Response to Reviewer Comments

Manuscript: *Impact of sea ice rheological parameters and grounded iceberg distribution on Antarctic landfast sea ice: a sensitivity study with CICE version 6.4.1*

Atwater et al.

July 29, 2026

General response

We thank both reviewers for their careful reading of the manuscript and for their constructive and encouraging assessments of the study. We are grateful that both reviewers found the manuscript interesting, well motivated, and suitable for publication after revision. The comments will help us improve the physical justification of the grounded-iceberg representation, clarify the scope of the study relative to other fast-ice stabilisation mechanisms, strengthen the treatment of sea-ice thickness and observational uncertainty, and consolidate the discussion of limited interannual variability in the stand-alone CICE configuration.

In response, we will revise the manuscript to: (i) clarify the rationale for the grounded-iceberg retention procedure and default grounded-iceberg fraction; (ii) explain why lateral drag is discussed as a relevant mechanism but not tested in the present experimental design; (iii) moderate the interpretation of simulated sea-ice thickness relative to ESA-CCI and acknowledge limitations of satellite altimetry in coastal Antarctic sea ice; (iv) add a physical interpretation of the threshold-like response between zero and non-zero grounded-iceberg fractions; (v) expand the discussion of coupled feedbacks and perturbed-forcing ensembles as pathways for improving interannual variability; and (vi) correct or clarify a number of textual, figure, and terminology issues.

Line numbers below refer to the future revisions in the manuscript unless otherwise stated.

Reviewer 1

Major comments

R1.1 Physical basis for the retention probability distribution and default grounded-iceberg fraction

Reviewer comment.

> *The reviewer asks for a clearer physical justification for the normal retention-probability distribution, a discussion of regional heterogeneity in grounded-iceberg cluster size, and a clearer rationale for the default grounded-iceberg fraction of 25%.*

Response. We agree that the physical and empirical rationale for the grounded-iceberg retention procedure need to be made clearer in the main text. The intention of the probabilistic thinning is not to represent individual iceberg grounding physics explicitly. Rather, it is to retain the observed spatial structure of the grounded-iceberg field while reducing the effective sub-grid anchoring density to a level compatible with the CICE grid and with the idealised mechanical representation of grounded icebergs used in this study.

We will revise Section 2.1 to clarify that the normally distributed retention probability was chosen as a pragmatic, smoothly varying method that preferentially retains cells with larger grounded-iceberg counts while avoiding a deterministic count threshold. A deterministic threshold would impose an abrupt and poorly justified transition between "inactive" and "active" anchoring cells, while a uniform random thinning would not preserve the observed count-dependent structure of the grounded-iceberg field. The normal-count weighting therefore provides a reproducible way to favour higher-count cells without treating the raw count as a fully resolved mechanical drag law.

We also will add text noting that cells containing several grounded icebergs may have a different mechanical effect from cells containing only one or two grounded icebergs, even when both are represented as grounded-iceberg cells at the CICE grid scale. This regional and sub-grid heterogeneity will be acknowledged as a limitation of the present approach and as a motivation for future work using higher-resolution grounded-iceberg geometry, count-weighted pinning strength, or explicitly formulated lateral-drag terms.

Finally, we will add a brief justification of the 25% default fraction in the main text. The 25% case will be described as a conservative reference configuration that preserves the dominant observed spatial pattern of grounded icebergs while avoiding the excessive spatial constraint produced by retaining all detected grounded-iceberg cells.

R1.2 Observational product for SIA validation

Reviewer comment.

> *The reviewer suggests using the OSI-450 sea-ice concentration product for sea-ice-area validation because it provides per-grid-cell uncertainty estimates that could be used to add observational uncertainty shading to Figure 1a.*

Response. We agree that observational uncertainty is important for interpreting the sea-ice-area validation and that a product with per-grid-cell uncertainty estimates would provide a stronger basis for judging whether small model–observation differences are meaningful. We will revised the validation discussion to make this uncertainty explicit. We will add OSI-450 as an additional observational sea-ice concentration product and revised Figure 1a to show observational uncertainty shading derived from the OSI-450 uncertainty estimates. This provides a clearer basis for comparing the model climatology with observational uncertainty in hemispheric sea-ice area.

