Response to review of "Sensitivity of iceberg drift and deterioration simulations to input data from different ocean, sea ice and atmosphere models in the Barents Sea (Part II)" by Herrmannsdörfer et al.

In the following, see numbered Reviewers comments () and Authors response [].

General Reviewer comments:

This is the second of a two-part manuscript. I have reviewed Part I and recommended it to be merged into Part II. (1) The Part II paper is the main body of results from the study, presenting Lagrangian iceberg simulations. Similarly to Part I, the focus of the paper is to exhibit the differences between iceberg simulations when switching one source of ice-ocean or atmospheric input data, but without an indication of the respective accuracy of each data sources.

(2) Part II simulates synthetic icebergs from their calving location until they are mostly melted, not observed icebergs. One example of simulated trajectory is selected and discussed in more details. This is an important information that should already be given upfront in the abstract to set the scene of the study.

(3) The dependency of Part II upon Part I is less strong than I originally thought. I have only counted three facts that could be included in this manuscript in replacement of some overly long technical parts (see below and see the Part I review).

(4) The model description is given in Annexes, although it is not clear whether the model is identical to the previous Monteban et al. study or if it has been modified. Either way, the authors should reconsider if the annexes need to repeat the previous paper or simply refer to it.

(5) The results are novel and interesting, and all the more important that the related literature is now quite outdated. However the Part II paper often lack a reflection on previously published results. This aspect should be strengthened.

(6) The discussion of the results could be reorganised to set the higher priorities first: if the location of the sea ice edge is the largest cause of divergence between the iceberg trajectories, then the authors could argue that the efficient assimilation of satellite sea ice concentrations is the priority issue when simulating icebergs and down-prioritise the technical issues on the search radius.

(7) The conclusion lack practical recommendations for other practitioners of iceberg modeling. Seeing that the statistics are "surprisingly similar" for the Topaz and the Barents-2.5 models, but knowing that fewer years of reanalysis are available from the Barents-2.5 model, I would be tempted to use the Topaz reanalysis all along and use the 30 years of data for the sake of statistical significance. Further, I would expect the conclusion to bring up the following topics: how important is the duration of the reanalysis, the consistency of the input data (with the example of the modifications of the Barents-2.5 model), the differences from / the commonalities with previously published iceberg simulation studies.

(8) The language should be improved with a better choice of vocabulary, logical transitions, and punctuation.

(9) Part II overall represents an interesting and valuable contribution to the field, although some improvements would be necessary, and I can recommend it publications after major revisions.

Author's response to general comments:

The authors thank the reviewer so much for the thorough and constructive feedback and for valuing our results. We think that we can improve the quality of the final product significantly with the help of the provided comments, hopefully leading to a successful publication. Based on the comments, we plan following major revisions to the manuscript:

- Although we put a lot of effort into part 1, we agree (after a lot of thinking) that merging part 1 and part2 into one paper will lead to a better overall product. (See reviewer's comments 1,3)
- We will re-structure the manuscript attempting to improve conciseness and coherence. Certain parts will be shortened (e.g. Section 2.5). (4,6) , and we will consider to arrange the sections of the manuscript by priority, especially the discussion (e.g. Section 4.1). (6)
- We will set the scene more clearly about what we analyse and what the limitations are. For example, we will mention that we do not include iceberg observations and that we do not analyse the impact of the iceberg model settings. (2,4)
- We agree to add a reflection on previously published results (5).
- We will add a discussion on the advantages and disadvantages of the different input datasets (e.g. temporal availability, consistency and resolution of environmental data) and the potential use for different applications (e.g. long term statistics or short-time forecast of individual trajectories). Therein, we should highlight how the choice of forcing input depends on the simulation goal. We will also investigate the projectability of our results on other studies. This shall serve as much-needed input to make educated decisions on forcing input for iceberg simulations. However, we cannot provide a generalised practical recommendation on which environmental data works best as forcing to iceberg simulations, as the suitability is highly sensitive to the application, region and time. (7)
- We will carefully consider whether or not to remove the annex (4)
- We will pay more attention to the language and we will certainly improve it. (8)

Furthermore, we plan to change following:

- We will improve the description of similarities and differences to Monteban (2020)'s iceberg model setup. (4)
- We will discuss the potential for improvement by assimilating satellite-based sea ice concentration products. (6)

Authors response to detailed reviewer's comments:
Detailed reviewer's comments that we comply to with response:

- l6: "are sensitive". I would expect the abstract to indicate a degree of sensitivity, it is very or moderately sensitive? [10] Although challenging to quantitatively measure the sensitivity, we will try to indicate at least subjectively the "degree of sensitivity" as suggested.

