Responses to community comment #1 for the article "The impact of landfast sea ice buttressing in the Larsen-B Embayment, Antarctica"

We would like to thank very much N. Ochwat and T. Scambos for taking the time to read this article, and for initiating a constructive and open discussion about this subject of shared interest. We agree entirely with the comments they have raised, and have implemented/will implement revisions that address some of them. Where this is not so, it is in general where the work would deviate too greatly from the main focus of the article - often by going into further detail about what precisely caused the changes in calving behaviour and dynamics of the glaciers, while the intention of the article is to study singularly whether landfast sea ice buttressing (defined in analogy to ice shelf buttressing) could have been responsible for these changes. We hope that the revised manuscript clarifies certain points of confusion collectively highlighted by the reviews, and that the discussion more completely describes the conclusions and limits of the study.

A number of interesting points are raised in the comments covering a range of features of our article. We respond individually to the points raised below. Comments are shown in violet and our responses in black. The first three comments relate to specific choices regarding the geometric setup of the model so we have aggregated them.

- Regarding the methods section and the data used in the modelling, we agree with the reviewer 1 that more information is needed about the methods and data used. In particular, selected bedrock data as well as mapped location of the grounding zone may play an important role in the model's results. There are two primary options for bedrock data, Huss and Farinotti (2014) and BedMachine (Morlighem et al., 2023). Bedmachine data has recently been updated to incorporate sonar derived bedrock information for the terminus area of Crane, collected from the RV N.B. Palmer in 2006. Additionally, the Huss and Farinotti (2014) data do not cover the whole study area displayed in Fig. 1. What did the authors do in these regions? Regarding the grounding line, it is unclear how this was determined and why it was chosen for the model; it would also impact how the glacier acceleration is discussed (i.e., floating or grounded ice). We note that the paper cited in support of the grounding line is not available at this time for review. It would be interesting to discuss the effect of using other, published, grounding lines (e.g. Rott et al., 2018, Sun et al. 2023, Ochwat et al. 2023).

  Furthermore, the model output of the "glacier terminus" (cyan lines figure 1) is likely incorrect as there is 300+ m thick ice several km further downstream from the glacier terminus identified in the model/figure 1. How are the authors defining the glacier terminus?

  What are the potential problems incurred by using a 2015 DEM with a 2021 Grounding line?

  Some similar queries were raised by the set of solicited reviewer comments. We have implemented revisions that aim to clarify the methods in general. Firstly, to address the calving fronts in the model, we realise we could have done a better job in making the ice shelves look more like they did in reality in 2021. For the revised manuscript, we created a new model geometry with larger ice shelves and performed the initialisation and each modelling experiment again. This has not changed the results of the article, but they are perhaps more convincing now the glaciers look more accurately rendered.

The bedrock topography of the Antarctic Peninsula is poorly mapped and all of the available bedrock topography products have flaws. For example, in BedMachine v3, there are many fast-flowing (> 500 ma^{-1}) areas where the ice thickness is < 20 m, which is clearly unrealistic. The Huss and Farrinotti (H&F) (2014) dataset seemed to have fewer of these areas of implausibly thin ice. We therefore used the H&F (2014) dataset, along with the REMA 200 m DEM, to construct a continuous grid of ice surface elevation and ice thickness amenable for modelling at 125 m resolution. Some minor modifications to the H&F grid were necessary to produce continuous thicknesses and surface elevations including smoothing at the grounding line, and the removal of steps in bedrock elevation at the edge of the H&F domain.

As with the bed topography, the grounding line of the Antarctic Peninsula is poorly mapped and most grounding line products are either discontinuous or valid for times prior to the late-2000s. We use a new grounding line product, now detailed in a pre-print submitted by Wallis et al., which addresses the problems presented by discontinuous or old grounding line products.

In general, though it is possible that differences in geometry products would result in different conclusions, this does not seem likely among those that could plausibly reflect the real glacier geometry. This is because the arguments we make in the paper are simple and mechanical. We have shown the results to be insensitive to different calving front geometries having now carried out the experiments for larger ice shelves. Though it would be nice to carry out a greater number of experiments showing more completely the insensitivity of the results to changes in geometry, it would take a fairly large amount of effort that we do not feel is warranted.

