

We appreciate the feedback from both reviewers and the editor which have substantially improved the manuscript.

Major Comments #1:

My biggest request for changes to the paper is that I would like to see some more work fleshing out the uncertainty around the simple 1D age-depth model used to date the timing of ice dynamic changes. The authors used both a Nye model (constant vertical strain rate) and a Dansgaard-Johnsen model (piecewise linear distribution of strain rate) to date their layers. However, they did not vary the same parameters for both models: they varied surface accumulation rate and basal melt rate for the Nye model, and surface accumulation rate and shape factor for the DJ model. They presented both models as equally likely in Figure 7; however, the Nye model is a special case of the DJ model, corresponding to the limit where $h=0$ and $\phi=1$. Finally, they did not include the effect of unsteady conditions in their age-depth modeling. Both age-depth models they used are steady state models, but this paper is explicitly focused on reconstructing unsteady dynamics. As a result, their age constraints are only “qualitative” (their word; L418) rather than quantitative.

I think that with a bit more work, they can transform their qualitative chronology into a quantitative one with defined uncertainty. In my opinion, the first step towards doing that would be to use a single age-depth model. As I mentioned, the Nye model is a special case of the DJ model corresponding to the limit where $h=0$ and $\phi=1$, so it is not necessary to include it as a separate model. If the authors consolidate their age-depth modeling into a single DJ model, then it will be easier to translate uncertain distributions of the input parameters into a distribution of the output age. Second, this single DJ model will need to incorporate a basal melt rate. I’m not sure if the DJ model has an analytic solution with nonzero basal melting, but in any event it should be quite easy to compute a numerical solution if not. Finally, they need to account for the uncertainty in the average thinning or thickening rate since the layers were deposited. This will produce a total of four free parameters: surface accumulation rate, basal melt rate, shape factor, and ice thickening rate. One way to display this parameter space would be to select 2D slices, as the authors did in their Figure 7. However, another option is to bootstrap the assumed distributions of the input parameters into a distribution of the layer ages. The authors have already made assumptions about the reasonable range of these parameters (other than thickening rate), and additionally made the implicit assumption that they are uniformly distributed (by presenting their 2D slices as though all regions of the slice are equally likely). They can keep the uniform distribution assumption or make other assumptions about these parameters (for instance, choose a central estimate with a Gaussian uncertainty). Either way, since the DJ model is very computationally cheap, it would be easy to sample the parameter space associated with the assumed distributions and produce probability distribution functions for layer age, thus transforming their qualitative chronology into a quantitative one with error bars.

To be clear, my above comments are a suggestion, not a command. The authors are free to choose different methods. They are also free to provide an argument as to why a more advanced treatment of layer ages is not appropriate or not possible. However, I think that some

attempt to tackle this problem head on- either more rigorous age-depth modeling, or an explanation of why more rigorous age-depth modeling cannot be done- would go a long way towards completing this paper.

The reviewer has raised several good points. The best way to date the stratigraphy in this area would be with cores and radar observations that connect core depth-age scale to radar layer stratigraphy and observed stratigraphic features (crevasses/onset of disturbed layers). Our approach intends to balance estimating feature ages plagued by imperfect assumptions inherent in the model (some of these assumptions have been pointed out by both reviewers) with assumptions for the processes that initiate these features (i.e. processes that drive layer disturbances and promote crevasses) which are also uncertain. We don't have a strong prior for how accumulation in this region has varied over time and space; parameters that describe the rheology (i.e., stress exponent, enhancement factor) are similarly poorly constrained.

Our goal is to get a rough estimate of the depth age structure at locations where we see disturbed layering with depth. The reviewer suggests using four parameters to evaluate the sensitivity of the changes in the depth age structure and take a more quantitative approach to the depth age solution. The DJ model we've implemented can be used to evaluate the vertical velocity of the column given a surface velocity, a kink height, an accumulation rate, and an ice thickness (this is how we made figure 7). It was not derived with a basal melt rate, but this matters less for a feature that we measure relative to the surface and in a location where we know the accumulation rates are larger than the basal melting rates.

