated by the anticipation of both improvements to SIT retrieval methods from current products (Landy et al., 2022) and the likelihood of increasingly accurate SIT measurements from current and future observing missions such as ICESat-2 (Petty et al., 2023) and CRISTAL (Kern et al., 2020).

Line 166: I found the sentence “As a result, an observation from any of individual ITD categories is prevented from updating any state-space variable not also in that same ITD category.” difficult to understand. A reformulation of the sentence helped me: "As a result, an observation from any of the

individual ITD categories is prevented from updating any state-space variable that is not also in the same ITD category".

Author response: We thank the referee for this suggestion! We agree that the proposed reformulation is clearer and have replaced the sentence accordingly (Lines 181-182 in the revised manuscript).

Line 174: "CICE rebalancing option" – please explain briefly what that is!

Author response: We have included some clarification around the CICE rebalancing option for postprocessing.

> *"All experiments in Table 2 make use of the CICE rebalancing option."*

now reads,

> *"All experiments in Table 2 make use of this default rebalancing option, which redistributes the ice fractional coverage in each category to ensure that the thickness bounds are respected and then calculates consistent ice and snow volumes, salinities, and enthalpies once the updates have occurred."*

in Lines 188-190.

Line 190: Is NSE not more commonly used as CE? - see https://en.wikipedia.org/wiki/Nash%E2%80%93Sutcliffe_model_efficiency_coefficient

Author response: NSE and CE are definitionally equivalent. CE is a term for the metric commonly used in paleoclimate applications of DA (Klein & Goosse, 2017; Steiger & Hakim, 2014; Brennan & Hakim 2022); most studies using CE in this context cite Nash & Sutcliffe (1970).

Line 204: iCE is to me not more intuitive!

Author response: We have rephrased this sentence to avoid implications of how a reader should engage with iCE (Line 220). We retain the use of iCE because the idealized nature of our experiments means that most of our simulations have a high CE to begin with (i.e. the model with data assimilation is a good predictor of TRUTH); to get a more nuanced picture of how different observation kinds or DA algorithms influence CE, we present the CE improvement (iCE) for each experiment relative to the baseline CE of the FREE case as a predictor of the TRUTH.

Figure 4: I question the usefulness of discussing the snow depth variable in the manuscript. To me it is just a way of diluting the results without learning anything essential.

Author response: This is a valid point, and we thank the referee for raising it. We choose to discuss the snow results in this context for 3 reasons:

a) Snow atop the sea ice is an important part of the sea ice state in the Arctic and plays a critical role in melt and freeze-up processes. We have seen this in our own work with respect the influence of changing snow parameters in the model (see earlier response to comments about *R_snw*).
b) Snow is a key variable in the derivation of sea ice freeboard, which precedes sea ice thickness calculations in most retrieval algorithms. Snow loading estimates in the Arctic are sparse or depend heavily on models however, and there has some idea that assimilating sea ice observations might help constrain snow estimates more accurately. We've shown that this is not likely to be the case, though there may be some small improvement when assimilating category observations (Figure 6, bottom left). This finding emphasizes a need for better snow observations.
c) Coincident work (Riedel et al., 2023) explores the impact of assimilating SIT, SIC, and SND observations in a perfect model OSSE context in the CICE-SCM-DART framework. The inclusion of the snow results here provides some nice additional context for that work.

Line 265: ",the dependency". White space is missing!

Author response: The whitespace has been added (Lines 286-287).

Line 290: It is true that SIT summer observations were missing (at least space filling) , but I would point to the forthcoming data products (e.g. https://www.nature.com/articles/s41586-022-05058-5).

Author response: We thank the referee for highlighting recent advances in SIT retrievals. The potential to have and to assimilated summertime SIT measurements is an exciting prospect, and one we are eager to explore in future work! However, we also anticipate that there may be applications for sea ice DA outside the 2011-2021 period for which the Landy et al. (2022) product is currently available, as well as for purposes of comparison and validation.

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