Reviewer Report on EGUsphere-2026-826: "High-Resolution Modelling of Landfast Ice Formation and Midseason Breakout on the Siberian Shelf"

July 14, 2026

Summary and recommendation

This manuscript presents a regional, approximately 2 km, coupled sea-ice–ocean configuration based on MITgcm for the Kara, Laptev, and East Siberian Seas. The study evaluates simulated landfast ice against U.S. National Ice Center (NIC) ice charts, examines the effect of a basal-stress grounding parameterization, and analyses four simulated midseason breakout events in the East Siberian Sea. The topic is timely and scientifically relevant. High-resolution regional modelling of Siberian landfast ice, combined with event-based breakout diagnostics, could make a useful contribution to Arctic coastal sea-ice dynamics.

I recommend **major revision**. I do not consider the manuscript ready for publication in its present form. The work appears to contain useful model results, but the scientific argument is not yet sufficiently supported, documented, or clearly presented. The model evaluation remains mostly qualitative, the fast-ice and breakout diagnostics are not described reproducibly, the interpretation of "grounding contribution" is stronger than the diagnostics justify, and the breakout attribution requires a more systematic treatment. In addition, the manuscript has substantial problems with language, notation, figure readability, and caption quality. These are not merely cosmetic issues: in several places they make it difficult to understand exactly what was calculated and therefore difficult to assess the robustness of the conclusions.

General assessment

The main strengths of the manuscript are:

- the use of a high-resolution regional model in a geographically complex Arctic shelf region;
- a direct comparison between a basal-stress experiment and a control experiment;
- an attempt to separate basal-stress-active and non-basal-stress-active portions of the simulated landfast-ice cover;
- an event-based analysis linking East Siberian Sea midseason breakouts to cyclone-driven offshore winds.

The main concerns requiring major revision are:

- the validation against NIC charts is too qualitative and should be supported by standard skill metrics;
- the landfast-ice diagnostic, especially the use of two-week mean velocity, is not specified rigorously enough to reproduce the analysis;
- the use of "daily detachment" diagnostics in **Fig. 12** is unclear and appears inconsistent with a fast-ice definition based on biweekly mean ice speed;

- the phrase "contribution of grounding" is used more causally than the current diagnostics justify;
- the basal-stress formulation, parameter notation, units, and tuning choices need correction and clearer documentation;
- the three-year simulation period and apparent tuning/evaluation over the same period limit the robustness of the conclusions;
- the breakout attribution requires a systematic event-detection and forcing analysis, including missed events, false events, and the possible roles of ocean stress, tides, pack-ice stress, preconditioning, and model thermodynamics;
- the figures, captions, and language need substantial revision before the scientific content can be assessed confidently;
- the code and data availability statement is insufficient for reproducibility of the model configuration and analysis.

Major comments

MC1. The manuscript needs substantial revision before the scientific claims can be evaluated reliably.

The manuscript has a promising scientific basis, but the current presentation is not at publication standard. Many sentences contain grammatical errors, broken phrases, inconsistent terminology, or unclear syntax. Several figure captions are too brief to be self-contained, and many figures are too small or too visually cluttered to support the detailed interpretations made in the text. This is particularly problematic in a modelling paper where the figures carry much of the evidence for model skill, mechanism, and event attribution.

I recommend that the authors revise the manuscript thoroughly for language, notation, and presentation before resubmission. The revision should not be limited to copy editing. The authors need to clarify exactly what was computed, how the model and observations were compared, how the diagnostic categories were defined, and which conclusions are supported directly by the diagnostics.

MC2. The model evaluation should be made quantitative.

The manuscript repeatedly states that the basal-stress experiment produces "realistic" or "highly realistic" landfast-ice distribution and seasonal cycle, but most of the evidence is visual. Please add quantitative validation against NIC charts for the full domain and for the Kara, Laptev, and East Siberian Seas separately. Useful metrics would include mean bias in landfast-ice area, RMSE, correlation, onset-date error, breakup-date error, integrated seasonal landfast-ice area, occurrence-frequency bias, probability of detection, false-alarm ratio, and/or landfast-edge position error. A concise regional skill table comparing CTRL and BD would substantially strengthen the manuscript.

