Response to Anonymous Referees #1 and #2

Holly Still and Christina Hulbe

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We would like to thank both referees for providing additional feedback to improve the revised manuscript. The reviewers' comments are in black and our responses are in blue. Excerpts from the improved manuscript are italicised.

Comments from Reviewer #1

Comment related to original line 118-125 "grid resolution". I would still discourage the use of "resolution". If you don't like "posting" how about "grid spacing".

We have changed resolution to grid-spacing when referring to interpolated data sets.

Line 149 comment. Perhaps I am missing something, and it's a minor nit, but if you have edge lengths of 500 m, then you can resolution down to ∼500m. For example, you could have 4 points forming a 500x500m square (2-triangles) in your mesh, which would be somewhat equivalent to a 500-m regular grid. So in that sense it "ranges from 500m to ..." Not 1000 m.

We have clarified that this is an "unstructured triangular mesh" and improved the relevant sentence:

"Mesh element size ranges from approximately 1 km edge lengths over pinning points, ice-stream shear margins and the grounding line, to approximately 10 km edge lengths over the inland ice sheet and central ice shelf."

Comments from Reviewer #2

Connection between pinning points and grounded bed properties

Extending on the comment 'proposed feedback' (2). Thank you for reformulating this and making clear that this is a hypothesis.

Line 360 - 367, 423: To make this clearer for the reader, I think you should add more discussion including your points from the reply, e.g., 'The existence and strength of this effect can not not be deduced from the experiments presented here. Alternative explanations for the different basal properties of both ice streams exist, such as differences in basal hydrology.'

We think that our writing is clear (thank you for the positive comment about the writing) and agree that a bit more could be said. While different basal materials or states would not discount the hypothesis, they would make it more complicated to investigate. We have added the following sentence:

'A model in which basal till properties are coupled to the basal hydrologic system would be required to investigate this hypothesis further."

Basal friction adjustment

Figure 3: Which color corresponds to the selected value of α = 200?

Excellent point. The α = 200 lines have been identified using a label on the figure.

Line 196, lines 438-442: Looking at the ice rumple morphology in Figure 3 in comparison with Bedmap2 surface elevation and observed ice velocities, I find it actually hard to say that any of the lines for fixed α is better than the initially inferred friction coefficient for all three examples shown, except for the regions where you find zero friction in the inversions. Or did you base your statement on a different argument?

We agree that it would not be sensible to use only these profiles to make a decision. As reported in the text, we used a regional, 2-D, evaluation and a comparison with results from a force budget method. It is not practical to show all of those maps in a figure and we think that the suites of profiles in Figure 3 show what is happening, mechanically, among the coupled variables as adjustments are made to α. This fits with the point of view and objectives of the study and the paper.

Line 197: using the force budget method to argue that the inverted basal friction is problematic could be strengthened by a discussion of the uncertainties related to inferring basal velocities in the light of findings by Bahr et al. (1994) and others.

We rely everywhere on an inversion using the shallow-shelf equations. Basal parameters found in this way are similar to those found by inversion of full-Stokes equations, except where ice thickness gradients are relatively large and the SSA assumption is too simple (Morlighem et al. (2010), comparison of SSA and full-Stokes inversions).

The ice rumples are such a location. Comparing basal drag resulting from the two approaches, the initial ISSM inversion gives an average basal drag of 14 kPa for the SCIR elements (most of which have $\tau_b = 0$) while the force budget approach gives 51.6$\pm$18.3 kPa. The scale of the difference is similar to differences across approaches in Morlighem et al. (2010). A reasonable conclusion is that the FS equations should be used not only at grounding lines, as those authors suggest, but also over ice rumples.

We have added to the Discussion, line 401: *"Different momentum equations, for example, full-Stokes instead of the SSA, will also yield different results (Morlighem et al., 2010), particularly where thickness gradients are large, and this may explain the difference between our ISSM-inversion and force budget inferences of the basal friction parameter."*

Line 180-187: I think this comment was lost from my first review: Please add that it could also be that the inferred friction is okay but inconsistencies in the basal or surface mass balance or other factors can causes the (undesired) changes during the relaxation period.