- l12: "surprising similarities". A word of explanation is necessary in an abstract: why are they so similar? Otherwise the reader is left on an apparent contradiction with the sentence on the sensitivity of the model. [11] We will rephrase this considering your comment.

- l41: the multiplication 4.2603.7 is mysterious, but the exact number is not necessary in an introduction.
- l62: Explain why 2603*7 or remove if the exact number is not important yet.
[12] The exact number is not important. We mentioned the numbers to express that we did a statistical analysis with a reasonably large number of samples, that is typical for the region We will change this in the manuscript (in e.g L41, L62 and L537)

- l71: Why do you need geostrophic currents when the models provide surface currents? And do Slagstad et al 1990 provide data relevant for your period or only the general formula for geostrophy?
- l92: How the pressure gradient force relates to geostrophic currents is not clear. Do the authors mean to account for the sea surface slope, with a force downslope rather than turning around the positive/negative anomalies of sea level? In this case, which data is used for the sea surface heights? The reader should not have to read Slagstad et al. 1990 to understand this sentence.
- l570: I was hoping to understand what "pressure gradient" is meant in the Annex, but to no avail. The confusion between ocean currents and geostrophic currents is still puzzling and the notation Fc,p rather indicates that the pressure gradient is the same as the Coriolis force.
[14] We inherited the model setup including drift equations from Monteban (2020). We found a typo in Equation A10. We correct the equation to: $F_{c,p} = m \cdot f \cdot [v - v_{geo};- u - u_{geo}]$. It is expressing Coriolis and pressure gradient force in iceberg drift simulations in one term. The idea is to subtract the geostrophic velocity of the ocean for geostrophic balance to receive the "Coriolis-related term" (Gladstone 2001 and used by Stern 2016, Monteban 2020). Slagstad (1990) only provides a general estimate. Nowadays data availability allows for including the sea surface height and slope more accurately. However, we do not intend to study the impact of the model setup

- l79: "Gaps of [...] few days", how many days? Winds are unlikely to persist for more than two days in the Barents Sea. [15] *"gaps on the scale of hours to 7 days".*

- Section 2.5: Distinguish assimilation from forcing.
- l44: Forcing a model is not data assimilation (systematic confusion, also commented in Part I).
[16] We are aware of the difference between assimilation and forcing. In the context of this work, we use the terms:

- Assimilation/to assimilate: The iceberg model takes in input data (with a certain method).
- Forcing- variables, data, data set (sometimes just "forcing"): Input data
- Forcing fields: 2d/3d/4d input data
- (nearest) forcing (grid) cell: Grid cell of the input data, that is closest to the current iceberg position. It is used as part of the assimilation method.

We did not find any other terms containing "assimilation" or "forcing" in Section 2.5 and did not find mix-ups between those terms. Nevertheless, we will pay extra attention to these definitions and try to avoid any confusion in the revised manuscript.

- l107: "for one time step": Time should be irrelevant for spatial interpolation. [17] "Timestep" refers to iceberg simulation time steps and its position. We will change to: *"Note that the environmental data sets have different grids, so that the forcing data for the same iceberg position is not raised from the same area."*

- Section 2.5 has too much details, and Figure 1 is not necessary since the ocean currents are notably inaccurate near the coast, as noted by the authors much later. The takeaway from that

section is that inputs near the coast are fetched from a nearest neighbour, which can be several grid cells aways. As a reader I am ready to accept that there are too many uncertainties near the coast and that as long as icebergs are seeded to the ocean, the following simulation is valid.
- Section 3.1 is similarly too detailed and Figure 2 can be removed without affecting the following results.
[19] We plan to shorten Section 3.1, 4.1 and 2.5 to the key-message to enhance the readability of the paper.