R1.3 Sea-ice thickness validation and the role of in-situ observations

Reviewer comment.

> *The reviewer asks that the manuscript explicitly acknowledge uncertainties in the ESA-CCI sea-ice thickness product, particularly near the coast and marginal ice zone, and refer where possible to in-situ measurements as an independent check.*

Response. We agree. We will revise the sea-ice-thickness validation discussion to state more clearly that satellite-altimetry-derived Antarctic sea-ice thickness products have substantial uncertainties, particularly in the coastal zone and marginal ice zone where landfast sea ice occurs. The ESA-CCI product is now described as a broad benchmark rather than a definitive pointwise validation data set for coastal landfast ice.

We will also add discussion of why the model–ESA-CCI difference should not be interpreted as model error alone. Simulated thickness may be biased low because a stand-alone CICE configuration can underrepresent dynamic thickening, ridging, snow-ice processes, or coastal ocean feedbacks. Conversely, satellite-derived thickness can be biased or highly uncertain in coastal and deformed ice because freeboard retrieval, snow loading, density assumptions, mixed surface types, and land contamination are challenging near the Antarctic coast. We will add appropriate references to in-situ, moored, or regional observational constraints where available, while noting that such observations remain sparse relative to the circumpolar domain.

R1.4 Physical interpretation of the step-change between zero and non-zero grounded-iceberg fraction

Reviewer comment.

> *The reviewer asks for a mechanistic explanation of why the largest change in fast-ice area occurs between the no-grounded-iceberg case and the lowest non-zero grounded-iceberg fraction, with smaller additional changes at higher fractions.*

Response. We agree that this result warranted a clearer physical interpretation. We will expand the discussion of grounded-iceberg fraction sensitivity to explain that the response is consistent with a threshold-like anchoring mechanism. In the no-grounded-iceberg case, the model lacks fixed internal obstacles that can stabilise compressive arches or provide local resistance to pack-ice motion near the coast. Introducing even a small fraction of grounded-iceberg cells can therefore create sufficient local pinning points for landfast-ice formation in bathymetrically and geometrically favourable regions. Once these key anchoring locations are present, additional grounded-iceberg cells increase the spatial extent or persistence of landfast ice more gradually.

We will describe this behaviour as threshold-like rather than linearly proportional to grounded-iceberg fraction. We also note that this interpretation is consistent with localised anchoring and subsequent thermodynamic consolidation: a sparse set of pinning points may be enough to nucleate stable landfast ice in favourable embayments or coastal configurations, whereas increasing the retained fraction beyond that point mainly fills gaps or strengthens already-favourable regions.

R1.5 Dominance-index bin boundaries

Reviewer comment.

> *The reviewer notes that the original dominance-index bins do not divide the range $D \in [-1, 1]$ into equal intervals, and suggests either adopting equal-width thresholds or justifying the original thresholds.*

Response. We agree that the original threshold choice required clearer justification. To avoid introducing an arbitrary asymmetry in the interpretation of the dominance index, we will adopt the reviewer's suggested equal-width thresholds. The revised classification will define model-dominant regions as $D < -1/3$, agreement regions as $|D| \leq 1/3$, and observation-dominant regions as $D > 1/3$.

This change will not alter the definition of the dominance index itself, but will provide a more transparent partitioning of the index range into three equal intervals. The relevant text and figure annotations will be updated accordingly.

R1.6 Interannual variability and potential ensemble simulations

Reviewer comment.

> *The reviewer asks for expanded discussion of whether stand-alone ensemble simulations with perturbed atmospheric and/or oceanic forcing fields could sample a wider range of fast-ice states without requiring a fully coupled model.*

Response. We agree that perturbed-forcing ensembles are a useful and realistic future direction for stand-alone CICE experiments. We will expand the relevant discussion to distinguish between two limitations of the present configuration: first, the absence of coupled ocean–ice–atmosphere feedbacks, and second, the use of a single deterministic realisation of the atmospheric and oceanic forcing fields.

