We sincerely thank Reviewer 1 for their insightful feedback. We agree that both major points—the reference value and the physical origin of shear zone weakening—are valid. In response, we have: (1) conducted additional model simulations demonstrating that the heterogeneous signal can also be reproduced by lowering the homogeneous Young's modulus from 1.6 GPa to 1.0 GPa; (2) substantially revised the discussion section to better contextualize these findings; and (3) rephrased sections of the manuscript that previously implied resolution of anisotropy from satellites. We appreciate the constructive comments and provide detailed responses below.

RV1-1:

This paper presents a model/data comparison looking at ice-shelf flexure at Priestley Glacier in Antarctica. The authors use a series of differential interferograms to estimate tidal displacement and examine the spatial pattern of these displacements. The time series is validated with GPS from 2018. They compare the observed displacements with three models of elastic flexure, which vary in terms of their assumptions about the geometry of the problem (ice thickness) and about the Young's modulus. From this comparison, they argue that the shear margins are extremely weak (20% of the expected strength). They conclude that DInSAR can be used to understand shear-margin strength.

This summary accurately captures the main findings of our study, and we appreciate the reviewer's thorough engagement with our work. You correctly point out some aspects in our methods that were not as clearly conveyed as intended. In the revised manuscript, we restructured key sections, particularly regarding the derivation of synthesized tidal displacements from DInSAR and how they inform our conclusions about shear-margin weakening. We hope this will improve the reproducibility of our approach and address any remaining uncertainties.

RV1-2:

The paper is likely to be of interest to the readership of *The Cryosphere*. It is reasonably well written, though I need some clarification at a few points, and the figures are clear and support the narrative (though some small changes are needed for legibility). However, I am skeptical of the conclusions, as described in "major issues." Specifically, I think the central claims in the abstract are not supported by the results, and it is unclear to me how the results will hold up to more careful checks on their robustness. I also have some reservations about the claims about the physical origins of this signal, assuming it is robust —I do not think fabric is the likely cause. If the authors can demonstrate that the signal of shear-margin weakening is not a byproduct of their assumptions but rather a robust feature of the data, and the fabric-related conclusions were either better supported or removed, I would be supportive of seeing this work in *The Cryosphere*.

We understand that the major issue is linked to our validation with two GPS stations located along the central flowline. Here, you correctly point out that the local heterogeneous model does not improve the fit. In fact, at these specific locations, all model scenarios provide similar predictions showing that these areas are comparatively insensitive to our modeling choices. This was not clearly communicated and hence correctly triggered your concern. However, as we move away from the GPS point locations and into the entire flexure zone (Fig. 11, Fig. 9f-h) the differences are clearer and also the better fit of the local heterogeneous model becomes apparent. We have changed relevant sections in the revisions to make this more clear.

We also agree that out study does not provide conclusive evidence what mechanism may cause the shear-zone weakening. While other studies have provided evidence for the role of preferred ice-fabric patterns in shear zones, our data do not substantiate these claims and other mechanisms are also possible. We have weakened our interpretations in this regards both in the abstract and the discussion. Also as a response to RV2-4, we now include more sensitivity experiments improving the robustness of our conclusions.

Major issues

RV1-3:

Unless I have missed something essential, the central claims of the paper as presented in the abstract are not supported by the results. The abstract claims “we find that a five-fold reduction of the Young’s modulus in the shear zone, i.e. an effective shear-zone weakening, reduces the root-mean-square-error of predicted and observed vertical displacement by 84 %, from 0.182 m to 0.03 m.” However, it seems this number is derived by comparing the unmodulated tidal height to the observed displacement, which fails to account for any elasticity. From line 386, it is clear that the 0.182 is the unscaled tidal forcing. In fact, the local heterogeneous model *underperforms* the local homogeneous one (0.029 vs 0.027 m misfit), so it would be more appropriate in the abstract to say that shear-zone weakening is unable to improve the fit! The central claim of the paper instead needs to rest on the comparison between the local homogeneous (or perhaps control) model and the heterogeneous model—this is what tells us the effect of the shear-zone weakening. All that can really be said based on the misfit is that an elastic model is useful—nothing about the three different models is conclusive with regards to misfit, as acknowledged by the authors at line 333. The matching claim in the conclusions (“we demonstrated that reducing ice stiffness in lateral shear zones significantly improves the accuracy of vertical displacement predictions, particularly along the grounding zone,” line 477) is also unsupported by the results.