- Table 2: Why not indicate total mass loss? The relative contributions can be misleading when one model melts icebergs much faster than the other. [18] We can include total mass loss mentioned in line 189 into Table 2. The relative contributions can certainly be confusing, but they also help to explain why and how the total mass loss varies. Thus, we will keep the relative contributions, unless strongly opposed.

- l179: Is M_fb the same as M_fw? [20] Yes, we will correct the typo.

- l201: What are "seeding characteristics"? The number or the size of icebergs seeded? An explanation why icebergs from some glaciers last longer than others would be interesting. [21] We add “seeding characteristics (number and size)…” in L201. We also add to the discussion 4.2 that icebergs from Franz-Josef-land last longer due to their larger seeding size and frequent presence of sea ice around the archipelago, while icebergs from Novaja Zemlja and Edgeøya are initialised with smaller size and in warmer, mostly sea ice-free waters.

- l210: that the drift distance is longer with Topaz is surprising, I would have expected that the tidal loops would have made them longer with Barents2.5. [29] We also found it surprising and could explain the behaviour for those sources.

- l223: In which projection is defined the output grid? [22] The output grid for the iceberg density resembles a curvi-linear Topaz grid at reduced resolution. This will be added in L223.

- l230: Are there other results in the literature to support these results? [23] We plan to relate the results to previous findings. However, we should not set focus on it, as we do not study absolute iceberg density, but the differences in density due to varied forcing.

- Figure 4 is too small for the paper version, but by blowing up on screen, the Barents2.5 shows more fine features than Topaz, which may be related to topographically steered currents. A few isolines of the topography (50 m or 200 m) may help reading this map. If Figure 4 is made bigger and more readable, Figure 5 can be removed as it seems redundant. [24] We agree that Figure 4 needs to be enlarged and that bathymetry isolines would help in the interpretation. However, we strongly advocate to keep Figure 5, as it i) contains some of the key-findings of this study and ii) makes the comparison of densities significantly easier than comparing the maps in Figure 4.

- Section 3.5: The notion of iceberg extent was introduced before (Keghouche et al. 2010, possibly earlier), is the same definition applied here?
- l448: About iceberg extension and environment forcing, Keghouche et al. 2010 did show the relationship between iceberg extent and the wind patterns. Does that relationship still hold in your study?
[25] Keghouche (2010) was certainly an inspiration for this study. We should mention Keghouche (2010) and relate our findings with the described dynamics and thermodynamics of interannual variability in iceberg extension. However, the studies are only comparable to a limited extend due to different focus of the analyses.

- Table 4: Sea water surface velocity is the norm or the v-component of the velocity?
- Table 4, the column named v_ai does not correspond to the caption. It seems the water and sea ice velocity columns have the wrong name.
[26] Table 4 provides the norm (the mean total sea surface velocity, that the iceberg experienced along its trajectory). The same is true for wind and sea ice drift. The variable notation is introduced in Section 2.2. We add “total [...] speed” in the caption to Table 4. The typo “v_ai” will be corrected to “v_si”.

- Table 5: Why are there two numbers in the last three columns? [27] The first number of each column shows the values along the entire trajectory, while the second number shows the values for all times with relevant sea ice. As the second numbers are not used in this version of the manuscript, we remove them.

- Figure 9 panel a) add the horizontal line at 0 degrees to indicate the melting temperature of glacial ice. Panels d-e-f are not very informative as they show the velocity modulus but not the direction. Feather plots could be more intuitive. [28] We agree to add the 0degree line to panel a). We think that panels d-f are valuable, as iceberg drift and deterioration largely depend on those speeds. We wanted to show the different temporal variability in the different forcing data, due to their temporal resolution and e.g. tides. We did not add feathers, as the temporal variability is too high. We did not plot the direction as line plot or plot u and v component separately, as we did not use any directional information in this version of the manuscript.

