

Authors response to second review of "Sensitivity of iceberg drift and deterioration simulations to input data from different oceans sea ice and atmosphere models in the Barents Sea" by Herrmannsdörfer et al.

(Review in black text colour and authors response in blue text colour)

The reviewers have complied to my main objections on the previous iteration (merging the two parts of the paper), provided new figures and improved the English grammar. Some other suggestions or criticism have not been taken up and - since no point-by-point rebuttal has been provided - I ignore why the authors disagreed with me. Some of my comments below will therefore be repeated from the first review, but I do not see any other solution.

I am however positive that the article is almost ready for publication in The Cryosphere, with the exception of minor requests listed below: There remains some repetitions in the text that the authors should chase for the sake of conciseness and the use of "geostrophic currents from Slagstad et al. (1990)" seems to be a misunderstanding. Also parts of the Appendices may be superfluous if the iceberg model is mostly unchanged from the literature.

The authors appreciate that the applied changes are positively recognised and that the manuscript is considered almost ready for publication. We apologise that the uploaded response document (in the interactive discussion section "AC2: Reply on RC2") did not explain and describe the changes detailed enough. We accept the suggestions of the reviewer listed below and make sure that all changes (minor comments etc.) are documented thoroughly in this review round.

Comments on the appendix:

- I am not convinced that the Appendices A2 to A4 are necessary if the model is identical to that of Monteban et al. 2020. If there are only a few modifications in the model, it would be sufficient to indicate these modifications in the main text. Appendix A1 could be merged in the main text where the sentences are already repeated.

We follow your suggestion to remove the appendix. For a description of the iceberg model, we refer to Monteban (2020). Only little changes in the methods section are necessary:

- L104 is changed to "The drift and deterioration equations and model parameters can be found in \citep{Monteban2020}."
- Delete L90 "~~More details on the seeding approach are given in the Appendix (Sect. \ref{sec:appendix}).~~"
- Change in L81 "Following the seeding procedure in \citep{Monteban2020}, icebergs are seeded near the tidewater glaciers of Franz-Josef-Land,..."
- L68 is removed "~~A detailed description of the iceberg seeding, model setup, drift and deterioration equations, parameters, and computational routines is provided in the Appendix (Sect. \ref{sec:appendix}).~~"

Aggregated comments about the usage of geostrophic currents and Slagstad data:

- l.94: I believe this sentence is incorrect: The geostrophic currents result from the equilibrium between the Coriolis force and the pressure force (down the slope of the surface heights, called pressure term F_p in Appendix A3). The geostrophic currents are therefore perpendicular to the slope of the sea surface heights.

- L.117: The currents in TOPAZ or Barents-2.5 already include geostrophic currents. By elimination I believe that Slagstad et al. (1990) provide the expression of the force exerted by the sea surface slope (also called pressure force). See comment on appendix A3 further below.

- Formula A10 is nowhere to be found in Slagstad et al. (1990) and geostrophic currents are not mentioned in there. The authors should indicate the actual source of geostrophic currents, if any. I do not even understand why an extra source of geostrophic currents is needed since the ROMS and HYCOM models do include these currents by construction.

- L.556: As repeated from my previous review, Slagstad et al. (1990) simulated a period in 1988, but not the period of this study (2010-2021). If these model results are used in this study, they should be compared with the TOPAZ and Barents2.5 results.

Thank you for the comment. Precise notation is very much appreciated here.

You are right about Topaz and Barents-2.5 already including the geostrophic currents. The geostrophic current data is from Slagstad. The data is a time averaged. Changing the model setup regarding this term is however not the interest of this study, as it is considered part of the iceberg model. This also applies to comparing the geostrophic current data to the current data from Topaz and Barents-2.5. We do not face this kind of problems in more sophisticated iceberg models and therefore a detailed analysis is not of value to the research community.

