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The Cryosphere Discussions

Interactive comment on "Mechanics and dynamics of pinning points on the Shirase Coast, West Antarctica" *by* Holly Still and Christina Hulbe

Anonymous Referee #2

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In their study, Still & Hulbe analyze the mechanical impact of ice rumples off the Shirase Coast on the ice flow of Ross Ice Shelf and upstream. They use the ice flow model ISSM and initialize the model using optimization techniques to infer basal friction and ice softness for a regional setup of the Siple Coast and Ross Ice Shelf. The influence of the ice rumples is tested by first running the model into steady state with ice rumples present and then doing perturbation experiments. In the simulations, surface mass balance is based on Vaughan et al. (1999) and basal mass balance is based on a linear, depth-dependent parameterization like in Martin et al., 2011 with parameters adjusted to keep the grounding line position close to its present day location during the relaxation simulation. For the initial steady state the basal traction inferred for the ice rumples is adjusted so that the geometry of the rumples in steady state compares

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with observations. Furthermore, a constant ice softness field is used instead of the inferred field to test the robustness of the results. In the perturbation experiments, the ice rumples are removed by digging them away in the topography, and the model is then integrated forward for 150 years and the response in driving stress, longitudinal stress, lateral stress, ice speed, ice thickness, grounding line position and shear strain rate is evaluated. This design of the perturbation experiments is a useful approach to test the effect of pinning points on ice dynamics. In addition, a force budget method is used on the SCIR and Roosevelt Island in the initial state and the perturbed state after 150 years.
The study is very detailed in the analysis of the stress changes, which makes it easy to lose track of the main results and conclusions and how they are reached. I think that a clearer argumentation and refocusing of the main findings would be very beneficial for the paper and make it an interesting contribution to The Cryosphere.

Major comments

- **Proposed feedback.** This comment concerns lines 9-10, 329-337 and 378-384. (1) I'm not sure I fully understand the proposed feedback: higher backstress from a pinning point is suggested to increase the ice thickness of the ice stream, thereby increasing the driving stress and basal drag which is then reflected by an increased occurence of sticky spots at the base of the ice stream, making the ice stream less responsive. But to close the loop, the last effect has to feed back on the backstress generated by the ice rumples. How does this work?
(2) I don't understand how the proposed feedback between pinning points and basal traction of ice streams can be deduced from your experiments. In your invsersion you find higher basal friction coefficients in MacIS in comparison to BIS. But if those arise due to the presence of the SCIR cannot be singled out. It could also be that it is the local ice velocity together with the ice thickness field that determines the occurrence of sticky spots in the inverted basal friction

coefficient. This is not to say that it might not be possible, but I do not understand how the conclusion 'In the model, the larger basal drag acting on MacIS is itself, via regional changes in driving stress, a consequence of the coupled ice shelf and ice stream response to the SCIR.' can be drawn from the experiments presented in this study.

- **Basal friction adjustment and ice rise morphology.** This comment concerns lines 10-13, 345-346, 386-388 and Figure 3.
(1) In the study, after the inversion procedure, basal friction coefficients of the ice rumples are adjusted in the relaxation simulations. I suppose that this is motivated by large-scale change in the ice rumple morphology when using the inverted basal friction coefficients in the relaxation runs? This would be interesting to extend on, and add the results of the relaxation simulation in Figure 3. Also it would be interesting to see how the overall results of this study would be affected by using the initially inverted basal friction fields.
(2) Overall, a wrong morphology of the ice rumples after the relaxation simulations does not necessarily imply that the inversion produced wrong basal friction values as implies by statements in lines 10-12 and lines 368-388. It could also be that inconsistencies in the basal or surface mass balance or other factors causes a thinning, thickening, grounding or ungrounding of the ice rumples during the relaxation period. Don't get me wrong here, I think that your ad-hoc approach to correct the basal friction coefficient is ok. But I think that this should be discussed further and I'd be careful to blame the wrong morphology on the inverted friction coefficients alone. This should be extended on in the discussion.
(3) Please be more clear: It is stated that 'by extension, any parameter that is affected by the initialization procedure' is represented incorrectly in lines 12-13. What parameters do you mean?
Similarly, in line 345 it is stated that ' the present work demonstrates the role of pinning points in parameter selection during model initialization.' Please explain

more: What parameters are selected during the initialization procedure that are affected by the pinning points? Which role do the ice rises play? How is this shown in this study?
Furthermore, in line 389-390 is stated that ‘The incorrect representation of pinning points also has implications for the inference of model parameters upstream of the grounding line during model initialization.’ My understanding was that you did run the inversion to infer model parameters of basal shear stress and ice softness upstream of the grounding line based on the Bedmap2 geometry in which pinning points should be correctly represented. Or what do you refer to here?

