We thank Referee #2 for their review and their suggestions for improving this manuscript. Below, the reviewer comments are in black, with our response following in blue.

General comments

The paper is well written and easy to read, with a sufficient amount of detail (generally). The paper is in line with a series of previous papers treating the buttressing effect of ice shelves: e.g., Fürst et al. (2016), Reese et al. (2018), Gudmundsson et al. (2019). In that sense, I think that the paper proposes a limited novelty in the field.

The key advances over the previous work mentioned are: The replication of the A68 iceberg calving event, the calculation of its impact on the upstream ice (including across the grounding line) and a comparison with observations; and secondly, the calculation made of the total amount of buttressing provided by the LCIS and then the exploration of what proportion of this total is generated in different regions and by different features (e.g. ice rises) within the shelf.

I think that the real case modeling of the A68 calving event and the comparison of the model velocity change with Sentinel-1 SAR observations is particularly well treated and gives a valuable and additional piece of information to the current literature on buttressing and the effect of calving events. Indeed, such processes are often treated only from an observation point of view or only from a model point of view. I really enjoyed the combination of the two here, although a previous and similar work has been conducted by Borstad et al. (2017), which is "acknowledged" by the authors.

Thank you for this comment, we agree that similar experiments were conducted by Borstad et al. and that was the reason we compared our results. In our work we extended the model domain to include the grounded tributary glaciers as well (Borstad et al. modelled the ice shelf only), allowing us to explicitly assess the impact on GLF.

However, I have some concerns about the significance of the instantaneous response of ice shelves and tributary glaciers to downstream ice mass loss. Since such steady modeling does not account for any transient, it seems only a good tool to simulate the effect of small variations for which the transients (ice thickness evolution, advection of the ice front post calving event, "degrounding" of grounding zones due to dynamic thinning, etc.) remain limited. I fear that the results obtained for the largest perturbations applied (thinning or calving of most of the ice shelf) are very theoretical (in the sense that they do not capture the entire physics of an ice-sheet evolution) and of limited value to assess the real effect of an entire (or substantial) collapse of the ice shelf. Also, community work such as the ABUMIP experiment (Sun et al., 2020) already focuses on the transient effects of similar events (ice shelf collapse).

We are certainly happy to emphasise more clearly throughout the text that we are investigating instantaneous responses and buttressing in this work. This was a point that was also raised by Referee #1 and we have provided a justification and explanation for our approach in our response to general comments from both referees.
The ABUMIP paper does explore the transient mass redistribution in response to complete ice-shelf collapse, but does not examine which parts of the ice shelves are generating the buttressing. That is the question that we set out to answer in this work. The papers by Reese and Gudmundsson mentioned above examine the GLF response to perturbations in ice thickness, but do not attempt to calculate the total buttressing provided by the ice shelves and then apportion that total to different regions (and features such as ice rises) within the shelf, which we are able to do with our methodology.

I therefore think that some transient simulations, allowing the ice shelf and its tributary glaciers to evolve after a calving event would greatly improve this study, bringing more insights on the real impact of calving on Larsen C but also other ice shelves. It seems that the authors intend to do such work in the future (as they raise some questions at the end of the discussion) but maybe a part of this work should be included in the present publication.

We completely agree that transient simulations of these perturbation experiments are interesting and important, and we are currently undertaking this work. We have set out our reasons for not including them in this study in our response to general comments from both referees.

Minor comments

Line 45: Similar work has also been conducted by the second author of (Gudmundsson et al., 2019) for the entire Antarctica. I would cite this work in the introduction. It would be interesting to make a few comparisons between this new study and Gudmundsson et al. (2019), to see if numbers agree.

Thank you for this suggestion, we will certainly reference this paper in the introduction and will also compare our most comparable thinning experiment results to those in the Gudmundsson paper.

Line 63: Could you refer to Figure 1 here, so that the reader directly looks at the map and locates the two IR.

Done

Line 86: I'd specify that n=3 is a standard in ice modeling (rather than a choice from the authors).

Done

Line 87: I'd delete the comma between "non-linear" and "Weertman".

Done

Line 92: I am not well aware of the impact of the m exponent in Hill et al. (2018) but I think that Gudmundsson et al. (2019) show a substantial impact when changing m over some regions (up to 40% of GLF in their supplementary Figure S9). I agree that even such change would not affect the main conclusions of the study. However, in Gudmundsson et al. (2019), their perturbation of the ice shelf is not as extreme as the cases where your remove the most of the ice shelf or where you "thin" the ice shelf by more than 50%. I therefore think that applying their conclusion to this paper is a little too much. A little more detail on the effect of the m exponent, with an additional experiment testing this effect, would be useful to the paper.

Thank you for highlighting this, we agree that our justification is lacking, and will include a further figure and discussion of the impact of using different m values in the appendix.

Line 95: "The calving front location represents a pre-July 2017 state". Could you precise how the calving front was defined (?), as it supposedly does not come from Cook and Vaughan which was published in 2010.

We defined the initial calving front by taking the ice extent of the BedMachine product (effectively the zero ice-thickness contour of the data). We will clarify this in the methods section of the manuscript.

Line 103: It is great that the authors tested the impact of resolution. As the impact seems negligible (conclusion of Appendix C), I'd suggest to specify the negligible effect in the main text too (avoiding to jump to the Appendix during a quick read).

Thank you for highlighting this, we will add a statement to that effect in the main text.

Line 111: What is the impact of the density on the model results? Could you also add a map of the integrated density (maybe in a supplementary material)?

The impact of using different depth averaged densities is small, but we will include a further figure and discussion of this in the appendix, as we agree that it would be useful to compare results using a spatially variable density to a those with a constant density of 917 kg/m^3 as is commonly used in modelling studies. We will also include a map of depth-averaged densities in this additional appendix.