The inferred friction was undesirable before it had undesirable effects on the ice thickness during relaxation. We do agree that limited knowledge of other boundary conditions does not help this situation. For example, a too large basal melt rate could also lead to ungrounding during relaxation.

Further comments

Line 2: You do not name any study in your introduction that analyses the role of pinning points using buttressing numbers and I am not aware of such a study.

In the Introduction, line 43 we stated: *Flow buttressing numbers (Gudmundsson, 2013; Fürst et al., 2016) provide a summary view of the non-local effects but do not quantify the pinning point contribution to individual resistive stresses.*

In their Supplementary Material, Fürst et al. (2016) compute along-flow and σ_2 buttressing numbers for the Larsen C Ice Shelf. They simulate the 'ungrounding' of Bawden Ice Rise by setting the basal friction coefficient to zero at that location, and they find that loss of the ice rise increases the area of passive shelf ice (i.e., modifies the maximum σ_2 buttressing number near the shelf front).

Additional authors who compute buttressing numbers with pinning points included in their model domains and acknowledge that pinning points contribute to the flow buttressing pattern:

- Kingslake et al. (2018) compute flow buttressing numbers using the method of Fürst et al. (2016) to assess the relevance of buttressing associated with ice rise formation in the Ross and Weddell Sea sectors.
- Borstad et al. (2013) consider the consequences of the Larsen C Ice Shelf losing contact with Bawden Ice Rise. They compute a buttressing parameter f (a normalised measure of the backstress introduced by Dupont and Alley (2005)) and find that Bawden and Gipps Ice Rises contribute locally high backstress which they describe as 'overbuttressed' with $f > 1$ (compressive flow).

While Gudmundsson (2013) provide a good overview of buttressing number theory, pinning points are not the focus of this paper. We will swap this for a reference that specifically computes buttressing numbers and considers the pinning point contribution. *Flow buttressing numbers (Borstad et al., 2013; Fürst et al., 2016) provide a summary view of the non-local effects but do not quantify the pinning point contribution to individual resistive stresses.*

Line 13: 'without feature-specific adjustment' → 'without feature-specific, a posteriori adjustment' to make clear that you did not change the inversion process.

Updated, thank you.

Line 19: I'm missing some kind of implication, conclusion or outlook at the end of the abstract.

We have moved a sentence to the end of the Abstract and edited it slightly. The sentence *"Where pinning point effects are important, model tuning that respects pinning point morphology is necessary to represent the ice-sheet and ice-shelf system as a whole"* has been changed to:

Where pinning point effects are important, model tuning that respects their morphology is necessary to represent the system as a whole and inform interpretations of observed change.

Line 27: Thanks for addressing this. To be clear here, I meant to replace 'flow-buttressing' simply with 'buttressing' as flow buttressing is only calculated in the study by Furst et al.

We have changed 'flow-buttressing' to 'buttressing'.

Altogether, the rate of mass flux is moderated in an effect commonly referred to as 'buttressing' exerted on upstream grounded ice by an ice shelf (Dupont and Alley, 2005, 2006; Gudmundsson, 2013; Fürst et al., 2016).

Line 32: where do the perturbations come from? Or do you mean 'a' or 'any' perturbation?

Thank you, we have changed this to:

Any momentum and mass perturbation must be balanced by changes in thickness and resistive stresses elsewhere in the ice-shelf and ice-sheet system.

Line 43: simply 'buttressing'

Corrected.

Line 55: Your approach, to model a system with and without the ice rises, is very similar to the approaches by Goldberg et al., 2009 or Favier et al., 2012. Your approach is different in that it analyses in detail the stress patterns involved.

Line 55: *"We take a different approach to examine the dynamical role of pinning points."*

Has been changed to:

"We take a different perspective to examine the dynamical role of pinning points that involves a detailed analysis of the stress patterns across the RIS. Our aim is to quantify..."

Line 71: You could add a quick overview over the structure of your manuscript here.

We have thought about this but the paper has a very standard structure and we could not find a way to do it that did not seem repetitive.

127: I'd suppose that you are using a combined velocity data set of both and not are running inversions for the two datasets (or even different points in time) individually?

