Manuscript Review

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.

This manuscript makes a significant and timely contribution to Antarctic sea ice modelling by introducing grounded icebergs (GIBs) into the CICE model framework and demonstrating their critical role in reproducing realistic Antarctic landfast sea ice (LFI). The methodology is described thoroughly, the experimental design is well-motivated, and the conclusions are well-supported by the sensitivity experiments. I recommend acceptance subject to the minor revisions detailed below, which are intended to strengthen the physical justification of certain methodological choices and to improve the clarity and presentation of several figures and passages.

Major Comments

1. **Physical basis for the retention probability distribution and default GIB fraction.** The authors state that the retention probability p(c) is drawn from a normal distribution and that "this probabilistic framework ensures consistency with observed spatial patterns while maintaining model realism." However, it would strengthen the manuscript to provide a brief physical justification for why a normal distribution was chosen over, for example, a uniform or exponential one. Additionally, the authors should consider whether the GIB dataset reveals systematic regional differences in GIB cluster size, as randomly thinning a dense cluster of 5–10 GIBs per grid cell will have a qualitatively different mechanical effect than thinning a region with only 1–2 GIBs. The sensitivity of model results to this regional heterogeneity would be a valuable addition to the discussion. Similarly, the default choice of $GIB_\theta = 25\%$ is not explicitly justified in the main text; while Appendix A provides some context regarding the scaled non-linear function of GIB count, the physical or empirical rationale for this default fraction should be briefly summarized in the main text (Section 2.1).

2. **Observational product for SIA validation (Figure 1a).** The authors currently use the NSIDC sea ice concentration product for SIA validation. Switching to the OSI-450 product would be advantageous because OSI-450 provides per-grid-cell uncertainty estimates, which would allow the construction of observational uncertainty shading on the SIA climatology in Figure 1a. This would considerably strengthen the validation, making it easier to assess whether model biases are statistically meaningful relative to observational uncertainty.

3. **Sea ice thickness validation and the role of in-situ observations (Lines 341–343).** The authors rely on the ESA-CCI satellite altimetry product to evaluate simulated SIT. While this is a reasonable benchmark, the ESA-CCI product carries substantial uncertainties, particularly in the coastal and marginal ice zones where LFI predominantly occurs. The authors should explicitly acknowledge these limitations and, where available, reference in-situ measurements (e.g., from upward-looking sonar on submarines, or moored

instruments) to provide an independent check that the simulated SIT is not substantially biased, and that any remaining discrepancy can plausibly be attributed to observational uncertainty in ESA-CCI rather than model error.

4. **Physical interpretation of the step-change in sensitivity between zero and non-zero GIB fraction.** The parameter sensitivity results (Section 3.3, Figure 7) show clearly that the largest change in fast-ice area occurs between the no-GIB case and the lowest non-zero GIB fraction, with further increases in GIB_θ producing comparatively modest additional changes. The authors report this finding but do not offer a physical explanation. Even a brief mechanistic discussion (for example, whether a small number of GIBs is sufficient to nucleate LFI arches that then expand thermodynamically, or whether the threshold reflects a percolation-like behavior) would substantially deepen the reader's understanding and the scientific contribution of this result.

5. **Dominance index bin boundaries (Lines 475–478).** The authors group the dominance index D into three bins: model-dominant ($D \leq -0.5$), agreement ($|D| < 0.5$), and observation-dominant ($D \geq 0.5$). This is a reasonable choice, but the boundaries are not symmetric in the sense that they do not divide the $D \in [-1, 1]$ range into three equal intervals. Thresholds of $|D| < 1/3$ for agreement, $D > 1/3$ for observation-dominant, and $D < -1/3$ for model-dominant would create three equal-width thirds. The authors should either adopt these equal-interval thresholds or explicitly justify why asymmetric thresholds are more physically appropriate in this context.

6. **Interannual variability and the potential for ensemble simulations (Figure 10, Lines 506+).** The authors acknowledge that a single stand-alone CICE simulation cannot reproduce the observed interannual range of mean fast-ice area, and attribute this primarily to the absence of coupled ocean feedbacks. The authors also briefly allude to the possibility of ensemble approaches in the text. I encourage the authors to expand this discussion: in principle, a standalone ensemble in which atmospheric and/or oceanic forcing fields are perturbed within their uncertainty bounds could sample a wider range of fast-ice states without requiring a fully coupled model. Have the authors considered this approach? Even a qualitative assessment of whether such perturbations could plausibly account for the observed interannual range would help readers understand the limits of the current framework and motivate future work.