- **Formulations**. Being more precise with your statements would make it easier for the reader to follow your ideas. For example in line 245 (‘In general, the SCIR act to reduce longitudinal tensile stresses in grounded ice upstream of their location.’): in your next sentence you already mention that this is only partly true depending on the ice softness field used. Here you could directly go to the specific result or you should discuss why the general statement you made before (best supported by literature if it is not textbook knowledge) is actually not true in your experiments. See also comments on lines 293, 309, 319.

- **Figure captions**. Figure captions should give all relevant information on what is shown in the figure. For example, sometimes grounding lines are shown, but it is not indicated if this is an observed position or the position obtained in the relaxation simulation or in the respective experiment. In addition, the appropriate grounding lines should be displayed in figures that are interpreted to show changes at the grounding line or upstream (Figs 5,6,7,8,11,12). See also specific comments to the figures.

Further comments

- Line 9-10: see major comment.

- Line 10-13: see major comment.
- Line 15: 'transient' changes in ice shelf geometry in constrast to 'persistent' changes in ice streams. I'm not sure I understand this statement as the changes in ice thickness and speed that you present in Figures 9 and 11 are visible in both, the ice shelf and the ice streams.
- Figure 1: Add Echelmeyer Ice Stream as you are referring to it later on.
- Line 21: Another interesting study analyzing this is done by Pegler in 2018 ('Marine ice sheet dynamics: the impacts of ice-shelf buttressing').
- Lines 24 and 61: A bit of care with the wording should be taken here. The term 'flow-buttressing' has been used previously in Furst et al. It calculates the buttressing parameter by selecting the ice flow direction as a normal direction. However, the ice flow direction can be very different to the normal direction at the grounding line which is used in Gudmundsson 2013 to calculate a buttressing parameter.
- Lines 65 and 118: 'models' $\rightarrow$ 'model configurations' as ISSM is only one model?
- Section 2.2: How is the basal friction parameter set in regions that are not grounded during the inversion but that ground during the transient forward simulations? How is basal friction treated in elements along the grounding lines?
- Line 163: Why 150 years?
- Figure 2: It would be helpful to add here that also B_u is shown in the colorbar of panel (a).
- Figure 3: It would be helpful to have (a) also the surface and velocity profile obtained with the inverted basal friction coefficient in the panels and (b) the mag-

nitude of the optimized coefficient (e.g., averages along the lines). In addition, in panel (d) is the grey box showing the grounded regions in Bedmap2?

- Line 149: Is the same time stepping also applied in the perturbation experiments? If yes, how is the snapshot after 1 year shown in Figure 9(a) obtained?
- Line 175: How is the morphology for the different friction coefficients obtained? I suppose that the 1000years relaxation was run with different basal friction coefficients for the ice rumples?
- Line 184: How does the optimized value compare to this value (see also comment on Figure 3)?
- Line 192: Fig S2.
- Figure 4: Is this an instantaneous velocity difference or a difference obtained after running the relaxation for 1000years with the corresponding basal friction coefficient? What grounding line position is shown? If it is not shown here, it would be helpful to show the final grounding line positions after the relaxation runs to see how the ice rumple geometry is affected by the adjustment.
- Section 3.2: What is your main finding or conclusion from this comparison in relation with the later chapters? I think that it would help for the following chapters to analyze the difference and similarities between the results in light of the robustness of the results.
- Line 221-222: This statement seems to be true for the largest of the ice rumples but not for the smaller, second-largest one to the left?
- Line 222-226: Since the figure does not show a grounding line position, it is hard to say, but from a rough estimate it does not look like driving stress along the grounding lines of the glaciers main trunks change significantly? It would

be helpful if you (1) add the grounding lines in the Figure and (2) add the driving stress changes in Table 3. In addition, do you know what the blue spots in MacIS in the B_u case are? Could they be numerical artifacts in individual mesh elements?

