Review 3 (reviewer comments in italic)

Summary

In this manuscript, the author employ a new state-of-the-art ice-flow model and assess the utility of various buttressing metrics for inferring grounding line response to distant ice-shelf thinning. For this purpose, they reconcile two former studies that introduce metrics for the local ice-shelf buttressing and the integrated flux response along the grounding line (GL). For local thinning perturbations, the authors show that for relevant parts of an ice shelf (away from the GL and unconfined parts of the ice-shelf), there is a positive correlation between the two metrics. Highest values are found when the buttressing metrics is computed along the first principal stress direction (p1). Yet, buttressing values increase in the vicinity of the thinning perturbations, which seems counter-intuitive with respect to the concurrent increase in the grounding line flux (GLF). This finding makes changes in ice-shelf buttressing utterly difficult to interpret. In a final step, an adjoint-based GLF sensitivity is computed, which shows comparability to results from a large ensemble of forward evaluations. This sensitivity measure has the potential to be very useful in delineating ice-shelf areas relevant for restraining present outlet-glacier discharge.

In order to avoid possible misunderstandings, we point out that we do not conduct any forward model (i.e., prognostic) evaluations here. All experiments are strictly diagnostic in nature. We have stated this clearly in the revised manuscript.

I gladly admit that I was very excited about this study because the authors present a computationally efficient adjoint-based method to compute GLF sensitivities that gives identical results as a cumbersome diagnostic perturbation ensemble. Initially, they also convinced me about the limited utility of changes in the buttressing index. Yet after plunging into the review, I strongly contest this judgment because the underlying analysis seems somewhat biased (see below) and I urge the authors to moderate their assessment. The authors themselves show that the buttressing index along the p1-direction is actually very informative in terms of GLF sensitivity. This is a very important conclusion, which will be appreciated by modellers that cannot compute this adjoint-based sensitivity. Moreover, I have identified a potential error in the index calculation, which might have severe implications.

We were also initially similarly excited by the possibility of a computationally inexpensive and easily calculated metric for use in assessing the impact of local ice shelf thickness changes on changes in grounding line flux (i.e., a way to obtain the information from the Reese et al. calculations but with less effort). Indeed, this was an initial goal of our research, along with providing some physical basis for better understanding and justifying the apparent correlations between local measures of ice shelf buttressing and changes in grounding line flux.

In the end, however, we concluded that we cannot in good faith make a recommendation for using these apparent correlations. First, we've found it difficult to provide a clear explanation for their existence (i.e., the physical mechanisms connecting them). Second, we've found and demonstrated clear contradictions between changes in buttressing on the shelf, in the vicinity of perturbations, and changes in integrated buttressing and grounding line flux, which are contrary to our understanding for how ice shelf buttressing works. Most important, however, is that even for simple or idealized ice shelf geometries, numerous data points near the grounding line -- the region that is most sensitive to perturbations -- must be removed for strong correlations to emerge. For a realistic ice shelf, only a small number of points near the center of the ice shelf remain useful at demonstrating the correlation. Lastly, as we show in a newly added Supplemental Table, there are many other physical quantities that correlate with changes in grounding line flux, some of which may simply be fortuitous or spurious (and, as with the buttressing number, we find that these same correlations become much weaker and less convincing when applied to realistic domains). Thus, while we appreciate the reviewer's comments, we argue that it is not within the goals or scope of the current work to come up with additional reasons to further justify the application of these easy-to-calculate metrics.

The potential error the reviewer alludes to is the swap of panels b and d in Figure 2. This is, however, an isolated mistake with no implications regarding the analysis conducted in the rest of the paper (as noted further below).

In summary, I remain very positive about this manuscript and I recommend that the editor should continue to consider it for publication in The Cryosphere after my concerns have been alleviated. This will require a major revision during which a fundamental change in the manuscript structure might be necessary.

We appreciate the reviewer's careful attention to our manuscript and, as detailed below, address as many of their concerns as we can without changing the fundamental interpretation of our results. We also note that many sections of the paper have been significantly revised relative to the initial submission, including additional analysis, arguments, and changes in presentation.

Erroneous calculation: From a vertically integrated perspective, the normal stress Tnn which is computed in the various directions should be maximal and minimal for the first (p1) and second (p2) principal stress directions, respectively. This implies that the buttressing is minimal in p1 and maximal in p2 direction (you show this nicely in Figure S1 yourself). In Figure 2, you show the buttressing values for the MISMIP+ setup in various directions. While the p2-values appear maximal, the p1 values seem larger than the values computed in flow direction. This cannot be correct. I suspect that you confused panels b) and c). If not, this comment might have severe implications. Please verify.

We appreciate the reviewer's pointing this out. The panel swap mentioned in Fig. 2 was definitely a mistake, which we have now corrected. The related buttressing number calculations, however, were / are correct and unaffected by this. Consequently, the mistake in this figure was isolated and did not / does not propagate to any of the discussion or conclusions in the rest of the paper.

