Response to review 1

The manuscript has been improved greatly and is not in a much better shape.

Personally, I still find that the search for a correlation between locally derived buttressing numbers within an ice shelf and integrated grounding-line flux is a bit beside the point. There will be some correlation because for confined ice-shelves local changes in ice thickness impact both grounding-line flux and the local measure of buttressing. But maybe this is just a question of how to think about these processes. I like to think about the thickness affecting the stresses. The fact that we then many have a correlation between stresses at two different locations, or between stresses where the thickness perturbation was applied and GL flux, is somehow less fundamental to me. But everyone should be entitled to have their own favourite way of thinking about these things. The bottom line is that using locally defined buttressing numbers within an ice shelf as a diagnostic for impacts on GLF is not a particularly sound idea and, as the authors find, any such relationship is tenuous at the best.

We thank the reviewer for his highly constructive comments. To make our points more clear, we change the sentence "This and the fact the correlation is generally much poorer when applied to realistic ice shelf domains motivates us to seek an alternative approach " in the abstract to "This and the fact the correlation is generally much poorer when applied to realistic ice shelf domains motivates us to seek an alternative approach for predicting changes in grounding line flux ".

The way the paper is written at the moment I feel there is a potential for some confusion. As far as I can see there are two main points in the paper. One relates to the usefulness of local buttressing numbers within an ice shelf as a diagnostic for impact on GL flux (bad idea), the other one is a purely methodological point: i.e. how to compute the impact of thickness perturbations on GL flux in a computationally efficient way. This second main point of the paper (use the adjoint method to speed up things) has nothing to do with the first point. But as the paper is written at the moment, it might appear to a reader that is not too careful, that the adjoint-based sensitivity calculation somehow links to the first point, or that it even offers a solution to the (lack) of correlation between locally calculated buttressing and GL flux.

To make this point more clear, we now change the following sentence in Section 4.5, "This motivates our investigation of a wholly different approach, which provides a GLF sensitivity map analogous to that from Reese et al. (2018).", to "This motivates our investigation of a wholly different approach, which provides a GLF sensitivity map analogous to that from Reese et al. (2018), instead of seeking for a simple buttressing number indicator to predict the GLF sensitivity. "

I don't see how the Rees approach can be considered 'ad hoc'. It's just computationally inefficient. In fact, one could argue that the Rees approach as the advantage that it does not have the inbuilt linearization of the adjoint method. The adjoint approach is much better at estimating the linear response than the finite-differences methods. It is both more accurate, and faster. But the (first-order) adjoin approach use by the authors cannot be used to estimate the range (amplitude) of the linear response as done by Rees. Also, in this particular example the adjoint method will not really make that much of a difference unless one only want to know the integrated impact on GL flux. As soon as one wants to calculate the perturbation at each and every point along the grounding line (as done by Rees) I suspect the computational efforts using the finite-differences and the adjoint methods will become similar.

We now remove "ad hoc" in the sentence "Thus, despite the added complexity in its computation, the adjoint-based method provides significant advantages over the simpler but more ad hoc(i.e., perturbation-based) analysis methods

discussed above." The new sentence is "Thus, despite the added complexity in its computation, the adjoint-based method provides significant advantages over the simpler perturbation-based analysis methods discussed above."

Not sure about this as I'm not that familiar with the notion, but is (6) not generally true whenever atm. pressure is neglected?

Equation (6) is true when we neglect atmosphere pressure (stress-free condition) on the surface. See Equation (14) in Perego et al. (2012).

I feel that the discussion about ice-shelf buttressing should reference the papers by:
Pegler, S. S.: Suppression of marine ice sheet instability, J. Fluid Mech., 857, 648–680, doi:10.1017/jfm.2018.742, 2018.
Pegler, S. S.: Marine ice sheet dynamics: the impacts of ice-shelf buttressing, J. Fluid Mech., 857, 605–647, doi:10.1017/jfm.2018.741, 2018.
Haseloff, M. and Sergienko, O. V.: The effect of buttressing on grounding line dynamics, J. Glaciol., 64(245), 417–431, doi:10.1017/jog.2018.30, 2018.
This could, for example, be done early in the paper (for example at the end of line 25) but there are number of other places were this could be added. These three papers are quite important addition to the literature.

These three new references are now added.

Citation to Cornford 2020 should be undated to refer to the final published version.

Updated.

References:

Perego, M., Gunzburger, M., & Burkardt, J. (2012). Parallel finite-element implementation for higher-order ice-sheet models. Journal of Glaciology, 58(207), 76-88. doi:10.3189/2012JoG11J063

Response to review 2

General

I want to congratulate the authors to this very well written and well structured manuscript, which is rich in content and of high scientific quality. The conclusions drawn in this article will rectify the view of glaciologists on locally defined measures of the ice-dynamic state of ice-shelves with regard to grounding line flux (GLF) response. Their recommendations in terms of assessing the GLF sensitivity are very useful for improving future assessments. The authors succeeded in resolving my main initial concerns and they moderated their assessment on the local measures. As it stands, I recommend this manuscript for publication in The Cryosphere after some few technical corrections have been addressed.

We thank the reviewer for the support of publishing this manuscript on TC.

Techincal comments

L128 By stating that R is quantified as the 'change in the GLF over a year due to a perturbation in the thickness' , I was initially confused and thought that you conduct transient simulations for a year. Yet you clearly state above that you quantify the instant response. Anyhow, I would reformulate this part to avoid confusion. You only need to mention this time period to obtain a non-dimensional number.

We change "where R is the change in the GLF over a year due to a perturbation in the thickness at a single grid cell" to "where R is the volume change by GLF over a year due to a perturbation in the thickness at a single grid cell".

L281 You missed to specify the components which enter the correlation mentioned here.

We now change this sentence to "Further, we show in the Supplementary Material and Table S01 that the correlation between Nb and Nrp may be spurious"

L368 Doubling of 'only' .

The first "only" is removed.

Fig. 3 I do not see the necessity to invoke the linear regression analysis in the caption here. Initially it confused my interpretation of the figure.

We change "linear regression" to "relation".

Fig. 3 In the caption, you distinguish between 'modeled' and 'predicted' values for Nrp but I am not sure how you distinguish them in the panels. I suspect the two shades of blue indicate these two categories. Please amend.

We rephrase it as "Modeled Nrp versus buttressing number Nb calculated along..."

Fig. 4 Same comments as for Fig. 3.

Corrected.

Fig. S2 nrp ⟶ Nrp

Corrected.