We will state that a stand-alone ensemble with perturbed winds, thermodynamic atmospheric forcing, and/or ocean mixed-layer properties could plausibly broaden the simulated range of fast-ice states. Perturbations to coastal wind speed and direction could affect breakout, compaction, shear, and the persistence of landfast-ice arches. Perturbations to ocean thermal forcing and mixed-layer depth could affect basal melt, freeze-up timing, and the seasonal growth-retreat balance. Snowfall perturbations could influence insulation, surface flooding, and snow-ice formation. However, we will not add new ensemble simulations in the present revision because the manuscript is designed as a controlled sensitivity study of rheological parameters and grounded-iceberg representation. We therefore present perturbed-forcing ensembles as a clear future extension rather than as a new component of the present experimental design.

R1.7 Time-varying grounded-iceberg geometry under future warming

Reviewer comment.

> *The reviewer suggests adding a brief forward-looking discussion of how accelerating ice-shelf calving and iceberg ungrounding under future warming could alter the spatial distribution and total extent of Antarctic landfast ice, and what this implies for static grounded-iceberg representations in climate models.*

Response. We agree. We will add a short forward-looking paragraph to the Discussion. The future revision to the text will discuss that future changes in ice-shelf calving, iceberg production, iceberg drift, and ungrounding could modify the spatial distribution of grounded-iceberg anchoring points and thereby alter the location and persistence of Antarctic landfast ice. We will also clarify that a static grounded-iceberg mask may be adequate for short hindcast experiments or for regions where the grounded-iceberg distribution is relatively stable, but is unlikely to be sufficient for long transient projections in which coastal geometry and grounded-iceberg distributions evolve.

Minor comments

R1.a Line 2: clarify "critical habitat"

Reviewer comment.

> *The reviewer notes that "critical habitat" should specify the relevant species or ecological group.*

Response. We agree. We will revise the wording to specify the ecological context of Antarctic landfast sea ice.

R1.b Line 28: replace "iconic"

Reviewer comment.

> *The reviewer suggests replacing "iconic" with a more precise scientific term.*

Response. We agree. We will replace "iconic" with more precise ecological terminology.

R1.c Lines 99–104: ambiguous use of "the latter"

Reviewer comment.

> *The reviewer notes that "the latter" is ambiguous where multiple mechanisms have been introduced.*

Response. We agree. We will revise the relevant paragraph to refer explicitly to the mechanism under discussion.

R1.d Lines 132–133: make future-projection motivation more prominent

Reviewer comment.

> *The reviewer suggests making the limitation of observational-mask approaches more prominent, particularly their inability to generalise outside the observational record.*

Response. We agree. We will strengthen this motivation in the Introduction. The revised text will more clearly contrast diagnostic or observational-mask approaches with prognostic model representations that can, in principle, be applied outside the observed period, including future projections.

R1.e Line 143: "The present study"

Reviewer comment.

> *The reviewer suggests changing "The present study" to "This study".*

Response. We agree and will make this change.

R1.f Lines 149–150: define k_2 and e at first mention

Reviewer comment.

> *The reviewer notes that the rheological parameters k_2 and e are introduced before being formally defined.*

Response. We agree. We will add brief parenthetical definitions at first mention.

R1.g Lines 209–211: replace “fixed” forcing

Reviewer comment.

> *The reviewer notes that “fixed” is not accurate for forcing fields that vary in time and space.*

Response. We agree. We will revise the wording to avoid implying that the forcing fields are temporally or spatially constant.

R1.h Lines 282–283: awkward sentence on rolling-mean speed

Reviewer comment.

> *The reviewer suggests revising the sentence describing the rolling-mean sea-ice speed criterion.*

Response. We agree. We will revise the sentence for clarity.

R1.i Line 406: punctuation

Reviewer comment.

> *The reviewer notes a punctuation issue in the sentence concerning moderate tensile strength.*

Response. We agree and will correct the punctuation.

R1.j Figures 3–6: aspect ratio and projection

Reviewer comment.

> *The reviewer notes that Figures 3–6 have a stretched aspect ratio and suggests either using a polar stereographic projection or correcting the current rectangular projection.*

Response. We agree that the readability of these figures could be improved. We will revise the figure presentation to avoid the stretched aspect ratio and to make the coastal landfast-ice signal easier to interpret.

R1.k Lines 428–430: provide regional examples

Reviewer comment.