$w = (2 * (1 - z / \text{thickness}) - \text{kink_height}) / (2 - \text{kink_height}) * a$ (equation 10. 1969)

We can then integrate this equation forward in time to get some estimate of the layer depth age structure (assuming that the horizontal flow divergence is 0).

This model is a kinematic 1D model, and isn't appropriate for treating thickness change due to dynamic thinning as this depends on horizontal velocity divergence, which we must assume is zero. To include these processes would require a simple flow line model and would introduce additional parameters for the horizontal velocity in the model.

We don't have great constraints on the kink height, but show in the existing version of the manuscript the degree to which this affects the age profile. We similarly do not know the accumulation rate history in this localized region, or the nature of accumulation rate variability. Reconstructions from ice cores exist and we could scale these with existing observations, but to use this to then state a formal uncertainty in the layer age does not capture the strong assumptions we have made in the process. We prefer the qualitative method we have articulated in the manuscript as it better reflects the state of the field and the assumptions required to make these claims.

Minor Comments #1:

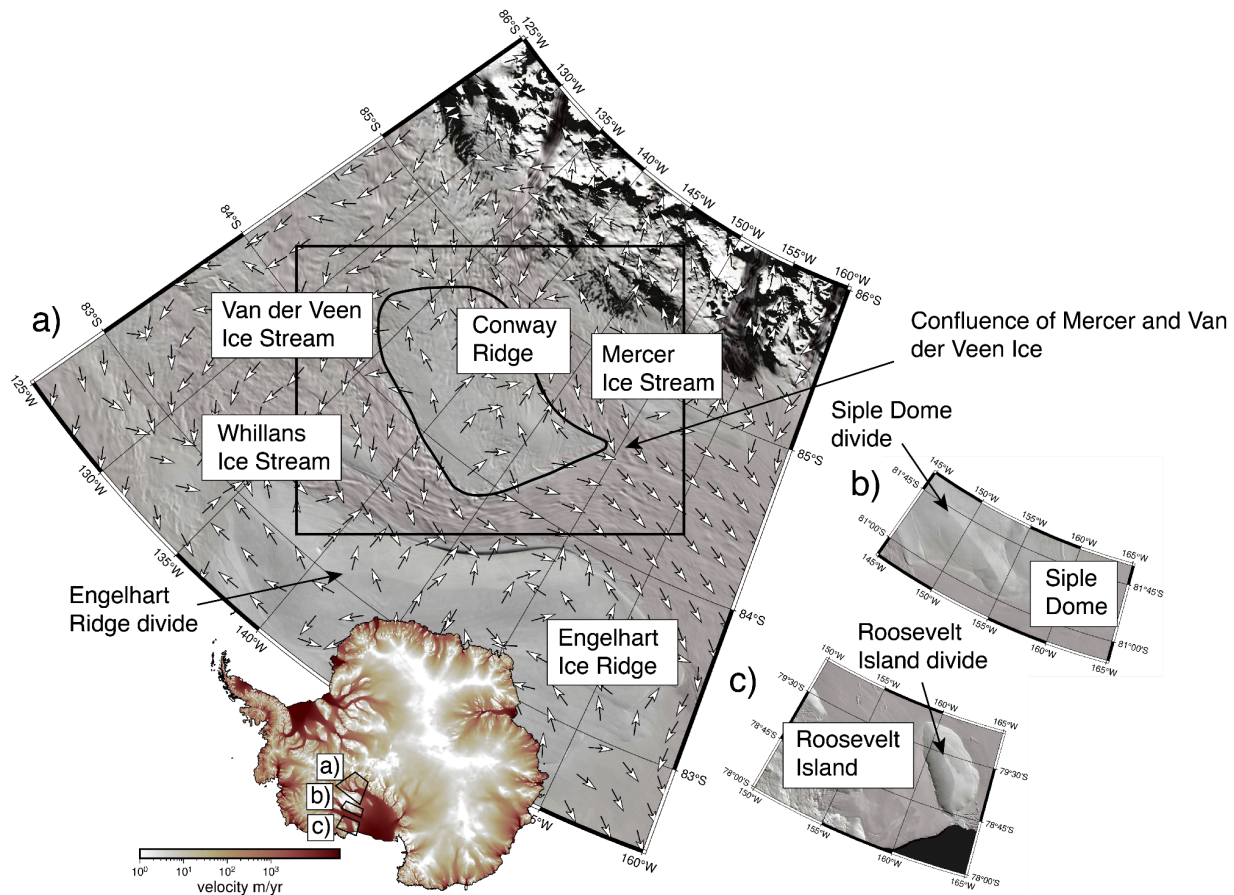
L38-39: "...make the Siple and Gould Coast ice streams a compelling natural laboratory for understanding ice-stream evolution."