This is particularly important because **Fig. 3** shows spatially structured biases: overestimation in parts of the Laptev and East Siberian Seas and underestimation in the Kara Sea. The text describes these patterns, but the magnitude, persistence, and regional importance of the errors are not quantified. Similarly, the statement that the control run reproduces approximately 44% of the observed landfast ice should be defined precisely: over what dates, regions, and metric is this percentage calculated?

MC3. The landfast-ice diagnostic must be specified more rigorously.

The manuscript identifies landfast ice using daily output, a biweekly mean ice speed threshold of $0.5\,\mathrm{mm\,s^{-1}}$, and sea-ice concentration greater than 0.8. Please specify exactly how the temporal averaging is done. Does the diagnostic use the magnitude of the two-week mean velocity vector, or the two-week mean of the speed magnitude? These are not equivalent. Please also state whether the two-week averaging window is centred, trailing, or leading; how endpoints are handled; whether concentration is averaged over the same window or applied daily; and whether model diagnostics are sampled on exact NIC chart dates.

Please also state whether any coastal connectivity criterion is applied. A velocity threshold alone can classify compact but temporarily stationary pack ice as landfast, especially under convergent conditions. If no connectivity criterion is used, this should be justified and discussed as a source of possible false-positive landfast ice.

The comparison with NIC charts also requires more detail. The NIC charts are described as 10 km weekly/biweekly products, whereas the model grid is approximately 2 km. Please state whether the NIC polygons were regridded to the model grid, the model was aggregated to the NIC grid, or both were compared on a common grid. Please also explain how partial coastal cells, land masks, and differing coastlines were handled.

MC4. The breakout-event analysis is internally unclear, especially the use of "daily detachments" in Fig. 12.

The breakout analysis is potentially valuable, but the diagnostic chain is not clear. The authors define landfast ice using a biweekly average ice-speed threshold, yet **Fig. 12** presents "daily detachments of fast ice" over individual dates. If the daily panels are based on daily differences in a diagnostic that itself contains a two-week averaging window, then the daily values are not independent daily detachments in the usual sense. They are changes in a temporally smoothed or windowed classification. This matters because the timing, sequence, and spatial progression of the detachments are then partly determined by the averaging window.

Please clarify how **Fig. 12** was calculated. Was a landfast-ice mask generated for every model day using a rolling two-week velocity average and then differenced between successive days? Or was a daily velocity field used for **Fig. 12** despite the biweekly diagnostic used elsewhere? If the latter, the manuscript is using two different fast-ice diagnostics and should justify this. If the former, the caption and text should not imply that the figure resolves daily physical detachment without explaining the temporal memory in the diagnostic.

More generally, **Fig. 12** occupies an entire page but is difficult to interpret. The panels are very small, the caption does not define the colours or diagnostic, and the relationship to the event narrative is not self-evident. The authors should either redesign this figure into a clearer event summary, move the full panel array to supplementary material, or replace it with a more quantitative figure/table showing daily breakout area, offshore wind stress, and basal-stress-active fraction for each event.

MC5. Clarify what is meant by the "contribution" of grounding.

The manuscript reports that grounded landfast ice accounts for approximately 56% of simulated landfast ice over the Siberian Seas, with regional values of 70%, 54%, and 41%. This is an interesting diagnostic, but it should not be interpreted too strongly as the causal contribution of grounding to landfast-ice formation or stability. As described, "grounded" landfast ice appears to mean grid cells that satisfy the fast-ice diagnostic and have active basal stress. That does not necessarily mean that basal stress caused those cells to become fast, nor that surrounding ungrounded fast ice would persist without the grounded cells.

Please revise the terminology throughout the abstract, results, discussion, and conclusion. I suggest using phrasing such as "the fraction of simulated landfast ice area for which basal stress is active" unless a causal attribution is demonstrated. If the authors wish to quantify causal contribution, they should use the control experiment more explicitly and discuss non-additivity: the difference between BD and CTRL is not equivalent to the area of basal-stress-active grid cells because the model state evolves differently in the two experiments.

MC6. The basal-stress formulation requires correction and clearer documentation.