The equation is taken from Stern (2016). Following, L596 could be changed to “The pressure gradient force is approximated with the “Coriolis-related term” \citep{Stern2016} assuming geostrophic balance and using average geostrophic water velocity from \cite{Slagstad1990}.”

As the detailed description of the iceberg model is deleted with the appendix, the term and the slagstad data is explained in detail and the reader is forwarded to Monteban (2020). We try to give an overview over the term in L94. L.94 is changed to “The pressure gradient and Coriolis forces are included under the assumption of geostrophic balance.” We delete L117 as it is described in Monteban (2020).

Detailed comments:

- Abstract L.14: add "however" or another logical conjunction to indicate the contrast with the previous sentences.

“However” was added to highlight the contrast in the sentences.

- L.42-44: the ideas of this sentence come in disorder.

We change the order and split the sentence for improved readability:

Previous version: “These environmental models describe the highly complex interaction of ocean and atmosphere of the Barents Sea due to its complex bathymetry and position between warm Atlantic waters and the cold Arctic Ocean with different resolution, model physics and representativity of the domain.”

New version:” These environmental models describe the highly complex interaction of ocean and atmosphere of the Barents Sea with different resolution, model physics and representativity of the domain. The complexity of the Barents Sea arises, among other factors, from its varied bathymetry and position between warm Atlantic waters and the cold Arctic Ocean.”

- L.71: The selection of data sources is very much focused on Norway / Europe. It would be fair to acknowledge that there are many other valid reanalyses that could have been used instead but

have not.

This is a very good point that should be mentioned.

We added following sentence in l.42: "More suitable models exist, that are not considered in this study, but could be examined in future research."

We also added following to l.553 (Conclusions): "We note that the choice in ocean, sea ice and atmosphere models exhibits a focus on European and Norwegian models, which is motivated by the location of the study area in the European Arctic and the accessibility to research purposes. We highlight the opportunity to extend this study using a larger number of suitable ocean, sea ice and atmosphere models that are disregarded in this study."

- l.91-92: This sentence is repeated from the introduction.

The sentence is varied to "The numerical model for the simulation of iceberg drift and deterioration and its settings is adopted from Monteban (2020)."

- l.97: h_{min} is coming from the sea ice strength in the EVP rheology, this should be explained in the text.

We add following explanatory sentence in l.98: "Note that heavy sea ice is assumed under the simultaneous occurrence of high CI and sufficient sea ice strength. Sea ice strength is described by the thickness threshold h_{min} , which is derived from an empirical relation of CI (Lichey and Hellmer, 2001)."

Also we moved the sentences about the sea ice categories in front of the explanation of the melt processes.

- l.98: "as" is falsely implying that there is no swell in the area. Rather make the assumption that the swell has little effect on the erosion of small icebergs.

We clarified the statement as follows: "The wave erosion term disregards swell waves due to simulated small iceberg sizes but includes local wind waves as function of wind and water velocity."

- l.106: "Assimilates" should rather be replaced by "is driven by" as no data assimilation is performed in this study. Similarly in l.109 and 110.

"Assimilate" is replaced by "driven by" and "taken from".

- l.137: The sea ice edge is not identical in Barents-2.5 and Topaz. Which of the two is used here?

The average sea ice edge of both models are depicted in the Figure. The CI difference is calculated where one or both models show $CI > 15\%$ at any point of the study time. We changed l. 137 to following: "...within the maximum modelled sea ice extent, including areas where sea ice is present only in one of the models and during limited time periods."

- Table 2: The 10m winds appear twice in the table, both in the ocean and in the atmospheric models, with different values and with the opposite difference between ERA5 and CARRA. Why? Are they computed on different domains?

and

- Figure 2: Specify whether the differences are TOPAZ-Barents and ERA5-CARRA, consistently with Table 2, or the opposite.