We appreciate the point that there are reasons why the Siple and Gould Coasts are unique and that the language used in this front material may contribute to the perception that these systems are representative when they are not. The models we applied have been used to understand the mechanics of sliding on Thwaites, and so, though the systems are different, our approach can translate to studies of these other more vulnerable marine outlet glacier systems alluded to by the reviewer.

Ground-based geophysical surveys present logistical challenges that make it difficult to propose and conduct this kind of work anywhere in Antarctica in a targeted survey so use of existing legacy data is valuable. Efforts like the International Thwaites Glacier Collaboration have taken several years to realize in part because there are no permanent research stations in the Amundsen Sea. All equipment and personnel must be flown in from far-field stations. Working where possible and using extensive pre-existing data archive(e.g. Siple and Gould coasts) can lead to new interpretations (e.g. importance of ice stream/glacier sliding) that can inform models developed and applied to more isolated regions.

Figure 1. It would be nice to show the velocity direction vectors either here or in a later figure, especially given the discussion about divides or the lack thereof, and the discussion later on about flow across the promontory

The data collected as part of fieldwork presented in this study was conducted based on RADARSAT-1 images shown in figure 1 that are sensitive to surface slopes. These RADARSAT-1 scenes weren't collected with enough separation to get velocities since all the images collected this far south were collected over about a month. The satellite velocity data that we now have was not available at the time these data were collected and did not inform the survey. These data also have limitations in slow flowing regions where the surface velocities are large compared to their uncertainties. I have added these data to the figure (for instance below is an example).



L90: “field seasons conducted in the austral summers of 2001-2002 and 2003-2004” It’s wonderful that we’re still getting use out of data that’s more than twenty years old.

[We appreciate the recognition of these data here.](#)

L121-122: “The corrected bed-returned power and depth-averaged and depth-variable englacial attenuation rates for individual traces were calculated for multiple reflectors in each trace”

[This was mentioned by the other reviewer as well. We have removed the mention of the reflection amplitude here, but leave this analysis in the supplement as a standard product that should be made public with the data.](#)

L132-134: “We choose to apply two relatively simple depth-age models due to spatiotemporal uncertainty in input parameters (accumulation rate, basal-melt rate, and ice-flow parameters) and vary these parameters over physically plausible ranges to estimate the age of englacial layers.”

[The steadiness of the flow is a strong assumption for the depth-age relationship, as are the underlying assumptions for thickness and rheology. We hope that the figure displaying ranges of ages for a chosen depth answers this question. We don’t want to assume a normal \(Gaussian distribution\) for parameters when we don’t know what their error structure is. Without ice core observations, it’s difficult to estimate/reconstruct accumulation in a region where we think there](#)

have been changes, and so we prefer the method we've included. We appreciate that the two models we explore are related to one another. The Nye model would be more suitable for fast flowing environments (where vertical shear is zero, and in these environments the flow).