- Same figure: the "delta" and "abs" on the vertical axes are mysterious, I think they can be removed. [30] The naming conventions relate to the ones used in the Table. We exchange a “delta” with “δ” (as used in Table) and exchange “abs” (==absolute) with the unit (10^5 kg2h−1). This should make the reader understand, that we no longer describe relative contributions, but absolute mass loss.

- l330 sounds rather dramatic about the data availability near the coast, but pragmatically the detailed conditions near the calving front are very uncertain anyway. Iceberg simulations may be stuck near the calving front (and need an initial push as you do with data interpolation) but as soon as they have escaped, the iceberg trajectory is valid.
- l333-340: Similarly, these considerations seem out of place, unless there is an implicit aspect that I am missing. From the results presented above, the resolution is of lesser concern than the sea ice edge disagreements between the models. The discussion could thus be shortened if it was restructured to highlight the mot important topics.
[31] The icebergs are seeded with a distance to the glacier front in which forcing data is available. However, icebergs may drift close to coastline and loose forcing information in the course of their lifetime. We found, that this happens more often for Topaz forcing due to its larger grid cell size. Due to the larger grid cell size, Topaz also provides uncertain information further into the sea, than Barents-2.5. The coarser land-sea-mask in Topaz further increases this effect. This considerations are only important along the coastlines and regions with complex bathymetry/topography. We agree that the described effects are less important than the sea ice representation and agree to shorten the section and shift it to a later point of the discussion. We also express more clearly where this effect is relevant while also stressing that the forcing is highly uncertain along the coastline anyways.

- l365: Although I expect this sentence to be correct, I fail to see this increased deterioration in May-June in Figure 9h. [32] We will double-check if Figure 9h is plotted correctly and change the

text if necessary. We should also mention that the deterioration rate (in kg) decreases with decreasing iceberg size.

- l372: The difference of temperature between Barents-2.5 and Topaz only comes here. The differences in sea ice variables come further on. This means that the relevant elements of the Part I paper can be inserted before section 4.2. [33] While it is true that the comparison of Topaz and Barents2.5 is mentioned first in those lines, we think that the comparison would be better integrated in another place for the sake of the reading flow (See reponse [1]).

- l375: The Barents-2.5 may well have too cold SST, but a comparison to satellite data would hammer the facts. [34] Including a comparison to satellite data is entirely out of the scope of the "merged" manuscript. Therefore, we use known error from e.g. Röhrs (2023) (see response [1c]).

- l381: The tidal loops should be mentioned earlier when the datasets are introduced. I cannot see tidal loops in Figure 7, are they too small for the figure or are the tides small in that area? [35] We will mention the representation of tides in the comparison of Topaz and Barents2.5 that will be adapted from part1. Iceberg 2013-788 is influenced by tides, as shown in the oscillation of the Barents2.5 sea water speed along the trajectory in Figure 9d. However, the tidal forcing along the trajectory forced by Barents2.5 seems to be too small to be visible on the scale of Figure 7. Other iceberg trajectories showed obvious tidal loops, but we decided to not include more examples.

- l385: The tidal component is essential for iceberg density and extent. This statement is not obvious from the figures, nor followed up in the rest of the paper. Could it be elaborated? [36] We should add that the tidal looping causes the iceberg to spend more time in the region and ii) thereby keeps the iceberg from drifting into warmer waters, which increases the drift duration and ii) increasing the local iceberg density (more timesteps with icebergs in density-grid-cell). We should elaborate in l385: *"One might conclude that the tidal component is not essential to where icebergs mainly drift in the Barents Sea (main pathways), however we found that it is essential to simulating individual iceberg trajectories, how many icebergs drift in different regions of the domain (iceberg density)."*

- l391-394: I miss the whole idea of this paragraph. Please rephrase to clarify. [37] The idea of this paragraph is to explain that regional differences (e.g. between Franz-Josef-Land and Hopen Trench) dominate ocean mode differences (e.g. SST difference in Barents2.5 and Topaz). We conclude that the ocean models capture the regional differences despite their differences, which is an important message for the choice of forcing input for iceberg simulations. We will rephrase the paragraph.