- Line 227: How do you conclude that changes in flow buttressing are equal in both cases?
- Line 235: Is this pattern consistent with the location of sticky spots and topographic features?
- Line 244: Not sure I understand this statement, Figure 8 shows particularly high lateral shear stress in this area?
- Line 244: 'exaggerated' $\rightarrow$ 'stronger'? Since B_{inv} is obtained through inversion, I would expect the velocity and ice softness field to be closer to present-day than for the ad-hoc assumption of constant B_u. Thus, I would think of B_{inv} as the reference simulation and B_u as a test case to support robustness.
- Line 247: Fig. 6 $\rightarrow$ Fig. 7.
- Line 272: 'increase divergence downstream of their location' - this seems to depend on the ice softness and there is a large spot of decreased divergence (red) directly next to the rumples (on their western side) and downstream?
- Section 3.3.5: Maybe move this earlier so that you define 'sticky spots' before you discuss them in Section 3.3.2.
- Figure 9: which case is shown here, B_{inv} or B_u?
- Line 285-286: It is really hard to tell from Figure 9 in which ice stream the speed increases more after 150 years. Would maybe be helpful to point to Table 3 here

and add also absolute and relative speed changes along the glaciers grounding lines (and maybe move figures to the SI). In addition, it would be interesting to have an estimate of how far speed changes extend inland for both glaciers.

- Section 3.4: I suggest to move this section before the changes in stresses are discussed to give the reader first an idea of how thickness, grounding line position and velocities change which then also makes it easier to interpret them with respect to changes in stresses.
- Line 293: Please be more precise here. How does this feedback work?
- Line 296-303: That you find an immediate slow-down upstream of the ice rumples is surprising and interesting to me. I think that your explanation that the initial slow will be reversed once the ice thins in the location of the ice rumples could be supported more: you could do an additional, simple experiment in which you do not only remove the ice rumples in the topography but also thin the ice at their former location so that the perturbed ice shelf is flatter (i.e., using the thickness distribution after 5 years) and then compare the instantaneous response. If the response is similar to your current 5 year response, then the initial response can most likely be linked to the initial thickness distribution in your perturbation experiment.
- Line 309-311: Be more specific here, what do you mean with 'the fundamental mechanism are generic'?
- Line 310 'mechanics and dynamics' $\rightarrow$ 'mechanics of Ross Ice Shelf'?
- Line 319-320: Be more precise here. A redistribution of mass from where to where? Is it large or small? And how do the pinning points affect the efficiency?
- Discussion: Discussion should be extended to include also a discussion of the model choices done here (e.g., sliding law), potential drawbacks and limitations

of the methodology (e.g., assuming that present-day Ross Ice Shelf and the Siple Coast Ice Streams are in steady-state).

- Line 329-337: I'm not sure I understand this feedback, see also the main comment.

- Line 335-338: This sentence could be misunderstood to indicate that the studies of (van der Wel et al., 2013; Hoffman and Price, 2014) investigate a physical coupling between pinning points and ice stream basal properties (none of the studies includes dynamic ice shelves).

- Line 338: Looking into Table 3, the relative mass flux increases following SCIR removal of MacIS and BIS seem quite close when comparing it to other ice streams listed. I agree that it is interesting that BIS shows a similar and slightly higher response than MacIS which is located more directly upstream of the SCIR, but calling it a 'contrast' is maybe a bit too much.

- Lines 345-346: See major comment.

- Conclusions: in this section it would be great if you could put your findings into a broader context, e.g., discussing the vulnerability of the SCIR in a changing climate and the implications of your work in this context.

- Line 387-388: This could be misunderstood to mean that you did apply the feature-specific tuning during the inversion and not after the inversion. The second part of that sentence could be misinterpreted to state that the ice rumple morphology influences the overall results of this study, but this is not shown, as the results from Figs 5 to 12 are all done using the same basal friction coefficient for the ice rumples.

- Line 388-390: see major comment.

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- Figure 10: Please also add the formerly grounded region in background of panels a and b.
- Fig S5: What is shown in the background of the figure?

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