- In Figure 5, please include more years as this will help with understanding the timing of the events. We also request discussion on the fact that melt was lower in 2022 than previous years, according to AMSR data and that the largest peak in T2M was in 2021.

  The section of the article discussing additional environmental forcing at the time of the sea ice evacuation (promoted out of the discussion in the revised version) is not supposed to be a focus of the article. Instead, we hoped to include it as something of additional interest noting the complexities of the environmental factors that could have been in play at the beginning of 2022. Though it would be nice to extend this section, it is not necessary for the conclusions of the article so we do not intend to add additional dates to extend the timeseries. However we will include reference to the AMSR melt estimates as suggested.

- The connection between basal melting and the near instantaneous break-up of the ice tongues after fast ice removal is unclear. Can the authors discuss how those two processes are connected? Also, do the authors have observation or model (reanalysis) indications of a large swell event leading to the breakup of the ice tongues? The relationship between the basal melting, swell, and ice tongue response is generally unclear.

It is true that this section regarding basal melting was a bit unclear. We did not present reanalysis or observations of swell around the time of the sea ice breakup. This information is already presented in your own pre-print on the event, which we will cite at this point in the manuscript, and we did not feel that repeating the analysis would strengthen our conclusions regarding the impact of landfast ice buttressing. We did choose to present the basal melt data because these are novel observations that open avenues towards alternative interpretations regarding the cause of the ice tongue breakup and glacier speed-up, which we feel enriches the discussion in the manuscript. Indeed, given the large magnitude of the basal melt anomaly, we feel it would be remiss to exclude those observations from the discussion. Again, these suggestions are not intended to be particularly conclusive, but we hope the revisions made to the article make the parameters of the discussion more well-defined.

- We also would like to see the results discussed in the context of Robel, 2017.

  Robel, A. Thinning sea ice weakens buttressing force of iceberg mélange and promotes calving. Nat Commun 8, 14596 (2017). https://doi.org/10.1038/ncomms14596

  This has been taken into consideration in the revised manuscript, which includes additional discussion of the potential coupling that iceberg mélange can provide between landfast sea ice and ice shelves.

  Specifically, the revised manuscript goes into a bit more detail about the mélange in the proglacial embayments in front of HGE and Crane Glaciers (that is not easily distinguished from the ice shelves themselves). It is quite possible that these transition regions, where the ice shelf becomes mélange that rarefies until it is entirely sea ice, (not dissimilar from those considered in Robel 2017) enable dynamic coupling between the landfast sea ice and the glaciers as they might be both able to buttress the ice shelves, and be sensitive to the presence of landfast sea ice. We do not model these regions of mélange as the study is interested in the buttressing of the landfast sea ice, but it is discussed as a point of future work. Specifically, we acknowledge a potential second-order impact of sea ice buttressing that we could not investigate where loss of buttressing causes a disaggregation of mélange which destabilises the ice shelves. However it should also not be assumed that mélange broke up because of loss of buttressing. For example, the mélange is likely to be susceptible to the same forcing that caused the sea ice disintegration.

- The paragraph in Lines 326-336 is not clear.

  Yes, this is just supposed to suggest that that the buttressing effect of landfast sea ice and its other influences might vary differently as a function of landfast sea ice extent. E.g. the buttressing effect of fast ice confined to the proglacial embayments is likely to be as large as if fast ice covered the whole Larsen-B Embayment, but its ability to reduce swell might be reduced. This could be a way of differentiating the influence of these effects. We will try and make this clearer in the revised manuscript.

- Lastly, the connection between the ice tongues' disaggregation and the loss of the fast ice needs to be better justified if the fast ice was not buttressing the tongues. The authors are implying an alternative mechanism for the instantaneous response if the fast ice was not offering any sort of buttressing effect, but the path to disaggregation is not clear in that case.

  The mechanism alluded to above involving iceberg mélange is a potential candidate cause of the ice shelf disaggregation, though even in its revised form the article does not take a firm position on precisely what caused the calving immediately after the sea ice evacuation. We do not consider this to be a particularly big issue as there is no reason to believe that loss of sea-ice buttressing is the only mechanism by which ice shelf disaggregation should occur with the loss of the landfast sea ice. However, we have taken the lack of clarity of this point into consideration in the revised manuscript.