- l408: This sentence is inconclusive about the mobility of icebergs near the calving glaciers, their grounding and data interpolation near the coast. Keghouche et al. 2010 computed the incidences of grounding, could the authors compute a similar map or a blowout of the averaged current vectors near a calving front to discriminate which effect is most important? [38] See answer [31], where we describe that icebergs did not ground close to their seeding location, but in their later drift. We, in general, observed little grounding events in comparison to timesteps in which forcing was not available (in the nearest forcing cell). Grounding events may have been more important in Keghouche (2010), due to different iceberg seeding sizes and potentially smaller melt rates due to different sea ice, ocean and atmospheric properties during the years 1985 to 2005.

- l425: "might increase": this sentence is blue sky to me. More sea ice means less melting or wave erosion, and additional sea ice stress, so the authors could be more assertive about the difference between the models. Also mention which model is more realistic. [39] We rephrase: "The more extensive sea ice cover over the Spitsbergen Bank in Barents-2.5, and thereby reduced melt rate by wave erosion and buoyant convection, increases the number of icebergs drifting as far south as Hopen Trench." As we did not find any previous studies comparing Topaz and Barents2.5 (to the same set of observations), we cannot say which model is more realistic with confidence. However, we try to mention weaknesses and strengths of both ocean models that may help in the judgement of which forcing works better in specific applications of iceberg simulations.

- l430-431: This sentence is too complex. Do you mean that higher resolution winds follow better the orography? [40] We wanted to say that higher resolution wind is more accurate over complex orography. We found that the addressed sentence is a repetition and therefore remove it.

- l432: The coarse resolution currents are extrapolated near the coast, irrespective if the currents are on-shore or off-shore, so I don't understand why the icebergs cannot drift close to the coast in Topaz. [41] We think that the addressed sentence creates more confusion than it adds information and remove it.

- l445: "iceberg drifted": it is singular because there was only one iceberg? [41] It should be plural ("icebergs").

- l451: I don't understand what is meant by varied forcing reproducing the variability. Rephrase. [43] "A similar multi-year variability of iceberg extension is simulated independent of the forcing input."

- l466: This idea is repeated. [44] We remove addressed repetition.

l470: Has the influence of the seeding mechanism been accounted for in the presented results or only mentioned as a warning to the readers? [45] The influence of the seeding mechanism has been accounted for in the discussion, but is still present in Figure 6. We remove the sentence to avoid confusion.

- l472-477: It is well known that Lagrangian trajectories diverge over time (there is a vast body of literature about dispersion in the ocean surface since Okubo 1971, see also Koszalka et al. 2009 for a geographically closer example), the first part of the paragraph is correct, but the end is unrelated: one single example of iceberg trajectory does not say much over averaged statistics (See Figure 4 for example).
- l475: "Similar initial conditions". Are they similar or strictly identical?
- l479: Use a more precise vocabulary: The iceberg trajectories diverge.
[46] We will highlight that the example trajectory is only presented for illustrative and explanatory purposes and that it cannot be used to make broad conclusions. We should also make sure that the reader is not confused into thinking we (wrongly) based our conclusions on known divergence of Lagrangian trajectories. We will change to" *The sensitivity of iceberg simulations to their environmental forcing is illustrated by the example of iceberg 2013-788. The example demonstrates how identical initial conditions and small deviations in the environmental forcing can still diverge (causing further deviation in forcing). Different drift trajectories ultimately lead to different potential exposure of structures and ships to icebergs."*

- l483: Here again, the end of the paragraph seems unconnected from the preceding argument. [47] *".. This also highlights the importance of temporal and horizontal resolution of the forcing data. Due to its high horizontal and temporal resolution, the use of Barents-2.5 may be beneficial in iceberg simulations, compared to the lower resolution Topaz data."*

- l496: "can be seen", are you referring to Figure 9? [48] Yes, we add "in Figure 9" in l 486.

- l489: Sentence unclear. It seems to be rephrasing the same idea, but with unclear words ("derive"). [49] We change to: "This illustrates how the sensitivity of iceberg simulations to sea ice forcing is given by the large impact on drift and deterioration and the large occurrence of sea ice in the iceberg pathways.