> *The reviewer suggests reiterating specific regional examples when summarising that grounded icebergs promote landfast-ice formation in bathymetrically favourable zones.*

Response. We agree. We will add regional examples to ground this general statement in the spatial results.

R1.l Conclusion: consolidate remarks on poor interannual variability

Reviewer comment.

> *The reviewer suggests consolidating the discussion of limited interannual variability into one cohesive paragraph identifying likely causes, required model developments, and implications for the paper's conclusions.*

Response. We agree. We will consolidate the discussion of limited interannual variability to avoid repetition and to state the limitation more clearly. The revised paragraph will identify likely causes, including the stand-alone model configuration, the absence of coupled feedbacks, limitations in ocean mixed-layer representation, and the use of a single deterministic forcing realisation. It will also identify future developments, including coupled simulations, improved ocean forcing or mixed-layer treatment, time-varying grounded-iceberg geometry, and perturbed-forcing ensembles. Finally, we will clarify that this limitation affects the model's ability to reproduce year-to-year variability, but does not undermine the main conclusion that grounded icebergs and rheological parameter choices strongly control the mean spatial distribution and seasonal persistence of simulated Antarctic landfast sea ice.

Reviewer 2

Overview

Reviewer comment.

> *The reviewer finds the study interesting, convincing, and well-written, and considers it suitable for publication after relatively minor revisions.*

Response. We thank the reviewer for this positive assessment and for the constructive comments. The future revisions we will make in response to Reviewer 2 particularly improve the clarity of the Introduction, the explanation of the stochastic grounded-iceberg thinning method, the interpretation of simulated sea-ice thickness, the discussion of coupled feedbacks and interannual variability, and several figure captions and terminology choices.

Major comments

R2.1 Lines 100–103: lateral drag mentioned but not addressed

Reviewer comment.

> *The reviewer notes that lateral drag is mentioned in the Introduction but then not addressed in the rest of the paper, and asks that we explain why it is outside the scope of this study.*

Response. We agree that this point needs to be made explicit. We will revise the Introduction to clarify that lateral drag is an important potential fast-ice stabilisation mechanism, but it is not tested in the present study because our experimental design is focused on two specific controls: the rheological parameter choices available within the CICE configuration and the mechanical effect of prescribed grounded-iceberg pinning points. Adding a lateral-drag parameterisation would introduce an additional mechanism that is not uniquely separable from grounded-iceberg pinning in the present experiment set and would require separate choices about form factors, drag law, coastal geometry, and boundary treatment.

We will state that lateral drag is therefore treated as a related but distinct modelling pathway and a topic for future work, rather than as a mechanism evaluated in this manuscript. This revision will also clarify the scope of the manuscript: it evaluates whether Antarctic landfast ice can be improved in a stand-alone CICE framework through rheological sensitivity and grounded-iceberg representation, not through a comprehensive comparison of all proposed stabilisation mechanisms.

R2.2 Lines 353–355: strength of the sea-ice-thickness claim

Reviewer comment.

> *The reviewer suggests that it is too strong to claim that the models produce a broadly realistic SIT climatology when all runs are biased at least* 0.5–1 *m below the satellite estimate, and asks for possible reasons for the low model thickness or high satellite estimate.*

Response. We agree. We will moderate the statement and revise the thickness discussion so that the manuscript no longer implies that the simulated SIT climatology is fully realistic in magnitude. The revised text will distinguish between reproducing the broad spatial and seasonal structure of Antarctic sea-ice thickness and underestimating the magnitude relative to ESA-CCI.

We will also add a short discussion of plausible causes. On the model side, the low bias may reflect underrepresentation of dynamic thickening, ridging, rafting, snow-ice formation, or coastal ocean feedbacks in the stand-alone configuration. It may also reflect uncertainties in snow accumulation and ocean thermal forcing, which influence insulation, basal growth, and melt. On the observational side, ESA-CCI thickness is uncertain near the Antarctic coast and in deformed or landfast ice because freeboard retrievals, snow depth, density assumptions, mixed ice types, and land contamination are challenging in precisely the regions where LFI occurs. We will therefore interpret the ESA-CCI comparison as evidence that the model remains thin relative to available satellite estimates, while avoiding the stronger claim that the thickness climatology is realistic in absolute magnitude.