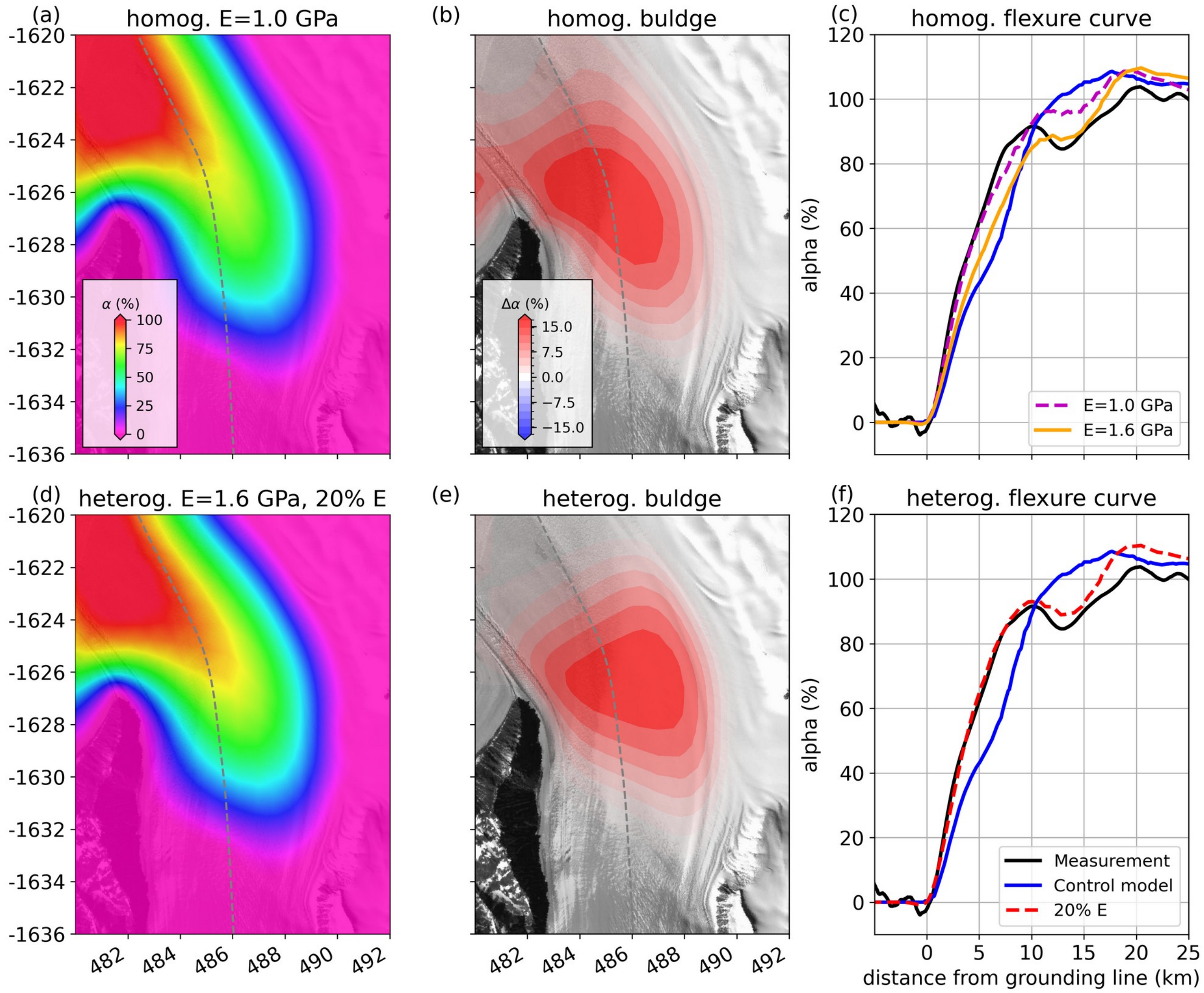
The reviewer is correct that the analysis based solely on the GPS point measurements does not provide significant evidence for either model scenario (related RV1-2). Our conclusion that a reduction in Young’s modulus, representing effective shear-zone weakening, improves the fit between model and observations compared to a homogeneous case is based on an area-wide comparison, as shown in revised Fig. 11.