Inconsistent and biased analysis: I certainly appreciate how carefully you have structured the analysis in this manuscript. You clearly state a correlation between the GLF response and the buttressing index in dynamically relevant areas (cf., Sect. 4.2, Fig.4). Thereafter, you show that buttressing changes in the vicinity of the thinning perturbations exhibit a counterintuitive behaviour which is difficult to interpret. Yet, this difficulty seems to have entirely undermined your confidence in the interpretability of this measure. In the abstract, you even condemn the correlation between the GLF and the local buttressing measures as remaining '[...] elusive from a physical perspective'.

This judgment is evoked throughout your manuscript and I somehow feel that I have to take up the cudgels for this metric. First, you show yourself that there can be good correlation (Figs. 4,5,11b). The more I tried to understand the details of your analysis, I have more and more doubts about its robustness. First doubts arose when I read through Sect. 4.3.1. You start by discussing non-local speed-up in the vicinity of the perturbed area (but excluding the centre). Thereafter, you focus on the local-scale buttressing changes within the perturbed area. This seemed inconsistent and this choice biases and discredits the buttressing change measure.

As noted above, we do eventually conclude that the correlation between local buttressing number and grounding line flux should not be applied in a 'predictive' sense (i.e., to diagnose the difficulty to calculate GLF sensitivity via the much simpler to calculate (local) buttressing number. Our analysis in section 4.3, which has been significantly revised and updated (including updated analysis and discussion of perturbations and the resulting changes that occur *at* the location of perturbations) is consistent with these conclusions. Throughout our revised section 4.3, we have attempted to clarify and emphasize the fundamental inconsistencies we find between the impacts of (1) local (at the grid cell) perturbations on various physical quantities, including the buttressing number, versus (2) changes in areas neighboring the immediate perturbation, versus (3) domain-integrated changes in buttressing and ice flux at the grounding line, and to more clearly tie the findings from this section of the paper to the broader discussion and conclusions. In general, we show that, on the ice shelf, changes in buttressing at and immediately neighboring to perturbation locations are generally not consistent with our broader understanding for how buttressing works and also not consistent with the changes observed by the integrated ice shelf / ice sheet system explored here (i.e., local perturbations (reductions) in ice shelf thickness *reduce* overall ice shelf buttressing, which in turn *increases* overall ice flux across the grounding line).

Initially, I was willing to accept this judgment but then I realised that the same counter-intuitive response is seen in the principal strain-rate components (Fig.7e and f). These also indicate compression within the perturbed area (and slightly beyond). Consequently, you also need to dismiss the usefulness of this measure. This is too much of a stretch for me. I simply think that your analysis should consistently avoid areas close to the perturbations. To substantiate my view, I want to briefly explain the 1st principal buttressing or strain-rate changes in Fig7 e and g. After the perturbation, you clearly get less buttressing and increased extension upstream and downstream (in-flow direction) of the affected area. Sideways, but still along the 1st principal direction, these effects result in increased buttressing and compression (similar to a bottleneck effect). This explanation seems reasonable. I therefore strongly urge you to moderate and adjust your assessment of the buttressing metric, accordingly

This is a difficult argument to follow because, by nature, the buttressing number calculations are *local* in nature. It's hard to support their use on the basis of physical arguments if one cannot understand and connect local changes in buttressing to the broader changes in buttressing that control overall flux across the grounding line. Nevertheless, we go through a detailed analysis in our revised section 4.3 (and related Fig. 6) where we attempt to connect the local and neighboring impacts of perturbations on the shelf (including their impacts on buttressing number) to the broader changes in buttressing experienced by the entire ice shelf and their impacts on grounding line flux. While we can provide a fairly detailed narrative for *what* happens when a perturbation is applied to the ice shelf, we still lack a convincing physical understanding for *why* it happens. That is, why should the grounding link flux sensitivity -- an integrated quantity -- be correlated or adequately characterised by a locally calculated buttressing number on the ice shelf? We cannot confidently answer that question here, which gives us great hesitation in blindly applying these correlations. Moreover, our findings that many other easily derived physical quantities (some trivial, e.g. ice thickness) also correlate well with grounding line flux suggest that there may be no direct physical connection between these two quantities that would support their broader use (the correlations could be spurious or fortuitous, as discussed in newly added parts of Section 4.3.4 and discussion and Table S1 in the Supplementary Material). Regardless, we clearly show that these same correlations become weak and unconvincing when applied to realistic ice shelf domains. We would be happy for someone else to carry on with trying to further understand and justify the use of local buttressing numbers as part of ongoing work, but that is not the goal of our paper. The reviewer is suggesting that we come up with a better definition for, calculation of, and understanding of a buttressing number that takes non-local factors into account. This is a laudable goal, but again, is well outside the stated aims and scope of this paper.