The description of the momentum equation and basal-stress parameterisation contains several apparent errors or ambiguities. In **L80**, basal stress is denoted using $\boldsymbol{\tau}_o$, which has already been defined as ocean-ice stress; it should be $\boldsymbol{\tau}_b$. The printed mathematical notation in Eq. (2) and the following text is difficult to interpret, and the units of the tunable constants are unclear. The manuscript alternates between k_R, k_G, k_1, k_2, and other notation. Please rewrite this section carefully, define all symbols, give units, and ensure consistency with Lemieux et al. (2015).

The parameter choices also need stronger justification. The discussion mentions $k_1 = 7$ and that $k_1 = 8$ overestimated landfast ice, but the methods list different notation and no systematic sensitivity analysis is shown. Please include a sensitivity table/figure or clearly state how tuning was performed, what criteria were used, and whether the same years were used for both tuning and evaluation. The discussion also refers to a high "P-value" of 22460; this should be defined as a sea-ice strength parameter, with units and formulation, not described in language that could be confused with a statistical p-value.

MC7. The short simulation period, spin-up, and tuning strategy should be addressed.

The simulations span only September 2012 to September 2015 and are initialized from nearly ice-free conditions. This may be acceptable for a first regional study, but the manuscript should discuss representativeness, spin-up, and uncertainty explicitly. Three winters are unlikely to sample the full range of Siberian landfast-ice variability, especially if parameters were tuned over the same period. Please consider extending the simulation, adding an independent validation period, or at minimum acknowledging this limitation and avoiding overgeneralization from a three-year mean.

The manuscript should also explain why 2012–2015 was chosen. If these years were selected because the breakout events were known in advance, then the event analysis may be closer to a case-study evaluation than an independent model-skill assessment, which should be stated clearly.

MC8. The breakout attribution needs a more systematic skill assessment and forcing diagnosis.

The four East Siberian Sea breakout events are a valuable part of the manuscript, but the current analysis is not sufficiently rigorous to support all causal statements. Please clarify whether the four events were first identified in NIC charts, in the BD simulation, or jointly. Were there observed events that the model missed? Were there simulated events without an observed counterpart? The missed February 2014 Laptev Sea event shown later in **Fig. 13** suggests that event skill should be summarised explicitly.

The attribution to cyclones and offshore winds is plausible, but it should be quantified. For each event, consider reporting wind-speed anomalies, wind stress, the component of wind stress normal to the landfast-ice edge, pre-event landfast-ice thickness, internal stress or strain-rate diagnostics if available, and ocean-ice stress in the breakout region. The statement that water stress is small is based on averages over basal-stress-active grounded

areas, whereas the breakouts occur at ungrounded outer edges. Ocean stress, tides, pack-ice forcing, and preconditioning may be more relevant there than in the grounded interior.

Please also document the cyclone-tracking method. At present, the reader cannot reproduce how the seven cyclones were identified from JRA-55 sea-level pressure fields. If cyclone tracks were manually identified, the authors should state this and provide the criteria.

MC9. The role of temporal resolution in forcing and output should be discussed.

The manuscript uses 6-hourly JRA-55 atmospheric forcing and daily model output while interpreting breakout events that may occur over short periods and during rapidly evolving storms. Please discuss whether 6-hourly forcing and daily output are sufficient to resolve the peak wind stresses relevant to fracture and detachment. If possible, report the maximum 6-hourly wind speed or wind stress during each event, rather than relying only on daily or event-period summaries.

MC10. The interpretation of non-grounding mechanisms is too broad.

The manuscript sometimes equates ungrounded landfast ice with "local geometry", "ice arcing", or "grounding-independent mechanisms". These are not identical. Geometric shelter, tensile strength, arching between islands, coastal boundary representation, numerical resolution, and internal ice strength can all contribute. Please define the categories more precisely and avoid implying that the model has separately diagnosed each mechanism unless additional diagnostics are added.

The statement that the lateral-drag parameterisation of Liu et al. (2022) was not included because fine coastlines and small islands are already resolved should be softened. A 2 km grid is a major improvement over pan-Arctic configurations, but it still cannot resolve many sub-kilometre coastal features, ridges, grounded keels, or narrow channels. This may be particularly relevant to the Kara Sea underestimation and to regions where grounding is weak.

