We thank the three reviewers and the editor for their suggestions for further improvements to the manuscript. Below, the reviewer comments are presented in black and our responses are in blue.

Anonymous reviewer #1

I thoroughly enjoyed reading this revised version of the manuscript. You have addressed my previous review comments and skillfully incorporated an explanation of the significant of the instantaneous response as opposed to a transient simulations.

Thank you for your time reviewing the new manuscript and your assessment of the changes we have made.

For final formatting it would be good to place Figures 3-6 earlier in the text so that it is easier to compare them with the corresponding results sections.

Thank you for this suggestion, we will make sure that this is the case in the final production version.

I spotted a few minor typos. Otherwise I recommend the manuscript is accepted.

Typos:

line 69: (outlined "alnd" labelled in Fig. 1)

Done

line 99: math format/italic (m)

Done

Lines 305-312: I think you need to end this paragraph by saying that 1m and 0.001m ice shelf would in reality provide very little buttressing and as such the difference between the thinning and calving experiments must be a result of the numerical method.

Thank you, this point was also highlighted by Clemens Schannwell. The opening of this paragraph now reads: "The maximum increase in GLF due to the thinning experiments does not equal that of the calving experiments (502% vs 607%), as in the thinning experiments a 1 m thick layer of ice remains across the ice shelf. This means that at the new calving front the ice thickness is linearly interpolated between the unperturbed nodes in the computational mesh and the neighbouring nodes with an ice thickness of 1 m. In the calving experiments mesh elements downstream of the new calving front are removed from the computational domain. We attribute the discrepancy between the maximum GLF increases to this difference in the numerical implementation of the calving and thinning experiments, and not to any residual buttressing effect of the 1 m ice layer."

Line 315: "the maximum basal melt rate was applied across the whole ice shelf" add *with no surface accumulation*

This paragraph has now been removed from the manuscript.

Anonymous reviewer #2

General comment

I want to thank the authors for having addressed most of my comments during their revisions. The current paper is nicely written and is well detailed. I also appreciate the new simulations they conducted in the Appendix to better emphasis some arguments from the main text. Yet, my main concern (as for my first review) is the significance of the instantaneous response of ice flow to a mass loss and the lack of a proper treatment of the transient effects following such event. I still think that this drawback limits the interpretation of the experiments, especially in the case of large ice-shelf thinning experiments that should not be treated as an instantaneous event (i.e. large thinning would most likely occur over a long-time scale, giving time to the upstream flow and geometry to adapt).

As the authors argue in their response to reviewers, the aim of this work is to identify the principal buttressing points of Larsen C Ice Shelf and their impact on grounding line flux when this buttressing is released — and, in this regard, the paper is very nicely wrapped. I also understand that the authors are currently conducting transient simulations and that such work can take time. However, I think that the current paper brings only limited insight in comparison to published literature, e.g. Furst et al. (2016) Reese et al. (2018), Gudmundsson et al. (2019) and Zhang et al. (2020) for buttressing, and e.g. Borstad et al. (2017) for the calving of A68.

The main conclusion of the paper, except the quantification of the real calving event of A68, which nicely couples modeling and observations, is that only a small portion of the ice shelf (close to the GL) really matters to buttressing. While this conclusion is in line with pre-cited studies, it does not really bring new insights about buttressing and the methodology is very similar to Reese et al. (2018) and Gudmundsson et al. (2019).

In the current study, the effect of the ice rise seems much smaller than in previous studies. For example, Zhang et al. (2020) also addressed the same question with an adjoint sensitivity (more reproductible and computationally much cheaper, in my opinion) in the context of ocean-induced melting on LCIS. Their results (particularly Fig. 12a) generally agree with Furst et al. (2016). In this regard, the current study results are more in line with Borstad et al. (2013) that see only a small effect of the ice-rise loss on the GLF. However, they observe a 25% change in velocity on the shelf, that, in a transient model, could have important effect. If the authors do not go with transient simulations, I think that they should better relate their paper to the previous literature (comparing results, etc).

For all these reasons, I had a really hard time deciding what recommendation to give for the paper. To me, the lack of transient experiment and the lack of novelty with respect to the published literature are a bit redhibitory. I therefore really recommend to the authors to pursue their effort towards transient simulations and include them in this paper.

We thank the reviewer for their time in providing another thorough review of the manuscript, and their suggestions for further changes that could be made. We hope we have addressed some of these points in this final round of revisions.