To clarify this in the revised manuscript, we explicitly delineated this area in revised Figs. 9f/g/h and rephrased the corresponding parts of the abstract and main text. The GPS site primarily serves to demonstrate that scaling tide model output with a measured (or modeled) alpha value is generally useful and that a 1.3-hour viscoelastic delay appears to be present at this site. However, this delay reduces the RMSE between the tide model and GPS data by only 2 mm (Fig. 12b), making it negligible within the context of this study and further supporting the elastic approximation. See new section 5.4 in this context.

RV1-4:

The problem with the misrepresentation above is that it forces the authors to wade into a more complicated comparison in terms of alpha. More physical explanation of alpha is warranted, as discussed in the general comments below, but in terms of the effect on results doing the comparison in terms of alpha obscures the effects of uncertainty and error. We need a careful analysis of these uncertainties and errors to understand if the comparison in terms of alpha is in fact meaningful—as is, I find the error analysis in the paper insufficient. I do not think a single, fixed value of E to treat as a reference that easily justified (for example, the 2019 paper by the same authors has 1.0±0.56 in the abstract). I do not see how the authors can exclude the possibility that there is substantial variation in E because of things like temperature, and that the mean value is incorrect, which in combination may explain much of the misfit. Similarly, the conclusions of the paper rest on the better fit of the model with weakened Young's modulus in the shear margins, but there is not systematic evaluation of how changes in Young's modulus affect the misfit. Maybe reducing the value elsewhere would produce a better fit—we simply do not know. Without a more systematic comparison, and without a clear evaluation of how uncertainty in the parameters assumed constant affect the results, I am not confident that we can in fact conclude that the authors robustly detect a signal in the shear margins.

We agree that E can vary for a number of reasons (temperature being one of them) that were not prominently mentioned enough in the first draft. To clarify, here we use E = 1.6 GPa as background values as derived in *Wild et al., 2017* from tiltmeter data from the McMurdo Ice Shelf and further applied in *Wild et al., 2018*. The E=1.0 GPa emerged from the Darwin Glacier (Wild et al., 2019), which supports your statement that values of E in situ are far from constant/known (also pointed out by RV2-5). Moreover, the stiffness scales with the ice thickness cubed (Eq. 4) illustrating that the model geometry is important. In order to account for this comment (and also in line with RV2-4) we conducted additional simulations with the synthetic model setup (Fig. A2) and the local homogeneous model and now investigate reducing the homogeneous Young's modulus (Fig. 12 and new section 5.1).



These additional model experiments show that reducing the homogeneous Young's modulus from E=1.6 GPa to E=1.0 GPa does indeed produce a similar result as our best fit heterogeneous experiment. As a consequence, we re-designed our discussion section.