MC11. The role of bathymetry smoothing and minimum depth should be examined.

The basal-stress parameterisation is directly controlled by water depth, yet the model bathymetry is interpolated, smoothed, and subject to a 10 m minimum depth. These choices can strongly affect whether basal stress is activated, especially near the 10–25 m depth range associated with many landfast-ice edges. Please provide more detail on the smoothing procedure and consider adding a sensitivity or at least a discussion of how bathymetry processing may influence the spatial distribution of grounded fast ice and the missed Laptev breakout.

MC12. Modeled ice thickness is not validated

The basal-stress parameterisation relies on accurate modeled ice thickness. No output maps of thickness are shown, nor comparisons between input and output ice thickness. Without this, we have no way of knowing how the modeled thickness is performing.

MC13. Unsupport supposition in ascribing breakouts to cyclones

Entirety of P15: The breakouts are being ascribed to the effects of cyclones, but there is no strong evidence for this - everything is presented as causal but, at best, coincidence is shown.

MC14. Data and code availability are insufficient for reproducibility.

The availability statement lists MITgcm and public input datasets, but it does not provide the specific model configuration, namelists, modified basal-stress code if modifications were required, preprocessing scripts, analysis scripts, or model outputs used to generate the figures. For a modelling paper, this is not sufficient to reproduce the results. Please archive the exact

configuration and analysis workflow, preferably with a DOI, and provide sufficient output or post-processed diagnostics to reproduce the main figures and tables.

Specific comments

1. **Title.** Consider "Siberian shelf seas" rather than "Siberian Shelf", since the study covers multiple shelf seas.

2. **L8–15**. The abstract should avoid overly strong claims such as "yields realistic simulations" unless quantitative skill metrics are included. Revise "Siberia shelf seas" to "Siberian shelf seas", use "breakout" consistently as one word, and avoid causal phrasing such as "whose offshore winds fractured the landfast ice" unless the force-balance evidence is strengthened.

3. **L20–24**. The fast-ice edge is said to be typically within the 10–25 m depth range. Later, the model imposes a 10 m minimum depth and uses a 20 m contour in **Fig. 6**. Please connect these choices more clearly to the observational literature and the basal-stress activation criterion.

4. **L35–43**. Replace "in-site" with "in situ". Use "breakout" consistently. Check spelling of Bogardus et al. (2020); it is misspelled in the text as "Borgardus".

5. **L47**. Replace "Beatty" with "König Beatty"

6. **L52–56**. "Very usefully" should be revised. The sentence also overgeneralises the limitations of coarse models. Parameterisations do not only compensate for coarse resolution; they may represent unresolved physical processes even at relatively high resolution.

7. **L59**. It is interesting to only use the Lemieux parameterisation, given that Liu et al. show the importance of also using the coastal drag parameterisation in MITgcm.

8. **L70**. The part describing JRA55 deserves a new paragraph.

9. Figure 1. No longitudes are shown. It may be better to avoid referring to "200 E" in **L93**; "20 W" would likely be clearer.

10. **L65–72**. Please provide the exact NIC product identifier and processing steps. The temporal resolution appears to vary between weekly and biweekly; explain how this was handled in model–observation comparisons.

11. **L84**. How does the value of u compare with that suggested by Lemieux? The same question applies to k_1 and k_2 in **L86**.

12. **L75–86**. Correct the momentum-equation notation. Basal stress should not be denoted as $\boldsymbol{\tau}_o$, and all variables in Eq. (2) require clear definitions and units. The expression for ice-speed magnitude appears incorrectly typeset. Also correct "added it to the right side" to "added to the right-hand side".

13. **L87**. Change "Model description and implement" to "Model description and implementation".

14. **L89–92**. The phrase "viscous-plastic dynamic thermodynamics" is unclear. Please distinguish the dynamic rheology/solver from the thermodynamic scheme.

15. **L98**. "A reset version" is not a particularly natural phrase.

16. **L100**. Is setting the minimum depth to 10 m a common procedure?

17. **L101**. It would be better to describe the run length and start/end dates before specifying the initial conditions.