7. **Implications of time-varying GIB geometry under future warming.** The authors note that allowing major calving events and large tabular GIBs to update coastal geometry through time would likely reduce some of the persistent LFI mismatches identified in the comparison with F2020, while a static treatment remains adequate for smaller GIBs whose distribution is comparatively stable. This is an important finding with clear implications for climate projections. I suggest the authors add a brief forward-looking discussion (2–4 sentences) addressing how accelerating ice-shelf calving and iceberg ungrounding under future warming scenarios might alter the spatial distribution and total extent of Antarctic LFI, and what this means for the adequacy of static GIB representations in coupled climate models used for future projections.

Minor Comments

a. Line 2: The phrase "critical habitat" is used without specifying habitat for which species or ecological group. Please clarify (e.g., "critical breeding and foraging habitat for ice-dependent species such as emperor penguins and Weddell seals").

b. Line 28: "Iconic" is imprecise in a scientific context. Consider replacing with a term that conveys ecological function more precisely, such as "secondary consumer" or "apex predator" depending on the species in question, or simply "ecologically critical."

c. Lines 99–104: The use of "the latter" in this paragraph, particularly in the final sentence, is ambiguous when more than two mechanisms have been introduced. Please replace with explicit references to the mechanism being discussed (e.g., "In this study, we focus on the first proposed mechanism: physical pinning by GIBs."). This also clarifies for readers that additional stabilizing mechanisms exist beyond the two discussed.

d. Lines 132–133: The statement that the observational-mask approach "does not readily generalize to periods outside the observational record, such as future projections of LFI" is an important point in favor of the prognostic approach developed here. Consider making this motivation more prominent, as it underscores a key advantage of the present study over previous work.

e. Line 143: "The present study" → "This study" (clarity).

f. Lines 149–150: The rheological parameters k_2 and e are introduced here but not formally defined until later in the text. Please provide brief parenthetical definitions at first mention (e.g., "tensile strength parameter k_2, which controls resistance to tensile stresses, and the ellipse aspect ratio e, which governs shear strength relative to compressive strength").

g. Lines 209–211: "Fixed" is not an accurate descriptor for forcing fields that vary in both time and space. Suggest replacing with: "By constraining the oceanic and atmospheric drivers to observational reanalysis products, the stand-alone configuration enables systematic sensitivity testing…"

h. Lines 282–283: The sentence reads awkwardly. Suggest revising to: "We employ a two-week rolling mean of sea-ice speed to distinguish LFI from pack ice and classify a grid cell as LFI if…"

i. Line 406: "…the addition of **moderate** tensile strength alone cannot reproduce the observed magnitude or timing of Antarctic LFI.").

j. Figures 3–6: These figures currently have a stretched aspect ratio that makes the coastal band difficult to interpret, particularly for readers accustomed to circumpolar Antarctic projections. The authors are encouraged to consider presenting the persistence maps in a polar stereographic projection (with the two sectors indicated by shaded arcs or inset boxes), which would substantially improve readability and allow more direct comparison with the observational literature. If the authors prefer to retain the current rectangular projection (easier to "see" the landfast ice changes), the aspect ratio should be corrected. I leave the final decision to the authors' judgement; this is very minor.

k. Lines 428–430: When summarizing that GIBs promote LFI formation in bathymetrically favorable zones, it would be useful to briefly reiterate specific regional examples (e.g., Lützow-Holm Bay, Shackleton Ice Shelf, Totten Ice Shelf) to ground the general statement in the spatial results already presented.

l. Conclusion section: The limitations associated with poor interannual variability are mentioned in multiple places across the Results, Discussion, and Conclusions sections in a somewhat fragmented way. I suggest consolidating these remarks into a single, cohesive paragraph (either in the Discussion or the Conclusions) that clearly identifies (a) the likely causes of limited interannual skill, (b) what additional model developments would be needed to address them, and (c) the degree to which this limitation affects the key conclusions of the paper.

In summary, this is a well-executed and important study that advances the community's ability to simulate Antarctic LFI in a physically meaningful way. The comments above are intended to help the authors clarify their methodological choices and strengthen the physical interpretation of their results. I look forward to reviewing the revised manuscript.