Summary and comments on the manuscript entitled

Diagnosing the sensitivity of grounding line flux to changes in sub-ice shelf melting

presented on 11.02.2020
by

T. Zhang et al.

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.

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.

In summary, I remain very positive about this manuscript and I recommend that the editor should continue to considered 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.

General comments

Erroneous calculation
From a vertically integrated perspective, the normal stress T_{nn} 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.

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 counter-intuitive 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 discusseing 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. 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.7*e* 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 reason-

able. I therefore strongly urge you to moderate and adjust your assessment of the buttressing metric, accordingly.

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. 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. 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 Fürst 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 asses the utility of the buttressing metrics (advice, limitations, etc.) to explain the GFL sensitivity.

Minimum and maximum speed increase
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.

Specific comments

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

2. Please introduce a figure of GLF response N_{rp} 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.

Detailed comments

L29 The term 'longitudinal stresses' seem to be too narrow here. I would rather speak of 'membrane stresses' following Hindmarsh (2006).
L42 Delete 'of ice'

L60 Insert comma after parenthesis.
L115 This sentence is not true. You do not show the response on the southern/botton part of the MISMIP+ setup.
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 N_{rp} can then be dimensionless. I think that I misunderstood the meaning of *P*. Please explain in more detail.
L169 You must have noticed the dip in the correlation with the p1-buttressing (Fig. 5a). So the best correlation occurs at $\pm 25°$. With respect to the flow direction, the optimal correlation is $\sim 100°$ turned (counterclockwise, Fig. 5b). Your statement in this line does note entirely hold.
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.
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. 7*g* 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.
L225-L238 This paragraph judges the results and it is therefore better located in the discussion conclusion. I also sense some redundancy.
L273-L289 This paragraph presents methodology so it should appear earlier (not as a sub-section of the Results).

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).
Fig.2 In the caption you speak about 'perturbation points'. The perturbation does not affect a single point but an entire patch. I would use different colours for the response number and buttressing metrics. Why do you get negative response numbers for perturbations next to the grounding line?

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

References

Fürst, J., Durand, G., Gillet-Chaulet, F., Tavard, L., Rankl, M., Braun, M., and Gagliardini, O.: The safety band of Antarctic ice shelves, Nature Climate Change, 6, 479–482, doi:10.1038/nclimate2912, 2016.

Hindmarsh, R.: The role of membrane-like stresses in determining the stability and sensitivity of the Antarctic ice sheets: back pressure and grounding line motion, Philosophical Transactions of the Royal Society A, 364, 1733–1767, doi:10.1098/rsta.2006.1797, 2006.

Reese, R., Gudmundsson, G., Levermann, A., and Winkelmann, R.: The far reach of ice-shelf thinning in Antarctica, The Cryosphere, 8, 53–57, doi: 10.1038/s41558-017-0020-x, 2018.