18. **L103**. As I understand it, the cited dataset provides sea-ice thickness using a 1D surface energy balance model, and the authors note that uncertainties become large for thick ice. Why was this dataset used to initialise and provide boundary conditions for ice thickness, rather than an altimetry-based dataset? Were the same datasets also used for the lateral boundary conditions? In general, this model setup section could use more detail.

19. **L95–100**. The grid spacing is anisotropic in longitude and latitude. Please state the approximate physical grid spacing across the domain or at representative latitudes. Provide more information on bathymetry interpolation, smoothing, and the effect of the imposed minimum depth.

20. **L101–110**. Clarify which sea-ice concentration and thickness products are used for initial and boundary conditions. The code/data statement later lists APP-X sea-ice thickness, but the methods text is not clear. Correct "rive runoff" to "river runoff".

21. **L110–111**. Define "BD" when first introduced.

22. **L111**. Are three-year runs sufficient? Could this be justified here?

23. **L111–115**. Define precisely the temporal averaging used in the velocity threshold. Please state whether concentration and speed thresholds were tested for sensitivity. A model-configuration table, possibly in an appendix, would help the reader.

24. **Fig. 1**. Increase the text size and simplify the map. Some labels and boxes are difficult to read, and the large map labels obscure information. Please explain how the sea boxes were defined, whether they follow cited regional boundaries, and why the displayed bathymetry/contours are relevant to the basal-stress parameterisation.

25. **L121**. Why are snapshots presented from the second model year? Wouldn't the third year be more appropriate, allowing additional time for spin-up?

26. **L125**. The phrase "as air temperatures decline" is misleading here. Air temperature does not create pressure ridges, which are the key mechanism for fast-ice stabilisation. I suggest removing this aspect.

27. **L122–128**. Replace "highly realistic" with a more measured phrase, or support it with quantitative metrics.

28. **Fig. 2**. The panels are too small. Consider enlarging the figure, splitting it into fewer panels, using a regional subset, or moving some snapshots to supplementary material. The caption should explain what is meant by "typical days" and should define all plotted fields. The red fast-ice shading obscures some concentration information.

29. **Fig. 3**. The colour scale for occurrence and difference should be easier to interpret. Please state whether occurrence is a fraction or percent; the figure and caption mix conventions. The caption should be self-contained and include the analysis period, data sources, and definition of the difference panel.

30. **L130–131**. "Following the method of Yu et al. (2014)" is too vague. Please specify the method and explain why January–May was selected.

31. **Fig. 4**. Clarify that grounded and ungrounded components are from the BD simulation only. The caption should define exactly how these components are diagnosed. The plotted colours and the text at **L169–171** are inconsistent.

32. **L153–156**. The shorter simulated fast-ice season is an important result. Quantify onset and breakup biases by region and discuss whether this is related to thermodynamics, thickness, snow, forcing, bathymetry, or the velocity diagnostic.

33. **L156–160**. The statements that short-term variability is "likely attributable to synoptic scale atmospheric variations" and that the Kara and East Siberian Seas are "more responsive to atmospheric forcing" require supporting diagnostics. Please substantiate these statements with atmospheric forcing analysis or soften the wording.

34. **L169–171**. The text refers to ungrounded landfast ice as the blue line and the CTRL run as the green line, but the caption states BD is red, CTRL is blue, grounded is magenta, and ungrounded is green. Please correct this inconsistency.

35. **Fig. 5**. The caption is not sufficiently informative. This figure may be better represented as a small table or combined with **Fig. 4**. If retained, define the averaging period, regions, units, and meaning of the ratio labels. The legend is confusing. Do both colours represent basal drag?

36. **L176–179**. Replace "proportion of grounded landfast ice to the total" with clearer wording, for example "the fraction of simulated landfast-ice area for which basal stress is active".

37. **Fig. 6**. The caption needs a clearer definition of occurrence, period, colour scale, and contour. Use "January–May" rather than "Jan to May", and write "20 m" with a space.The text is illegibly small.

38. **L194–198**. Averaging signed stress components over a spatially heterogeneous region can lead to cancellation. Consider reporting stress magnitudes or stress components projected onto the wind direction and/or the local fast-ice-edge normal.