Wind speed differences are computed on different subsets of the domain and time period. Wind speed differences are calculated along all trajectories using ERA5 (Combinations Topaz-ERA5, Barents-ERA5) and CARRA (Combinations Topaz-CARRA, Barents-CARRA) (Table2, atmospheric models). This shows the difference due to the wind input. The wind speed difference is also

calculated along all Topaz trajectories (Combinations: Topaz-ERA5, Topaz-CARRA) and Barents-trajectories (Combinations: Barents-ERA5, Barents-CARRA) (Table 2, ocean models). This shows how the wind varies, not due to the wind input, but due to trajectory difference due to ocean and sea ice input.

To explain the difference more thoroughly we add and change following:

- Add in Fig2 caption: "The values for v_a are given for the iceberg pathways as simulated by different ocean and sea ice (a) or atmospheric (b) input." And added a,b in the Table.

- change l.171 to "...and varies just as much between the two atmospheric models as between the pathways caused by varied ocean and sea ice input."

- l.157 throws many issues that may cause low predictive skills without explanations. The authors should either explain or remove this sentence altogether.

We agree to remove any explanation for low predictive skills, as it is outside of the scope of the study and the authors' experience. We change the sentence to "Model skill varies over time and spatial scales and the predictive skill for surface water speed and direction in Barents-2.5 is low. Some skill can be."

- l.180. Should we expect that other models exhibit larger differences?

Yes, we may expect larger differences for models that are in no way connected. However, that does not exclude the possibility that some "un-connected" models show high similarity.

We add the sentence "Unrelated atmospheric models may exhibit larger differences."

- l.200: Is 62 tons the mass loss for one iceberg? From which initial mass?

62 tons is the average massloss for a iceberg from simulation start to end. The initial mass varies strongly with random iceberg size seeding. Iceberg size seeding is reproduced exactly for simulations with different environmental input (e.g. Barents), meaning that the deterioration difference is caused solely due to the environmental input . We change l.200 to "The average iceberg mass loss (from simulation start to end) is larger in simulations with Topaz input (+6.2 10^4 kg)."

- l.267: Simulated icebergs are seeded from July to December. How realistic is that model setting? A sentence or a reference would be welcome.

This choice is based on the assumption that the termini of the glaciers are surrounded by sea ice (and icebergs are locked in the sea ice) for the rest of the year. This might be realistic for the icebergs originating from Franz-Josef-Land, but less realistic for Svalbard and Novaja Zemlja.

We inherited this assumption as part of the seeding settings from Monteban (2020) who based the seeding settings on a large number of satellite observations he made.

- l.310-311: I don't understand this sentence, nor what it implies for the results. Are there more differences of winds because the changes of currents steer the icebergs onto different trajectories than the change of wind forcings fields themselves?

Precisely! This is the same as in Table 2, but this time for the example of one iceberg. This implies for the results that varied atmospheric input has low impact on the iceberg simulation results (compared to ocean/sea ice input).

We change L310-311 for increased understandability: "Further analysis (not shown) obtained that the wind speed varies less between the atmospheric input than between the trajectories caused by varied ocean input."

- l.319: "Further analysis"... Unfinished sentence.

Thank you. We delete the phrase.

- Figure 8 is missing the colour scales for temperature. It is also a busy (too busy) figure so the empty star is hard to find and I could not see the ice edge in the right column. You could use a full star on all panels (no risk of confusing the stars), remove the bathymetry, remove wind and currents on heavy sea ice (since they are not acting anyway). This should improve the clarity.

Thank you for those suggestions. Following changes are made:

- “Empty stars” are replaced by filled black stars.
- The sea ice edge in the right column was displayed in a dotted style and is changed to a line style.
- The colour scale for the temperatures is added.
- The bathymetry was added on request in the first review round, but is removed here for improved visibility.
- Wind and current arrows are removed over heavy sea ice.
- For consistency, the similar hatches for heavy sea ice are used in both columns.
- The legend and caption are changed accordingly.

- l.374: "need to be viewed in the light of low reliability of the data". Did you mean "are more dubious/uncertain"?

Yes. “Low reliability” is changes to “large uncertainty”.