Figure 2.

The SAR image is used as a background image for surface raster images of velocity, velocity change and elevation change. We would prefer to keep this image to orient the readers to the image.

L167-168: "This diagnostic approach allows us to solve for ice-flow speeds without prescribing accumulation and ocean melt-rate forcing"

The dh/dt field computed from the model just considers the velocity divergence.

L175, Equation 1

We have added the shelf stream citation to the main text.

L178-179: "We include thermal softening by coupling the 1D thermal model of Meyer and Minchew (2018) to solve for the average ice temperature at every grid point."

Do you take the column average of temperature before computing the rheology, or compute the rheology first and then take the column-average? Because of the nonlinearity of the relationship between temperature and rheology, you will not get the same results both ways. The correct way is to first compute vertically variable rheological stiffness, B , and then take the column average of B . Note that you should use stiffness B , not softness A , for this calculation, because stiffness is linear in the stress balance equations (effective viscosity η is directly proportional to B) but softness is not. From this description, it sounds like you did this the wrong way, by first computing the column-average of temperature, then using that to compute rheology. If that's the case, then I don't want to say that you should throw out all of your results and completely redo the model, but you should at least do a sensitivity test where you compute the column-average rheology correctly to see how much of a difference it makes to your results.

We compute the temperature of the column, and then take the column average of the ice softness, A . We have added text that clarifies this important step, as we agree taking A from the average temperature would neglect the non-linear nature of this thermodynamic softening. Additionally, we agree with the reviewer that it would be more proper for us to have taken the column average of stiffness, B , not softness. We have run sensitivity studies to quantify this effect and find very little variation in results near the promontory, but some difference near the van der Veen ice stream shear margin (Region II). The overall stress state is very similar as well. In the figure below, the left column (labeled A) is with ice softness being averaged over the column, and the right column (labeled B) is with ice stiffness being averaged over the column. The 2nd row is ice speed difference relative to the case where ice softness is averaged over the column. The 3rd row is the surface stresses as output from our model.

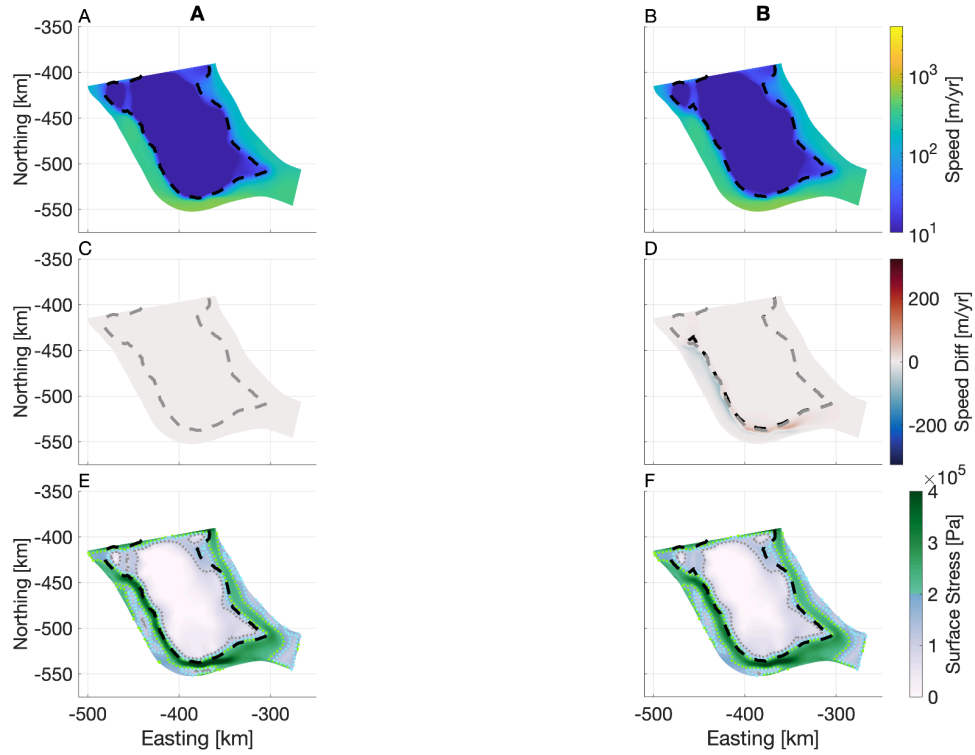
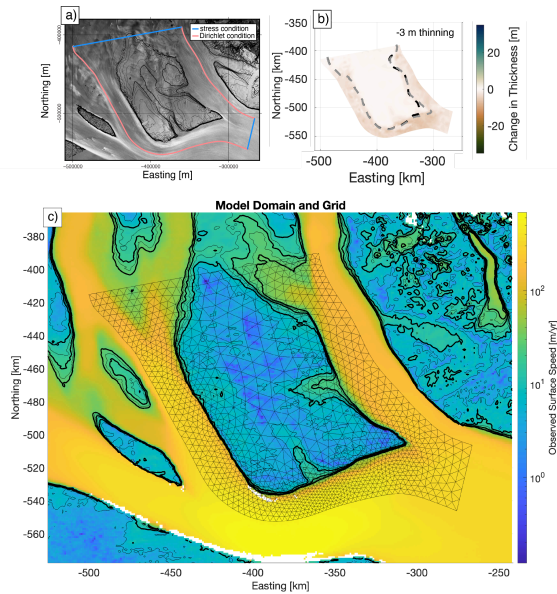