39. **Fig. 7**. The axes and legend are difficult to read. Specify whether the plotted stress is an area mean of vector components, a vector magnitude, or a directional component. Explain why the average is taken over non-zero basal-stress areas if the breakout analysis focuses on ungrounded edge regions. The colours used for wind stress and water stress are very similar. The data should also be truncated on the left.

40. **L203**. Use consistent capitalisation in section headings; "Landfast Ice" should not be capitalized here unless used as a formal title style throughout.

41. **L204**. Are these the only four breakout events across the three winters? What time period does each winter cover? Does the reported 8% refer to the entire fast-ice area, or only a more localised region?

42. **L208**. I would argue that only the first three events are truly "captured" by the model, based on Figure 8.

43. **L205–209**. Justify the 8% threshold used to define a breakout event. State whether other thresholds were tested and whether conclusions are sensitive to this choice. Clarify whether events were identified from NIC, BD, or both.

44. **Table 2**. Use consistent scientific notation and spacing, e.g. $3.77 \times 10^5\,\mathrm{km}^2$. The column "Area of basal stress in breakout region" has very small values relative to breakout area; please clarify whether these are instantaneous areas, time means, or integrated areas, and how they are computed on an approximately 2 km grid. Avoid excessive precision where it is not physically meaningful. The last column is confusing. Should it instead read "Area of broken-out region with non-zero basal stress"?

45. **L218–223**. The wind-direction description is confusing: “southwesterly winds prevailed during the first three breakout events, and southwesterly prevailed during the fourth”. Please rewrite. Also clarify how “offshore” is defined for each breakout region.

46. **Fig. 8**. The panels are too small to support the text. The caption should explain how breakout areas were calculated from pre- and post-event masks, whether the masks are NIC-derived or model-derived, and how the colour categories are defined.

47. **Fig. 9**. The wind roses are too small to read. Consider replacing or supplementing the figure with a table of directional statistics, including mean wind speed, maximum wind speed, and the offshore component of wind speed or wind stress. Some of the text is illegibly small. The border appears unnecessary. Why is this not presented as a vector plot? For BK4, is the wind rose calculated across both dark blue regions? If so, that is probably undesirable, as the offshore wind direction differs between the left and right breakout regions.

48. **L231–235**. Provide the cyclone identification/tracking algorithm. If tracks were manually identified, state this explicitly and describe the criteria.

49. **Fig. 10**. The trajectory panels are difficult to read. The caption lacks a colon after “Figure 10”, and the figure should define the colours, dates, symbols, and event correspondence more clearly. This figure should use the same four-column layout as the earlier breakout figure. It is also difficult to interpret in the context of the earlier plots because it uses a polar rather than Cartesian projection, and the fast-ice breakout location is not marked.

50. **Fig. 11**. The figure is too small to resolve the wind vectors and pressure contours. The black boxes are not described. The caption should include the event dates, the meaning of the boxes, and whether the snapshots are instantaneous 6-hourly fields or daily means. Similar comments apply as for Figure 10. The projection changes again, and the fast-ice breakout is not annotated, making the figure difficult to interpret. How were the snapshots selected within the week-long window? They may not be representative of the winds over the full week, as storms move substantially during that period. Were the snapshots chosen arbitrarily?

51. **Fig. 12**. This full-page figure is difficult to interpret despite occupying substantial manuscript space. The caption does not define the colours, the derivation of “daily detachment”, or the relationship between daily panels and the biweekly fast-ice diagnostic. The authors should redesign, move to supplement, or replace it with a more quantitative event summary.

52. **L245–267**. The event-by-event narrative is lengthy but not sufficiently quantitative. Consider condensing the prose and supporting it with a table containing cyclone timing, minimum pressure, maximum wind speed, offshore wind-stress component, pre-breakout fast-ice area, breakout area, and basal-stress-active area.

53. **L251**. “Event” should not have a capital “E”.

54. **L251–254**. The phrase “large-scale sys Sea and western East Siberia Sea” is broken and should be corrected.

55. **L263–267**. The figure references for the fourth breakout event appear to be incorrect; they should presumably refer to panels d in Figures 10–12, not Figures 10c–12c.

56. **L268–270**. The statement that the events were “primarily driven” by winds is plausible but should be softened unless supported by a force-balance or stress-threshold analysis. Also, below-freezing 2 m air temperature alone does not fully rule out thermodynamic preconditioning; this claim should be qualified.