We have added an additional statement:

Surface velocities are from the 750 m grid-spacing Landsat 8 dataset (Fahnestock et al., 2016) and the 900 m grid-spacing MEaSUREs dataset (Rignot et al., 2011) representing time periods from 2013 to 2016, and 2007 to 2009, respectively. The two velocity datasets are merged, with the MEaSUREs dataset used to fill the region beyond the Landsat 8 latitudinal limit.

Line 181: 'do not yield a realistic ice rumple geometry' → 'do not yield a realistic ice rumple geometry after the relaxation simulations.' (because the inversion itself does not affect ice geometry).

Corrected.

Line 224: The name of the section does not fit with the content (basal drag, driving stress included) anymore.

We have changed the original section subheading '*3.3 Partitioning of resistive stresses*' to '*3.3 Stresses*'.

Line 236: That such a geometry is required to maintain the flow is stated at multiple locations. Maybe not required everywhere?

We have deleted the statement *'This geometry is required to maintain mass flux past the obstacles'* in Section 3.3.1. We have left the idea in Section 3.1 where it is first introduced.

Line 240: that the change in driving stress and mass flux drives grounding line retreat is a bit unprecise here, because the stress changes you plot are with respect to steady states, so the grounding line retreat and thickness changes will have influenced the stresses as well. You could say 'are in line with'.

Thanks, we have made the suggested change.

Line 349: What do you mean with Ross Ice Shelf stability? And what do you mean with an 'unstable ice shelf configuration is required for irreversible grounding line retreat'? The studies by Weertman and Schoof are both based on passive ice shelves which would be equivalent to the absence of an ice shelf. Do you mean that the ice shelf is required to provide only little buttressing?

We have replaced 'Ross Ice Shelf stability' with 'the present-day configuration of the RIS'.

We meant for these references to apply only to MISI type retreat but agree that this is unclear. The paragraph has been re-written:

This finding alone does not reveal how important the SCIR are to maintaining the present day configuration of the RIS. If stability is associated with grounding line position, then the simulations imply that the SCIR are unimportant (SCIR removal results in a transition to a new steady-state), despite the relatively large flow resistance they provide. This is due to a regional redistribution of ice thickness and resistive stresses. The redistribution, which itself depends on embayment geometry, moderates the sensitivity of the coupled ice-sheet and ice-shelf system to the ice rumples. Similar redistributions should be expected for changes to other, individual pinning points. If stability is associated with crevasse and rift formation (Bassis and Ma, 2015; Borstad et al., 2017; Lai et al., 2020), the SCIR generate shear and tensile stresses that form crevasses and therefore the removal of pinning points may be expected to improve stability, although changes to shear stresses elsewhere may promote crevasse and rift formation in those locations.

Line 357: Please explain better: I'm not sure I understand how stability is related to ice shelf thickness in the study by Gudmundsson et al. (2019)?

We thought that Gudmundsson et al. (2019) was quite relevant:

"Our results have important implications for assessing future mass loss from the Antarctic ice sheet and resulting sea-level rise: Thinning of ice shelves is now causing a significant increase in discharge of grounded ice into the oceans; because this process is almost instantaneous, we are not protected against the impact of the Antarctic ice sheet on global sea levels by a long response time." (Gudmundsson et al., 2019)

We revised and simplified the paragraph on stability and the Gudmundsson et al. (2019) sentence and reference have been removed.