We do, however, disagree that our method does not bring new insights into ice-shelf buttressing on the LCIS. Whilst using a similar approach to that of Reese et al. (2018) and Gudmundsson et al. (2019), the diagnostic experiments we conduct allow us to calculate the total buttressing capacity of the ice shelf for the first time, which is not possible using the methods employed in those works and is a conceptual shift in assessing ice-shelf buttressing. It provides a new perspective on how much of the total buttressing different regions of the ice shelf, and the ice rises, provide, rather than assessing GLF sensitivity to ice-shelf perturbations alone.

In regard to our experiments on the loss of basal contact at the ice rises, we agree that our findings are similar to those of Borstad et al. (2013) but note that they do not model the grounded ice, and therefore cannot calculate the impact on GLF of a loss of contact at the ice rises as we do in this manuscript. We feel that these aspects of our results have been compared to the existing literature in section 4.2, including a comparison with the results presented in the supplementary material of Fürst et al. (2016) on the loss of contact at the Bawden Ice Rise.

Reese et al. (2018) and Zhang et al. (2020) find elevated GLF sensitivity to perturbations in ice-shelf thickness close to the Gipps Ice Rise, but they do not examine the loss of basal contact as we do, and are therefore answer a different question. Similarly, the map of buttressing number of Fürst et al. (2016) shows elevated values upstream of both ice rises, but we take a different conceptual approach in this work. By calculating the total buttressing capacity of the ice shelf, we find that whilst the GLF may be sensitive to perturbations in ice thickness in these regions, the removal of ice here does not significantly impact the GLF when considered against the total buttressing generated by the ice shelf as a whole. We agree that this comparison could be made more explicit and have now included an additional paragraph at the end of section 4.1 which discusses these differences.

Minor comments

Title: I would change “ice-shelf thinning” for “thinning” only to avoid a repetition.

This has been addressed in the revised title, which now reads: “The instantaneous impact of calving and thinning on the Larsen C Ice Shelf”.

Line 48- 63: I think that I think that the ABUMIP paper from Sun et al. (2020) should be cited in the context of this study.

The following sentence has been added to the introduction on lines 63-64: “Finally, Schannwell et al. (2018) and Sun et al. (2020) explored the transient response of the grounded ice to the complete collapse of the LCIS, and the associated removal of all ice-shelf buttressing.”

Line 55: I would change the sentence “Gudmundsson et al. (2019) modelled the impact of an instantaneous perturbation to Antarctic ice-shelf thickness, the spatial pattern and amplitude of which was taken from observations” for “Gudmundsson et al. (2019) modelled the impact of an instantaneous thinning of Antarctic ice-shelf on the grounded ice and GLF, with a pattern and amplitude derived from observations”.

Done

Line 58: change “for the last 18 years” for “from 1994 to 2017”. I would also reorder the sentence “[...] the instantaneous ice velocity response due to the observed ice-shelf thinning of the last 18 years, and the subsequent reduction in buttressing” as follows “[...] the instantaneous ice velocity response and the reduction in buttressing due to the cumulated observed ice-shelf thinning from 1994 to 2017”.

Done

Line 64 to 66: I thank the authors for clearly stating the “instantaneousity” of their experiments. I think that these two sentences could be reshaped in only one, more powerful sentence. For example, “a series of diagnostic perturbation experiments of increasing magnitude” is a bit vague and only gets clearer reading the second sentence. The literature they used to build their experiment is also not only related to LCIS (but the sentence seems to say the opposite, i.e. “existing literature on the ice dynamics of the LCIS”).

Thank you for highlighting this, whilst we have not combined these two sentences, the opening sentence of this paragraph has been changed to read: “Here, we build on this existing literature through a series of diagnostic perturbation experiments, including ice-shelf calving and thinning and ungrounding from ice rises.”

Line 70: correct the typo: “alnd”

Done

Line 101: Thank you for the addition of Appendix E. Should this be Appendix A, as it is the first you reference in the main text? I do not know what are the referencing rules for Appendix.

We feel that it makes more sense to include this sensitivity study later in the appendices (at least after the appendix on the model initialisation and mesh resolution) but will change this if the journal rules require it.

Line 128: reformat the references in only one parenthesis?

Done

Equation 7 and Appendix A: Why do you choose γ"#/% = 10)? I agree that your modelled velocities nicely fit observations so that might be a bit picky but the L-curve in Fig A1. (a,b) seems to show that γ"#/% = 10* is a better value, i.e. with the smallest velocity misfit. Similarly, I would be tempted to say that γ+#/% = 10 is a better value than 1. It also seems that you treat your L-curves independently but I assume that the choice of one parameter impacts the choice of the others, why not going with a multi-dimension L-curve like in Furst et al. (2016).