57. **L274**. I think Liu et al. (2022) showed that neither fine spatial resolution nor the resolution of fine-scale topography is required for accurate landfast-ice simulation. I therefore disagree with this statement.

58. **L275**. The manuscript does not demonstrate that runoff or salinity are important for landfast-ice breakout.

59. **L278**. This is not the appropriate place to mention that the lateral drag parameterisation was not used. That discussion should appear in the introduction.

60. **L274–280**. The claim that the fine grid "accurately resolves" coastal topography and small islands should be softened. The statement that lateral drag is unnecessary because coastlines and islands are resolved should be discussed as an assumption and limitation.

61. **L281–294**. Several language and terminology issues occur here: "indicts" should be "indicates", "grounding ice" should be "grounded ice", "ungrounding ice" should be "ungrounded ice", and "This differences arises" should be revised. The reference to Eq. (1) for the grounding scheme appears to be incorrect and may need to refer to Eq. (2).

62. Figure 13 and **L300** onward. There appears to have been a large breakout event that is not included among the four examples. Are BK1–BK4 simply the "good" examples where the simulation agrees well with the observations? This gives the impression of cherry-picking favourable cases.

63. **L304**. What constitutes "a large seabed elevation"?

64. **L305**. What is the "submarine bulge"? Please indicate its location on the map.

65. **L305–308**. Define the high "P-value" of 22460, give units, and explain how it relates to the standard sea-ice strength parameterisation used in the model.

66. **L307**. "Like many other models" requires supporting references.

67. **L309**. The tuning of k_1 and k_2 belongs in the Methods section rather than the Discussion.

68. **L309–316**. Use consistent notation for the basal-stress parameters. The sensitivity to k_1 should be shown or summarised, not only mentioned qualitatively. The phrase "during the four landfast ice breakout events for examples" should be revised.

69. **Fig. 14**. The caption is not self-contained. Define the shading, vector units, dates, panels, and whether the plotted field is stress magnitude, vector stress, or a component.

70. **L324–334**. The conclusion should be grammatically corrected and softened. For example: "These events occurred along the outer edge of the landfast ice, where basal stress was inactive, and coincided with cyclone-driven offshore winds." Avoid repeating causal claims that are not yet fully supported.

71. **L337–344**. The code and data availability section should include the exact model configuration, namelists, code modifications, preprocessing scripts, analysis scripts, and sufficient output diagnostics for reproducibility. There is also no space after "MITgcm."

72. **References.** Check all references carefully. Examples include inconsistent spelling of Bogardus, Whitefield/Whitfield, malformed DOIs, inconsistent capitalization, missing punctuation, and spacing artefacts such as "Pan- Arctic Ice- Ocean", "land- fast ice", "A rctic", and "L aptev S ea".

Suggested additional analyses

The following additions would substantially strengthen the manuscript:

- a regional skill table comparing CTRL and BD against NIC for area, occurrence, onset, breakup, and edge position;
- a sensitivity table for k_1, k_2, and the ice-strength parameter P^*, at least over one representative winter;
- a clear map of BD–CTRL landfast-ice occurrence difference, separate from BD–NIC, to show the dynamic impact of basal stress;
- event-skill statistics for breakouts, including missed observed events and false simulated events;
- wind-stress decomposition into along-edge and offshore components for each breakout event;
- a compact table of cyclone diagnostics for each event: timing, minimum sea-level pressure, maximum wind speed, peak offshore wind stress, and pre-event ice state;
- a short limitations subsection discussing tides, ocean stress, bathymetry uncertainty, unresolved coastal geometry, diagnostic temporal averaging, and the three-year simulation period.

Overall conclusion

The manuscript addresses an important topic and contains results that are likely to be of interest to the sea-ice modelling and Arctic coastal-process communities. However, the present manuscript requires **major revision** before publication. I would support reconsideration after the authors provide quantitative validation, clarify the fast-ice and breakout diagnostics, correct the basal-stress formulation and parameter documentation, revise the interpretation of grounding contributions, strengthen the breakout-event attribution, improve code/data availability, and substantially improve the language, captions, and figure readability.