Modified figures

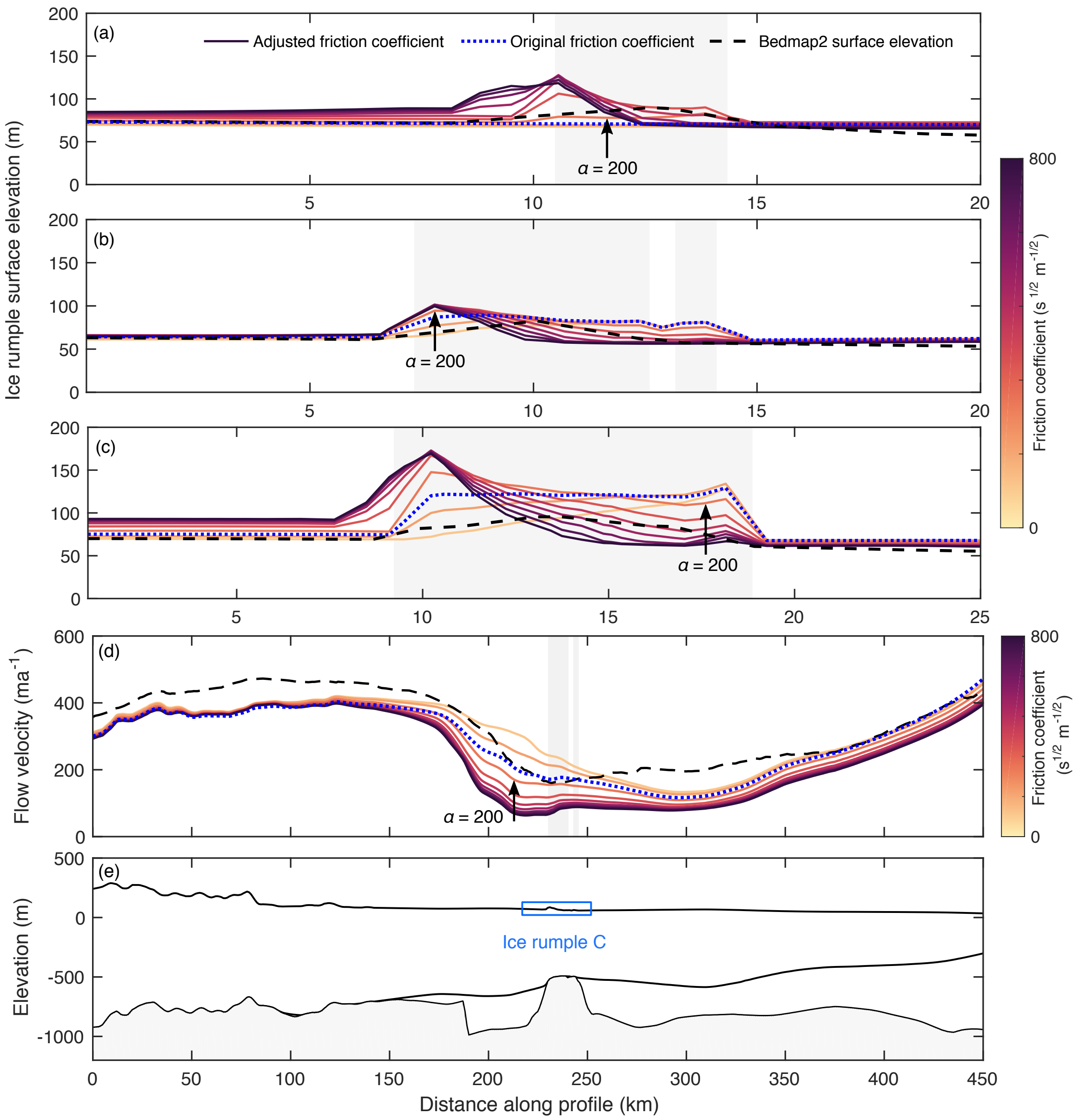

Figure 1: Surface morphology and ice velocity for different basal friction coefficient α values assigned to SCIR model nodes. Along-flow surface elevation profiles in panels (a), (b) and (c) demonstrate how selection of the friction coefficient before model relaxation affects ice thickness and surface elevation for three ice rumples in the SCIR complex (ice rumples A, B and C, respectively, see Fig. 1b for their location). Grey shaded boxes indicate model nodes where the ice shelf is grounded. Panel (d) demonstrates how selection of the friction coefficient affects the velocity magnitude. The profile in (d) represents a single pathway that begins 150 km upstream of the MacIS grounding line, intersects the SCIR rumple C, and ends at the shelf front. Panel (e) shows ice thickness and the underlying seafloor along this pathway in the reference model. The locations of the profiles in (a) to (e) are mapped in Fig. 1b.

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