A main motivation for why I raise this point is that many ice-flow models are not capable of an adjoint-based evaluation. It would therefore be constructive, if you could give some advice on how to best evaluate the local buttressing metric wrt. the GLF sensitivity. You nicely show that there is a correlation.

We fully appreciate this and, as stated above, it was a primary motivation when we initially undertook this study. Unfortunately, we cannot advocate further for the application of this method for the reasons argued above.

Your strategy to introduce a buffer zone around the grounding line is valuable (it is anyhow clear that these regions are important for the GLF sensitivity). From Fig.4, I think that areas with negative buttressing values should also be excluded (gives more flexibility than prescribed masking). So you could give some advise on how this metric can still be useful.

We have updated and improved the discussion of the necessary "buffer zone" in the revised manuscript. Specifically, we now introduce a more quantitative way of calculating this buffer zone based on the ratio of shear to normal stress (Section 4.1 and Fig. S2). Unfortunately, this does nothing to address the fundamental problems of needing a buffer zone in the first place; 1) this removes many of the most sensitive areas from consideration, and 2) when applied to realistic ice shelves, one is limited to a small area of the ice shelf if strong correlations are of interest.

Moreover, you should emphasise that if the interest is in the GLF sensitivity, the buttressing metric should be computed in p1/flow direction as against Furst et al. (2016). This comment further implies that you might want to reconsider the structure of the document: I suggest that you start with the GLF sensitivity of Reese et al. (2018). Then you could show that the adjoint-based approach gives equivalent results. Afterwards, you might want to assess the utility of the buttressing metrics (advice, limitations, etc.) to explain the GFL sensitivity

Indeed, we do clearly argue that buttressing calculated in the p1 and ice flow directions are better for quantifying the GLF sensitivity than the p2 direction, at least for the case where strong correlations are observed. We have not, however, opted to restructure our manuscript as suggested because our current conclusions and recommendations are better supported by the current organization and narrative.

<u>Minimum and maximum speed increase</u>

It took me a while to get my head around the retrieval of the direction of the minimal and maximal speed increase (L182ff). Although I am very impressed by the distinct peaks in the resultant distribution (Fig.6b), I wonder about its utility in this study. After its presentation, this measure is briefly compared to Gudmundson (2003) and shortly re-raised for the Larsen C *setup. It is not discussed nor mentioned in the conclusions. I therefore urge you to re-consider its utility*

This section and the related figures have been removed from the revised version of the paper.

1. Please reduce the overall amount of footnotes. Sometimes they keep valuable extra information, which should appear in the text.

We have significantly reduced the number of footnotes by including most of the relevant material in the primary text.

2. Please introduce a figure of GLF response Nrp and the buttressing values (p1,p2, flow) for Larsen C. It might help you to delineate the area in which the GLF response and buttressing values are correlated.

This has been added as a third column of panels to a new figure that combines several figures from the original version of the manuscript. This information can now be found in Fig. S6 in the SI.

L29 The term 'longitudinal stresses' seem to be too narrow here. I would rather speak of 'membrane stresses' following Hindmarsh (2006).

Thanks for this suggestion. We have updated the manuscript accordingly.

L42 Delete 'of ice'

Done.

L60 Insert comma after parenthesis.

Corrected.

L115 This sentence is not true. You do not show the response on the southern/botton part of the MISMIP+ setup.

What is meant here is that we do analyze the response to perturbations over the entire model domain but we don't conduct perturbation experiments over the entire domain. This is because the response will be symmetric about the centerline. For example, the response of the change in GLF to a perturbation at (x,y)=(480 km, 50 km) will be the same as to a perturbation at (x,y)=(480 km,30 km), just mirrored about the ice stream / shelf centerline.

L118 As in the original study by Reese et al. (2018), I do not understand the meaning of P. You say it is the local mass change associated with the perturbation. So it should be rather constant *(despite element size variations on Larsen C). Units should be m 3 . The GL flux change R is however in units of m 3 /yr. I do not understand how Nrp can then be dimensionless. I think that I misunderstood the meaning of P. Please explain in more detail.*

We have added more information to this section of the paper to clarify the units on both R and P. In Reese et al. (2018), it is implicit that the time period of interest is one year (according to personal communications). Therefore, P should have units of m^3, which are the same units as R. We have explicitly stated that the units of R and P are both in m^3 (in which case their ratio is non-dimensional).

L169 You must have noticed the dip in the correlation with the p1-buttressing (Fig. 5a). So the best correlation occurs at ±25◦ . With respect to the flow direction, the optimal correlation is ~100◦ turned (counterclockwise, Fig. 5b). Your statement in this line does note entirely hold.