RV1-5:

I also do not buy the argument that fabric is likely to explain these observations. The authors conflate the viscous anisotropy of ice, which is very strong (an order of magnitude weakening or hardening) and anisotropy of the elastic properties, which are much weaker. There has been extensive work on this topic in the seismic literature, so we have a reasonable number of measurements of the effect of fabric on seismic wave speed, which have found values in the range of 5% and below (e.g., Lutz et al., 2022 https://doi.org/10.5194/tc-16-3313-2022, Rathmann et al., 2022, https://doi.org/10.1098/rspa.2022.0574, and many references therein). Since seismic waves are elastic, I would expect the effect of fabric on ice shelf flexure to be similar to its effect on seismic waves, i.e., about 5% rather than the 80% needed to explain the results here. As a starting point, I suggest looking at Rathmann et al., 2022, since they formulate the effects on elastic anisotropy in terms of the anisotropy in the Lamé parameters, which could relatively easily be converted to anisotropy in the Young's modulus and Poisson ratio, and it appears that the value would be on the order of a few percent. Thus, I think section 5.2 should be reworked to acknowledge the limited effect of fabric on elasticity, and to propose alternatives. Most obvious to me are things like thickness errors, damage, and errors in the value of *E* used as a baseline. Alternatively, if the authors think this is really a viscous effect, then the validity of the purely elastic model is called into question. The conclusions should be changed to reflect this viscous/elastic difference. I am not convinced that there are grand implications for ice-stream initiation and would certainly need to see more discussion in section 5 if this were to remain in the conclusions.

As already pointed out in responses to RV1-2 and RV1-5 (and RV2-5) we believe that you capture a valid point here and we are thankful for your suggestions. We acknowledge that ice-anisotropy may be a candidate for shear-margin weakening, but how this would imprint on our model assumptions in terms of *E* is a different story. We have significantly reworked section 5.2 taking your suggestions into account and removed the statements about ice-stream initiation from the conclusions.

General comments

RV1-6:

The least-squares adjustment needs more explanation. It is a bit strange to do least squares with an underdetermined system—I guess this amounts to trying to adjust the tide model as little as possible? What this assumption implies deserves explanation. However, I am confused as to how the misfit is not reduced to zero when the system is underdetermined—is this system of equations not linearly independent? A sentence explaining why there is any residual misfit would help clarify.

Yes, our approach aims to adjust the tide-model output as little as possible to match our DInSAR observations. This assumption implies: (a) that DInSAR observations provide the absolute reference for tidal displacement on the freely floating part of the ice shelf, and (b) that tide-model uncertainties are more significant in the amplitude of tidal constituents rather than their phase.

We further quantify the role of phase uncertainty in Fig. 12, where we apply a net phase shift of 0.37 h to the tide-model output to maximize the match with our GPS data. However, this phase adjustment only improves the RMSE by a few centimeters, suggesting that amplitude discrepancies remain the dominant source of misfit.

The reviewer is correct that the residual misfit is essentially zero (within the computer precision). Reworded.

RV1-7:

I am a bit unclear how the load tide is handled. Is the bed underneath the grounded portion of the glacier truly assumed fixed, so that w=0 there? Or is the load tide assumed to apply only where there is ocean water, neglecting the elastic effect on land upstream? This choice should be clarified and justified in the text.

Yes, in our model, the load tide is only applied on the floating portion of the domain and is not transmitted through the bed to the grounded ice. This choice is based on two key considerations: (a) Magnitude of the load tide: As shown in Fig. 4a/b, the load tide is an order of magnitude smaller than both ocean tides and the inverse barometer effect (IBE). (b) Minimal tidal forcing at the grounding line: The elastic response of the bed to ocean tidal loading is negligible compared to the displacement observed in freely floating areas. This justifies our assumption that the bed underneath the grounded ice is effectively fixed, meaning w = 0 there.

RV1-8:

The mixture of alpha and w is confusing to me. Line 204 says that alpha is “the mean vertical displacement that can be expected during SAR data acquisition”, but based on units it is the fraction of maximum displacement expected. A clear, physically motivated definition of alpha, with units, would help if placed in 3.1.4. Also, we need a bit more physical explanation about how alpha is determined—in particular, I am not clear on what assumptions about spatial variations are employed here.