Line 113: What is the impact of using one velocity dataset (MEaSUREs) or the other (Sentinel-1)? I believe that the the MEaSUREs InSAR-based Antarctic Ice Velocity v2 data set (Rignot et al., 2011; Mouginot et al., 2012) is a "post-publication" version and that the data should be cited properly too. Instructions are provided at this NSIDC link: https://nsidc.org/data/NSIDC-0484/versions/2. Could you also give a reference for the ENVEO data?

The Sentinel-1 derived ice velocities are slightly higher across the ice shelf when compared to the MEaSUREs data set (the difference is on the order of 10-20 m/a). The reason that the Sentinel-1 velocity data was not used for the model initialisation is that the ice velocity in the A68 region of the shelf is much higher than in the MEaSUREs data set (on the order of 100-200 m/a) during the 2014-2016 period before the final calving event. We did not want to initialise the model velocities in the A68 region of the shelf to these higher values, which are the expression of the iceberg beginning to detach from the rest of the shelf.

Thank you for pointing out the incorrect referencing of the MEaSUREs data, we will correct that. Unfortunately, there is no reference available for the ENVEO data, as the data has not been made publicly available online. The data is available on request from ENVEO, which we have stated in the Data Availability section.

Equation 6, Line 123–127, and Appendix A:

- I understand that you use the same regularization parameter for both the A and C regularization. What is the order of magnitude of A and C (or their gradient), there must be a risk to over or under regularize one of these parameters if they have a different order of magnitude.
- Would the method beneficiate from a proper 3D treatment of the L-curve (e.g., Fürst et al., 2015) where both regularization parameters are varied together? I think that would allow a better optimization of the model (even if your velocity misfit is relatively low). Again, considering different regularization parameters for functions of A and C could be useful.

Thank you for highlighting this, we realise we did not justify our use of the same regularisation parameter values for A and C. We will remedy this by including further figures and discussion in Appendix A to show the impact of varying the regularisation parameters for A and C separately.

• What are these priors? You should state these since the choice of prior can affect the optimization.

Thank you for pointing this out, we should have stated what the prior values are. We will include the values and a description of their calculation in the relevant passage of the methods section. Both A and C priors are chosen to be spatially uniform across the model domain. The A prior corresponds to ice at a temperature of -10 degC, as suggested in Morland and Smith (1983). The C prior is derived from the sliding law, assuming a basal velocity of 100 m/a and a basal shear stress of 80kPa.

Section 2.5: I have one concern about the effect of the spatially varying ice density when instantaneously thinning the ice shelf. I expect the vertically averaged density to vary as the ice thin. Also, thinning by surface ablation/melting will have a different impact on the averaged density than basal melting. I think that since you use a spatially varying vertically integrated density, the density change with thinning should be investigated. If density has no significant effect, maybe mention that despite the model accounts for a varying density, its effect is not important to this study.

Thank you for this suggestion. We do not account for changes in the depth integrated density when thinning the ice shelf in these experiments. The additional figures and discussion on the sensitivity to ice density mentioned in an earlier comment will address this.

Figure 4: please specify that the red line in (c) is the grounding line.

Done

Line 201: Please refer here to Sec. 2.5 that explains the two types of thinning (also use the same wording to define them along the paper).

Done, and thank you, we will ensure that the same terminology is used throughout.

Line 225: You mention that the velocity decrease is likely an artefact in the model due to the timing of the velocity you assimilated for initializing the model. I believe that the negative response could also be partially due to a change of stress state and that acceleration in a region can lead to a deceleration in another one, although such behavior might be only possible when considering a transient model.

Thank you for this suggestion. Yes, we agree that this response is possible, but would be hard to disentangle from the potential artefacts of the model initialisation approach in this region.

Line 280: I would change “dynamic response” for “instantaneous response” as there is no dynamical/transient effect in this study.

Done

Line 286: The (instantaneous) buttressing effect of the ice rises is indeed relatively small but if the ice rises had to disappear, the dynamical effect might be more important than a 2.2% change in grounding line flux. In my opinion, this is where a transient experiment would bring very valuable insights.

We agree that the transient mass redistribution in response to the loss of the ice rises is a very interesting question, and we are currently doing these experiments. But we think that this is still an important result, and that the buttressing provided by the ice rises can be fully captured by these instantaneous experiments. From our initial transient experiments, we find that this 2.2% increase in GLF is a maximal response to the perturbation, and that the GLF decreases towards its initial value through time, due to the thinning of tributaries in response to their initial acceleration. However, our reasons for not including these in the present work are set out in our response to general comments from both referees.

Section 5 (Conclusions): Following my comments on the importance of considering the dynamics of the system and not only the instantons buttressing effect, I think that some of the results, such as the minor contribution of most of the ice shelf (e.g., up to 5 km from the grounding line) should be tempered.

We are happy to clarify the language throughout the manuscript to ensure that the reader knows that these are instantaneous experiments and responses. But we think that ice-shelf buttressing can be fully characterised by these instantaneous experiments. The transient mass redistribution in response to changes in buttressing is a crucial, but separate question to the one we are trying to answer in this manuscript.

References

Fürst, J. J., Durand, G., Gillet-Chaulet, F., Merino, N., Tavard, L., Mouginot, J., Gourmelen, N., and Gagliardini, O.: Assimilation of Antarctic velocity observations provides evidence for uncharted pinning points, The Cryosphere, 9, 1427–1443, https://doi.org/10.5194/tc-9-1427-2015, 2015.
Sun S. et al. (2020). Antarctic ice sheet response to sudden and sustained ice-shelf collapse (ABUMIP). Journal of Glaciology 66(260), 891–904. https://doi.org/ 10.1017/jog.2020.67