We maintain that this statement is correct: if we move the curve in Fig. 5b to the right by around 50 degrees, the point with the best correlation in Fig. 5b moves to 150 deg, similar to the local maximum in Fig. 5a. The point with the second best correlation in Fig. 5b is shifted to ~210 deg, i.e., 30 deg, corresponding to the second local maximum in Fig 5a.

L173 You envoke the notion of an overall best buttressing metric. I do not think that this exists as such. It will depend on the spatial focus which can be the GL, central areas of the ice shelf or the calving front. Please remove this notion of a best metric.

The notion of a "best" metric is not ours but comes from the previous work of Fürst et al. (2016). We state this clearly in our paper. We're not supporting its use or definition here. To avoid confusion, we have changed "best" to "good" in this sentence

L194-L207 Prior to this section, you focus on the speed-up signal 'among neighbouring cells' (L182-L194). In this section, you then discuss buttressing changes within the perturbed areas. This seems inconsistent. From Fig. 7g and h, I think you can extract a meaningful, aggregated index for buttressing changes excluding the perturbation centre. Upstream of the perturbation (in flow or p1 direction), the buttressing decreases with highest decrease close to the perturbed area. This inconsistent treatment therefore seems deliberate and strongly biases your interpretation. This bias leads to harsh judgments of the buttressing metric in the subsequent two sections, which are, in my opinion, note well justified. Please stay more objective. You also show the strain rates fields in the principal direction which also show overall compression within the perturbed zones. You do no discredit the usefulness of these values either.

We have updated the analysis and discussion in this entire section, including a focus on the impacts of perturbations exactly at the grid cells where perturbations are applied. As noted above, we agree that one can conduct a careful analysis of a single perturbation in order to understand how, overall, that perturbation leads to the broader changes in buttressing that are expressed as changes in GLF. However, we still lack a detailed understanding for how this cause-and-effect is physically connected to the concept of a locally calculated buttressing number. We also show (in a new section in the SI) that similarly strong correlations exist between GLF and other physical quantities, some of which are unrelated to buttressing or buttressing number. This, and more importantly, the lack of strong correlations when exploring realistic ice shelf domains leads us to abandon further investigation of the utility of this method as a proxy for understanding GLF sensitivity.

L225-L238 This paragraph judges the results and it is therefore better located in the discussion conclusion. I also sense some redundancy.

This section (4.3.3), which has been significantly revised, is a necessary summary of our findings from the detailed analysis conducted in the sections immediately above it. Further, it is a necessary transition from discussion of the idealized MISMIP+ test case domain to the more realistic Larsen C domain.

L273-L289 This paragraph presents methodology so it should appear earlier (not as a sub-section of the Results).

While this change would make our paper more closely follow the strict formatting of a standard research paper (e.g., introduction, methods, results, conclusions) we think that the overall readability would suffer as a result. Further, a number of other reviewer comments indicate that the paper and interpretation would be easier to follow if this strict partitioning is avoided. Therefore, we opt to keep the formatting of these sections as they currently are.

Fig.1 Poor figure quality. Missing overview figure for localisation of Larsen C. What did you do about Bawden Ice Rise? Could you also show the observed velocity magnitude on Larsen C. Please indicate in the figure that the velocities you show, present the state after the relaxation (you only mention this in the text L105).

The location of Larsen C has been added to the figure. We have also added Figure S1 (to the Supplementary Material) showing the comparison between modeled and observed ice surface speeds on Larsen C. With respect to Bawden Ice Rise, we have looked into this in some detail and it appears that it is a small feature that does not show up in our domain due to our initial data interpolation onto a mesh with a minimum resolution of approximately the same size as this feature. We thank the reviewer for pointing this out, as it is something we will pay closer attention to including in future meshes.

Fig.2 In the caption you speak about 'perturbation points'. The perturbation does not affect a single point but an entire patch.

We now refer to these as "perturbed grid cells" instead of "perturbation points".

I would use different colours for the response number and buttressing metrics.

While we tried out multiple colorbars for the different panels in Fig. 2 (panel a vs. panels b, c, and d), we ultimately decided to keep them the same. This is primarily because it is then much easier to compare the spatial pattern of the GLF response number with the spatial patterns of the buttressing numbers in different directions (i.e., making a qualitative comparison by "eyeball").

Why do you get negative response numbers for perturbations next to the grounding line?

We observe negative adjoint sensitivities only for cells intersecting the grounding line. For those cells, changes in thickness directly affect the grounding line position/length and the thickness over the GL, which could lead to negative responses. We have added a note to the Fig. 2 caption on the topic of negative response number. This topic is also discussed in the 4th paragraph of Section 4.5 (starting around line 331).

Figs.11&12 I would try to merge these figures. Panels (a) can be placed as an inset into panels (b).

As suggested, we have merged Figs. 11 and 12. They have also been moved into the SM (currently as Fig. S6).