R2.3 Lines 505 and 620–621: coupled feedbacks and interannual variability

Reviewer comment.

> *The reviewer asks us to elaborate in the Discussion on the coupled feedbacks that could influence year-to-year variability and the physical mechanisms by which they may act.*

Response. We agree and will expand the Discussion. The revision will explain that limited interannual variability in the stand-alone CICE experiments likely reflects both the prescribed-forcing framework and missing coupled feedbacks. Potential feedbacks include atmosphere–ice feedbacks, in which persistent wind anomalies alter compaction, divergence, breakout, and arch stability; ocean–ice feedbacks, in which mixed-layer temperature, stratification, and coastal polynya activity influence basal growth and melt; snow–ice feedbacks, in which snowfall affects insulation, surface flooding, and snow-ice formation; and geometry feedbacks, in which time-varying grounded icebergs or large tabular bergs modify local anchoring and coastal sheltering.

We will also link this discussion to the ensemble point raised by Reviewer 1. A perturbed-forcing stand-alone ensemble could test the sensitivity of LFI variability to uncertainties in winds, atmospheric thermodynamics, snowfall, and ocean mixed-layer properties, whereas a fully coupled model would be needed to capture feedbacks that depend on interactive adjustment among the atmosphere, ocean, sea ice, and coastal geometry.

Minor comments

R2.a Lines 28–29: repeated species name

Reviewer comment.

> *The reviewer notes that "Aptenodytes forsteri" is repeated.*

Response. We agree and will remove the repeated species name.

R2.b Lines 149–150 / 194–150: define rheological parameters

Reviewer comment.

> *The reviewer notes that the parameters introduced here are not defined clearly at first mention.*

Response. We agree. This overlaps with Reviewer 1 minor comment f. We will add brief parenthetical definitions at first mention so that readers are not required to wait for the later formal model-description section.

R2.c Lines 183–184: explain the stochastic thinning notation

Reviewer comment.

> *The reviewer asks for the stochastic thinning method to be spelled out more explicitly, including the role of $U(0,1)$ and the retain/remove decision.*

Response. We agree. We will revise the description of the grounded-iceberg thinning procedure to be more explicit. The revised text will state that, for each candidate grid cell, a random value u is drawn from a uniform distribution $U(0,1)$. If $u < p(c)$, where $p(c)$ is the retention probability associated with grounded-iceberg count c, the cell is retained as a grounded-iceberg cell; otherwise it is removed from the thinned mask. This explanation will be included in words before or immediately after the compact mathematical notation.

R2.d Line 282: rolling mean wording

Reviewer comment.

> *The reviewer suggests revising the phrase to "a two-week rolling mean of sea-ice speed to distinguish ...".*

Response. We agree and will adopt the suggested wording.

R2.e Lines 285–301: improve the flow of the landfast-ice classification metrics

Reviewer comment.

> *The reviewer notes repetition and unclear ordering in the discussion of $M_{\mathrm{FI,day}}$ and $M_{\mathrm{FI,bin}}$, and suggests defining both metrics before explaining why the binary metric is used.*

Response. We agree. We will reorganise this paragraph so that the two classification diagnostics are defined before they are compared. The revision will first define the daily threshold metric $M_{\mathrm{FI,day}}$ and the binary-days metric $M_{\mathrm{FI,bin}}$, then will explain why $M_{\mathrm{FI,bin}}$ is used for the main analysis, and only then discusses its practical advantages relative to the two-week rolling-speed diagnostic. We will also remove repeated explanatory sentences and clarify the transition that previously began with "Rather".

R2.f Line 303: comma after "i.e."

Reviewer comment.

> *The reviewer notes that a comma is needed after "i.e.".*

Response. We agree and will make this correction.

R2.g Line 313: clarify forcing sentence

Reviewer comment.

> *The reviewer notes that a comma is needed between "forced with ERA5" and "and ORAS" to avoid implying that ACCESS-OM2-025 is forced with both ERA5 and ORAS.*

Response. We agree. We will revise the sentence to remove the ambiguity in the forcing description.