The L-curve approach was used to ensure that the regularisation parameters chosen represent an acceptable trade-off between matching observed ice velocities whilst avoiding over-fitting, which is the case for the parameters we used. The values you suggest could also have been chosen, but any parameter combination that lies close to this break in slope of the L-curves is a reasonable choice. A ‘multi-dimensional’ approach could have been used, but we feel that our parameter choices have been justified by the sensitivity testing presented.

Line 142: penalize deviations?

Done

Line 193: Maybe precise why did you see such negative velocity change. Reorganization of the ice flow due to the change of geometry and buttressing?

This is addressed in the discussion section on lines 254-261.

Line 237: add a comma to “[...] (of 1,500 m), the ice shelf [...]”.

Done

Line 238-239: Precise that these metrics are before perturbation.

Done

Line 320: space between “m” and “a./”.

Done

Line 320-324: I think that this is where the instantaneous approach really shows its limit. I don't think that these numbers are really meaningful. When making estimations of future state of the ice sheet, we really want to know what will be the total mass loss, which is not possible with the approach of this paper.

We thank the reviewer for highlighting this and accept their suggestion. We have decided to remove this paragraph from the manuscript.

Clemens Schannwell

General comments:

The manuscript by Mitcham et al. presents a very thorough and detailed numerical study of the instantaneous effect to a number of idealised ice-shelf thinning and calving perturbations to Larsen C Ice Shelf. For the calving perturbation, they find that most of the buttressing is exerted by floating ice within 5 km downstream of the present-day grounding line position. For the ice-shelf thinning experiments, the authors show that a significant thinning (ca. 200 m) is necessary to get a doubling of ice flux across the grounding line. Overall, I find the manuscript very well written and easy to follow. The provided Figures are appropriate and of high quality. The main criticism in the first round of reviews was about the novelty of the study, as a number of previous studies exist that have investigated the instantaneous response of Larsen C Ice Shelf already, albeit with slightly different foci. I should mention that I was not a reviewer in the first round of reviews.

My opinion is that the depth of experiments including additional sensitivity simulations are just enough to warrant publication as a full research article in TC, without having to undertake transient perturbation simulations. However, I think the fact that this is the instantaneous response should be highlighted throughout. To be fair the authors already do acknowledge this in several places throughout the manuscript. I think that drawing any conclusions about the future should be avoided in the manuscript (e.g. L313-317). In the following I outline my list of minor suggestions below and hope the authors find my comments helpful.

We thank Clemens Schannwell for his time in carrying out this thorough review of the manuscript and his suggestions for further improvements before publication.

Specific comments:

• I think the title should already convey the information that this paper is looking at the instantaneous response. Since the paper is also focusing on ice flux across the grounding line, my suggestion would be to also include this in the title. So maybe something along the lines of : ”The instantaneous response of Larsen C Ice Shelf grounding line flux to calving and ice-shelf thinning perturbations.”

Thank you, we agree that the instantaneous nature of these experiments should be highlighted in the title, but think that including the reference to the grounding line flux makes the title too long, and also misses that fact that we examine the change in ice-shelf velocities in response to the A68 calving and ice-rise ungrounding experiments as well.

Combining this with the suggestion of reviewer #2, the title we have chosen is: “The instantaneous impact of calving and thinning on the Larsen C Ice Shelf”.

• Just a comment to line 33-35. It reads like there are no studies on the transient upstream response to changes in Larsen C ice shelf thickness and extent. I would just like to point out that at least for the extreme scenario of complete ice shelf removal, we published a paper in TC in 2018 (Schannwell et al. 2018) about this. We basically find that the sea-level contribution and upstream thinning decay very rapidly (<50 years) after the perturbation.

We have read your paper with interest for the transient experiments that we are conducting – to which it is most relevant – but agree that it should be referenced here. We have included an additional sentence in the introduction that reads: "Finally, Schannwell et al. (2018) and Sun et al. (2020) explored the transient response of the grounded ice to the complete collapse of the LCIS, and the associated removal of all ice-shelf buttressing."

• In section 2.1., can you please mention if you assume isothermal ice and if so what temperature and why?

There is no thermodynamic component in Úa, and therefore the temperature of the ice does not enter the system directly. It will of course impact the rate factor, and we discuss how we choose a prior value for that – based on a uniform ice temperature – in section 2.3. The impact of any variation in ice temperature on the flow is implicitly captured in the rate factor during the model initialisation.

