2nd review of 'Mechanics and dynamics of pinning points on the Shirase Coast, West Antarctica' by Holly Still and Christina Hulbe

I would like to thank the authors for considering all the comments thorougly! The updated manuscript addresses most of my comments from the first review. I think that the manuscript is in a good shape now except for some remaining remarks listed below. I'm supporting the publication of this manuscript after they were addressed. Line numbers refer to the revised manuscript (not the track changes version).

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 explanantions for the different basal properties of both ice streams exist, such as differences in basal hydrology.'

Basal friction adjustment

Many thanks for addressing the points here! Some follow up questions:

- Figure 3: Which color corresponds to the selected value of $\alpha = 200$?
- 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?
- 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.
- 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.

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.
- line 13: 'without feature-specific adjustemnt' → 'without feature-specific, a posteriori adjustemnt' to make clear that you did not change the inversion process.
- line 19: I'm missing some kind of implication, conclusion or outlook at the end of the abstract.
- line 27: Thanks for adressing this. To be clear here, I meant to replace 'flow-buttressing' simply with 'buttressing' as flow buttressing is only calculated in the study by Fuerst et al.
- line 32: where do the perturbations come from? Or do you mean 'a' or 'any' perturbation?
- line 43: simply 'buttressing'
- 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 71: You could add a quick overview over the structure of your manuscript here.
- 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?
- 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).
- line 224: The name of the section does not fit with the content (basal drag, driving stress included) anymore.
- line 236: That such a geometry is required to maintain the flow is stated at multiple locations. Maybe not required everywhere?
- 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'.
- 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 irreversibe 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?
- 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)?

References

Bahr, D. B., Pfeffer, W. T., and Meier, M. F. (1994). Theoretical limitations to englacial velocity calculations. *Journal of Glaciology*, 40(136):509–518.

Gudmundsson, G. H., Paolo, F. S., Adusumilli, S., and Fricker, H. A. (2019). Instantaneous antarctic ice sheet mass loss driven by thinning ice shelves. *Geophysical Research Letters*, 46(23):13903–13909.