R2.h Figure 1: inconsistent model-period caption

Reviewer comment.

> *The reviewer notes that the caption for panel (b) says the models span 1994–2017, while the overall caption says 1994–1999.*

Response. We agree and will correct the inconsistency.

R2.i Line 425 and elsewhere: use LFI terminology consistently

Reviewer comment.

> *The reviewer suggests using "LFI" where the manuscript says "landfast" and checking terminology consistency throughout.*

Response. We agree. We will check the manuscript for inconsistent use of "landfast", "landfast ice", "fast ice", and "LFI". Where the term is used as a noun or defined scientific object, we will use “LFI” or “landfast ice” consistently. We will retain adjective usage only where grammatically appropriate.

R2.j Figure 6: stretched aspect ratio

Reviewer comment.

> *The reviewer notes that Figure 6 appears horizontally stretched, particularly the axis-label text.*

Response. We agree. This overlaps with Reviewer 1 minor comment j. We will correct the aspect ratio and regenerate the figure so that text and spatial features are not distorted.

R2.k Figure 7: vertical position of coloured dots

Reviewer comment.

> *The reviewer asks why the coloured dots appear at different vertical positions within each row and what the y-position represents.*

Response. We agree that this was insufficiently clear. The vertical offsets were used only to reduce overplotting among simulations within the same parameter group; they do not encode an additional physical variable. We will revise the figure and caption to remove this ambiguity.

R2.l Figure 10: legend label

Reviewer comment.

> *The reviewer notes that the Figure 10 legend should say “F2020” rather than “AF2020”.*

Response. We agree and will correct the legend label.

R2.m Lines 521–524: remove redundant summary

Reviewer comment.

> *The reviewer notes that this summary is redundant because a more detailed summary follows and the point is repeated in the Conclusions.*

Response. We agree. We will remove the redundant summary sentence to improve flow and reduce repetition.

R2.n Lines 537–538: repeated phrase

Reviewer comment.

> *The reviewer notes that “tensile strength parameter” is repeated.*

Response. We agree and will remove the repetition.

R2.o Line 585: clarify 67% and near-equivalent reference

Reviewer comment.

> *The reviewer asks us to clarify what the simulated mean fast-ice area is 67% of, and what the near-equivalent circum-Antarctic fast-ice area is being compared with.*

Response. We agree. We will revise the sentence to explicitly state the comparison baseline. The 67% value refers to the simulated mean fast-ice area expressed as a fraction of the observed AF2020 circum-Antarctic mean fast-ice area over the common analysis period. The phrase "near-equivalent" will also be revised to state the explicit reference case or observational estimate being compared.

R2.p Lines 796–797: remove "your earlier note"

Reviewer comment.

> *The reviewer notes that "Consistent with your earlier note" is unclear.*

Response. We agree. This phrase was an editing artefact and will be removed.

R2.q Citation link

Reviewer comment.

> *The reviewer provides the public review citation: https://doi.org/10.5194/egusphere-2026-1541-RC2.*

Response. We acknowledge the citation and will ensure that the response is linked to the correct public review in the EGUsphere discussion system.

Summary of principal revisions

In response to both reviewers, the principal revisions will be as follows:

(1) We will clarify the physical and empirical rationale for the grounded-iceberg retention probability distribution and the default grounded-iceberg fraction.

(2) We will expand the explanation of the stochastic thinning procedure, including the retain/remove decision based on a draw from $U(0, 1)$.

(3) We will clarify that lateral drag is a relevant but separate fast-ice stabilisation mechanism that is outside the scope of the present experiment set.

(4) We will strengthen the treatment of observational uncertainty in sea-ice-area and sea-ice-thickness validation, and moderated the statement about simulated SIT realism.

(5) We will add a mechanistic interpretation of the threshold-like response between zero and non-zero grounded-iceberg fractions.

(6) We will revise the dominance-index binning to use equal-width intervals.

(7) We will expand and consolidate the discussion of limited interannual variability, coupled feedbacks, and perturbed-forcing ensembles.

(8) We will add a forward-looking discussion of time-varying grounded-iceberg geometry under future climate change.

(9) We will correct terminology, figure captions, repeated phrases, and several minor wording and punctuation issues.