- l.422: Unclear sentence. Is this the same idea as in l.310-311? I believe it can be removed without loss of information or logic.

Yes, it is the same idea as in l310 and is therefore removed.

- Table A1: Indicate units for location and scale (meters?).

Thank you. I added the unit, which is in this case meters.

References and typos

- l.14: "inputs" plural because there are ocean, sea ice and atmospheric terms. Correct, changed in the reviewed version of the manuscript.

- l.42: Schyberg et al. Is missing a year. Added year in reference.

- l.47: "related to" could be replaced by "extracted from". Replaced by “Extracted from”.

- l.47: what is a "composite of knowledge"? “Composite of knowledge” replaced by “Combinations of knowledge” . This refers to the combination of knowledge from previous quality reports and analysis done in this study.

- l.57: is missing a closing parenthesis. “)” added.

- l.60: "analysing on" -> "an analysis of" Changed to “We emphasise that this study focuses on the impact of the choice of environmental input data on iceberg statistics rather than an analyses of the absolute iceberg statistics.”

- l.65: "assess" -> "to assess" Changed.

- l.131: There are two papers "Röhrs et al. 2023", please number them by a/b for disambiguation. Added “b” in notation of second paper.

- Table 1: TOPAZ temporal resolution, remove "to monthly" since only daily files are used in this study. [Changed.](#)
- Table 1 could also mention that TOPAZ data are ensemble averages of 100 members. [Thank you for this information. Added to table.](#)
- L.150 use "faster" rather than "larger" for velocities. Also in L.154. [Changed.](#)
- L.152 Missing space at beginning of sentence. [Space added.](#)
- Table 2: What does the $\emptyset$ symbol mean? An average? [Yes. Added “ \$\emptyset\$ denotes the variable average” in Table caption.](#)
- L.158: "the speeds" of currents or sea ice drift? [Added “water and sea ice” for clarifying which speeds are ment.](#)
- L.161: "it's" -> "its". [Changed.](#)
- L.168: "varies just as much as in the pathway differences by the the ocean and sea ice input". Unclear sentence, can you rephrase it? [Was changed to “.varies just as much between the two atmospheric models as between the pathways caused by varied ocean and sea ice input.”](#)
- L.204: "daysduration shorter" -> days shorter. [Changed.](#)
- L.224: "every occurring i" -> "every iceberg i". [Changed.](#)
- L.243: "in father" remove "in". [Changed.](#)
- L.260: "adapt" -> "adopt"? [Changed to “We adopt and modify the definition of the iceberg extension from Kegnouche \(2010\) and show the relative number.”](#)
- L.298: use "warmer" rather than "larger" for temperatures. [Changed.](#)
- Caption of Figure 8, 5th line, the closing parenthesis should be before "resolution", not after. [Changed.](#)
- L.492: Missing REF. [Reference added.](#)
- L.503: "the year of 2010 to 2014" -> "the years from 2010 to 2014". [Changed.](#)
- L.504: "constriction" -> "constrain" [Changed.](#)
- L.507: "the basis" -> "assimilated" for once that this is the correct term. [Changed to “...although these are assimilated into the sea ice models already \(e.g. Topaz, Barents-2.5 Forecast\).”](#)
- Equations A15, A16 and A17 should indicate units. [Added “The melt terms M are given in m s⁻¹.”](#)
- L.632: "Xie J." -> "Xie". [Changed.](#)
- L.647: Indicate co-authors of Giusti et al., consistently with the journal standards for citations. Also punctuation error "reanalysis.," [Corrected punctuation. Changed author to “ECMWF” as current version of document can no longer be traced back to “person” author.](#)
- L.663: Is there an update of "Idzanovic et al. in progress, 2024"? [The authors of the report confirmed that there is no updated or published version of the report.](#)

- l.674: Apparent mistake in the authors list with "t". "T., and Wang, Z..." is a continuation of the reference "Schyberg et al. 2023". The print-ready version of the manuscript will have different page breaks, eliminating the problem.