We acknowledge that this sentence was not correctly phrased. An alpha value of 10% means that a 1 m tidal forcing results in a 10 cm vertical displacement at that location. The "mean vertical displacement" phrasing refers to the fact that alpha is derived by averaging all available interferograms, capturing the expected response over multiple tidal cycles. We now clarify this in Section 3.1.4 and provide a more precise, physically motivated definition of alpha. Alpha is percentage tidal displacement and therefore has no unit.

RV1-9:

Some reorganization of methods and results is needed. Section 4.2 is a confusing mix of methods and results. I am not clear on what these mosaics in Figure 7 are. I am assuming they are DInSAR images aggregated in some way, but it is not clear how. It seems to me that this relates to section 3.1.4, but I am not completely clear. Lines 321 to 326 are methods and so belong under the top-level header of 3.3.3 (this would have helped me understand the motivation of multiple models better there, too).

We appreciate this suggestion and have restructured the revised manuscript accordingly (also in line with RV2-3). The mosaics refer to synthesized DInSAR images, which are based on an alpha-map derived from the original DInSAR images and least-squares adjustment. Figure 7 illustrates the same procedure as Figure 5 but applied across the entire grounding zone rather than just a single point on the freely floating ice shelf.

RV1-10:

I would like to see a brief analysis of how thickness errors would affect the results. I assume this is minor, based on how thickness enters Eq 4, but it would be nice to exclude this completely.

Uncertainties in ice thickness are effectively captured by the difference between the Control Model and the Local Homogeneous Model, which represent the lower and upper bounds of the present ice thickness distribution. The range of flexure curves resulting from these two thickness distributions is smaller than the residual misfit to the observed flexure profile from DInSAR (Fig. 9k). This suggests that ice thickness variations alone cannot explain the observed flexure, and an additional weakening mechanism is required to match DInSAR. In our analysis, weakening of the lateral shear margins provided the best match to the observed flexure profile (Fig. 9l).

Line comments

all agreed

Figure 2: The scales appear distorted in b (Antarctica is the wrong shape). The axes should be checked so that squares are square.

The map inset was indeed scaled, which lead to the distortion.

L209: It is not clear whether the adjusted maps are alpha itself or the DInSAR measurement after adjustment

L230: How does a fulcrum facilitate transmission?

L289: Reduced accuracy makes it sound worse; improvement like this is normally referred to as greater accuracy

The reviewer was right that the least-squares adjustment improves the misfit to virtually zero, so we re-worded this sentence accordingly.

L299: “Notoriously” is hyperbolic and unnecessary. Simply remove it.

Removed.

L305: Not clear what it means to “perform…combination”

DInSAR imagery captures tidal flexure as a combination of tides (+1/-2/+1). Our methodology separates these images into single-tide components, which are then recombined for comparison with the original DInSAR imagery. We refer to the original imagery as 'Measurement' and the recombined version as 'Mosaic.' This step is crucial for demonstrating the robustness of our method, particularly for different DInSAR images acquired at various stages of the tidal cycle. This is why we present Fig. 7, which shows that their difference is essentially zero.

Figure 7: Plotting in blue on top of an image with surface meltwater is just confusing. I suggest making the background image black and white throughout

We have applied a grayscale version of the background image for all figures that use a blue colormap. However, we retained the original colored image for other figures, as the distinct blue ice surface of Priestley Glacier is an important feature to sidestep the firn problem (as also noted by RV2-1).

L380: I think this sentence needs rephrasing—does the IBE really do the reduction?

Reworded. The reduction is from tidal loading AND the IBE

L391: Tide deflection ratio is not defined—is this alpha?

Yes it is.

L429: The crystal lattice typically refers to sub-grain structure (i.e., the arrangement of molecules), not the aggregate of grains as used here. Suggest “the polycrystal” instead.

Reworded.

Throughout: hyphens are only used between double nouns when they modify something. So “raise sea level” is correct, as is “sea-level rise,” but it is incorrect to write “raise sea-level.” There are a number of errors in this vein in the manuscript.

Re-hyphened accordingly