• In section 2.2 L107-108, this reads like Gmsh is creating linear finite elements. As far as I know, Gmsh simply creates the triangulation or mesh, but you can use any finite element type on this mesh. Does this mean that you are using linear Lagrange elements? Would be nice to spell this out more explicitly.

Thank you for highlighting this. We have made this more explicit, and the opening of this paragraph now reads: "The finite element mesh used in the computation was generated with the open source Gmsh software (Geuzaine and Remacle, 2009). We chose to use linear shape functions on these elements."

• L305-307, I find the result that a 1 m or even 0.001 m thick ice shelf vs. no ice shelf gives a difference of ∼100% in ice flux across the grounding line surprising. This warrants a discussion why there is such a large discrepancy.

We have examined this again and find that this discrepancy is due to the different numerical implementation of the two experiment types. We have now expanded this discussion and the opening of this paragraph now reads: "The maximum increase in GLF due to the thinning experiments does not equal that of the calving experiments (502% vs 607%), as in the thinning experiments a 1 m thick layer of ice remains across the ice shelf. This means that at the new calving front the ice thickness is linearly interpolated between the unperturbed nodes in the computational mesh and the neighbouring nodes with an ice thickness of 1 m. In the calving experiments mesh elements downstream of the new calving front are removed from the computational domain. We attribute the discrepancy between the maximum GLF increases to this difference in the numerical implementation of the calving and thinning experiments, and not to any residual buttressing effect of the 1 m ice layer."

• L313-316 I find this scenario to be a bit too far fetched and recommend deleting this paragraph.

Thank you, this was also highlight by the second anonymous reviewer. We agree and have now removed this paragraph from the manuscript.

• Appendix E: I do not know if Ua has pressure-limited sliding laws implemented (Tsai, Budd, or Schoof sliding relation), but I think as a community we are moving towards pressure-limited sliding relations, so it would be interesting to see if the effect between these two relations is already large in the instantaneous response or only in transient simulations.

We feel that further sensitivity testing of these results with multiple sliding laws is beyond the scope of this appendix. But pressure-dependent sliding laws have now been implemented in Úa and could be tested in future work.

• Appendix E: Why did you rerun the inversion for different sliding law exponents? In theory you should be able to use one inversion for all different exponents because they must satisfy C2|u|m2 = C1|u|m1, where m1 = 1 and m2 = 2 for example. You could then rearrange that for C2. Maybe you can comment on that?

We are aware of this approach and in fact often do exactly this to arrive at starting values for our A and C distributions. But this is not exactly a correct solution to the inverse problem (correct in the sense that it provides a minimum point) because the cost functions we use also involve A and C directly in the regularisation terms. The method suggested by the reviewer would work just fine if the only term in our cost function was the misfit term (i.e. the likelihood), but as we have these additional regularisation terms we always find that further inverse iterations are required.

Technical corrections:

L69 outlined and labelled

Done

L86 I think this should be reworded. The stress balance equation is always solved diagnostically. There is no time-dependence in Eq. 1. Only when you couple the stress equation to a transport equation (e.g. ice-thickness evolution equation) does it become transient (time dependent).

Thank you for highlighting this, it has been re-worded for clarity on this point and now reads: "In this work we conduct diagnostic – or time-independent – experiments, in which the equations for stress balance are solved together …"

L430 Whilst

Done

Figures:

Fig. 1: Does the ps in the axes label stand for polar stereographic? I would consider scratching this.

We would prefer to keep the ps reference in all map axes in the manuscript, but will make this explicit in the caption to Fig. 1.

Fig. 5: Is there really only speed-up in panels a-c?

There is almost exclusively speed-up in those three experiments, and the small decrease in speed that was modelled over a few disparate nodes within the computational mesh (almost entirely < 10 m/a) was not visible when plotted. We therefore decided to truncate the colour scale at 0 for clarity. We thank the reviewer for raising this point and will add a sentence in the caption to explain this.

Fig. C1: sizes were half

Done

Fig. E1: (Eq.5).

Done

Sincerely,

Clemens Schannwell

References

Schannwell, C., Cornford, S., Pollard, D., and Barrand, N. E.: Dynamic response of Antarctic Peninsula Ice Sheet to potential collapse of Larsen C and George VI ice shelves, The Cryosphere, 12, 2307–2326, https://doi.org/10.5194/tc-12-2307-2018, 2018