Figure 3

We have removed Figure 3 and now just refer the reader to the model domain figure in the supplement, which has been expanded to include the figures that were part of Figure 3.



L189-190: “We interpolated the basal strength field from estimates of the basal resistance inferred using the Ice-sheet and Sea-level System Model”

The use of an inverted friction field means that you are, to some extent, enforcing the locations of shear margins and fast flow because the inverted friction field is constrained by the observed present-day velocity. This somewhat undercuts the claim that your model can be used to solve for the location of the shear margins. It also goes a long way towards explaining why your model was incapable of simulating the narrowing of van der Veen ice stream. However, it also means that when your model did simulate a change in shear margin location (at the promontory) that that result is more likely to be robust, because a model using a present-day inverted friction field is always going to be biased towards reproducing the present-day flow pattern.

The reviewer is correct in that our assumption of constant basal friction from inversion of present day sliding velocities would bias our models towards the current ice-flow velocity distribution. Another framing is that our model can solve for shear-margin position within the limitation of constant basal conditions. This restraint is largely driven by the limited constraint on what past basal conditions and basal friction may have been. We agree that this limitation in tandem with the finding that we did not reproduce the narrowing of the van der Veen ice stream suggests that the basal friction distribution may have changed from current values to reproduce a narrower van der Veen ice stream. This connection between basal friction transitions driving shear-margin migration is well motivated for ice streams and specifically studied for the neighboring Kamb ice stream (e.g. Winsborrow, 2010, Elsworth & Suckale, 2016). To explore how interior regions that are not flowing quickly, respond/weaken is outside the scope of this study. We do modify the scale of basal friction as it approaches floatation, but, as the reviewer notes, this is a very diffuse signal and the fine scale (1-10s km) distribution of basal friction is preserved, which biases ice velocity towards the modern flow configuration. We also agree with the reviewer that the finding of migration at the promontory is more robust in that we show that migration of the margin is possible here, even without significant changes in basal friction. We have added text to the discussion clarifying this limitation and how our results imply that basal friction changes are likely responsible for the modern widening of the van der Veen ice stream.