- l499: You found that the results were not so sensitive to the change of atmosphere reanalysis, do not state the opposite in the conclusions.
- l500: Surprising similarities: Please elaborate on how the differences in the forcings cancel out in the final statistics.
[50] We want to communicate the small difference due to the atmospheric forcing that we found (e.g. in iceberg densities). We don't want to go into detail in the introductory paragraph of the conclusion. The similarities are discussed in l521-526. We change to: *"We found that the results of such simulations are sensitive to the input from ocean, sea ice and atmosphere reanalyses or forecasts. However, the extend of the impact varied with the environmental parameter and the iceberg characteristics.* ~~*The study exhibited both small forcing differences leading to large differences in iceberg trajectories and surprising similarities in the statistics, despite large forcing differences."*~~

- l502: This statement is contrary to previous statements that average sea ice thickness and sea surface temperatures cause most of the differences, not so much the resolution of the data. The resolution only seems to be an issue near the coast.
- l503: the enumeration lacks logic. This paragraph is restating previous results instead of concluding on the take-home message.
[51] We agree that we should be more precise but also conclusive in this paragraph. We did not intend to say that the resolution has larger impact than e.g. the sea ice concentration. We change the paragraph to the below and shift it further back (e.g. in front of the similarities paragraph): *"Spatial and temporal resolution of the forcing data cause large impact in simulated individual trajectories (e.g. the location and timing of when an iceberg drifts out of the sea ice). The horizontal resolution has a small impact on the iceberg statistics (e.g. the iceberg density in the domain) by influencing the availability of forcing information and its representability for the iceberg position, along the coastlines and regions with complex bathymetry. We highlight the importance of this effect in coastal regions, despite its unreliable forcing information, due to the lack of other (environmental and iceberg) information."*

- l515: Did we see that the iceberg looping increases their density? [52] We will add it to the discussion as described in answer [36]. We think that is appropriate to be mentioned in the conclusion as well.

- l572: Drag coefficients are different from those in Table 2. [53] The coefficients in Table 2 are correct. "and water and air drag coefficient Cw,a ~~≈ 1~~."

We will change following as suggested and no further comments are needed. We also adapt the manuscript for the comments about typos and language as suggested.

- l19: "ice features" -> "icebergs"
- l20: Grounding is missing here, and only mentioned far down in the manuscript.
- Grounding is missing from Section 2.4 and first mentioned on l.412.
- Table 1 could contain more information in additional columns and remove Column 1 (objective) which is not followed up in the text: for example, the duration, horizontal resolution, frequency and presence of tides.
- l66: the acronyms ERA5 and CARRA have already been introduced.
- l72: Indicate the version of the IBCAO bathymetry used.
- l82: Many readers may be unfamiliar with the area, move the general map A1 from the annex to this location. Hopen, Bear Island and Storfjorden should be indicated as well and a few isolines of the ocean bathymetry.
- l85: "Empirical relations". Indicate the sources of these relations
- l94: "Coefficient set to zero". Indicate "no added mass" because the coefficient has not been defined here.
- Figure 9 g-j continues on a different page, so the related part of the caption should follow as well.
- Figure 6 is not colourblind-friendly. I cannot tell the two CARRA simulations apart. Perhaps make them dashed lines.
- l251: Contradiction between the two sentences. Replace "all directions" by "most directions"?
- Section 3.6: add "simulated" in the section title for clarity.
- l397: "may indicate an impact". Please introduce the next discussion in a more direct way.
- l491-495: This paragraph sets the context of the results and should be moved to the beginning of Subsection 4.4
- Eq. A15. Indicate the reference for this equation.
- Section A5. It is not necessary to have a section header and one line there since the Tables are referenced in the text.

References:

Stern, A. A., A. Adcroft, and O. Sergienko (2016), The effects of Antarctic iceberg calving-size distribution in a global climate model, J. Geophys. Res. Oceans, 121, 5773–5788, doi:10.1002/2016JC011835.