L252-253: "Near the confluence of Mercer and Whillans Ice Streams, there is also a promontory where ice flows slowly from Mercer Ice Stream towards Whillans Ice Stream."

This is the sort of thing which would be easier to visualize if you included velocity vectors in one of your figures.

We agree here and have modified the velocity vectors accordingly. We have also labeled this confluence zone and the divide locations in Figure 1.

Figure 4 - The caption should specify that the color scale representing basal roughness is only displayed along the flight line. When I first saw this picture I thought that the main map was showing two color scales simultaneously and I was confused. In addition, the background map should be simplified. It looks as though you plot the background SAR imagery and the velocity colors on top of each other, but the brightness variations from the SAR background make it harder to interpret the velocity data. I would say to either plot one or the other, don't plot both of them in the same location. The same comment applies to figures 5 and 6 as well.

We appreciate the suggestion and have edited the Figure 4 caption accordingly. We think the SAR imagery adds context for the ridge and defer to the editor on how best to plot these results. The SAR imagery in the case of the velocity field also demonstrates the imagery that is used to inform the surveys presented here (surface velocities were not readily available at the time when these data were collected, but the backscatter imagery was available).

Figure 8 Would it be an accurate interpretation of this figure to say that the model can reproduce small changes in shear margin position at the promontory, but not the larger changes observed elsewhere in the domain? As I mentioned above, this is probably related to the fact that the model is forced by inverted basal drag, which is constrained by the present-day velocity field. Also, what do the contours represent in plot a?

The balance is a global solution over the entire area where we solve our equations, so any changes in one part of the domain will depend on stresses in the other. Where we apply the largest dh/dt field is where we see the largest change in response (and where the ice is closest to floatation). It would be accurate to say that this model, assuming a distribution of basal friction based on current inversions, is unable to reproduce the changes elsewhere in the domain. As the reviewer comments, this likely is due to the larger changes being triggered by changes in basal friction configuration, but behavior around the promontory seems unique in that it can be reproduced with only changes to surface height of the ice sheet in this region. We have edited the discussion of our results to emphasize this point. The contours in (a) represent the observed speed of the ice, with major contours at 10, 20, 30 m/yr. We have added this to the caption.

L391-392: "Our simulations show little additional inland migration when we prescribe additional surface thinning"

A weakness of your argument here is that you only tested surface thinning fields obtained by extrapolating the pattern of present-day thinning into the past. However, the spatial pattern of thinning could have been different if the ice sheet configuration was different. In particular, the present-day pattern has thinning mostly co-located with fast flow, and if the old area of fast flow was more extensive, then the region of thinning might have been as well.

The present rapid flow is a result of weak substrate (e.g. the bed in these regions is weak) to get thinning in regions that are not flowing quickly, we need steepening which requires downstream thinning. For these reasons, we focus on how modern observations that constrain the stress balance fit into the context of past observations of flow.

L406-413: Paragraph about the model not being able to reproduce the changes in the northern van der Veen Shear Margin. I am glad that you included this discussion here. It would have been a major omission if you left it out. I would suggest adding two things to this discussion: 1) the fact that the model uses a basal drag field tuned to the present-day velocity field, and 2) the fact that the spatial pattern of thinning used in the perturbation experiments is also largely confined to the present-day fast flow.

We will make these changes. Maybe a more fundamental assumption is that the stress balance at the base of the ice sheet changes less in time than the forcing that adjusts flux and changes the distribution of stress (which our model can explore).

Figure 9 Is this schematic supposed to show a narrowing of van der Veen? It looks the same width to me.

It is very similar to the same width and the figure is focused on the promontory where we have the strongest observations of past change. The changes where we constrain changes in van der Veen width are actually on the south side of the ridge which isn't visible in illustration.

Appendix A: What is even the point of including the reflectivity analysis here? It turned out to be unreliable and you didn't actually use the results for anything. Is the point merely to document everything you tried, regardless of whether or not it worked?

This was mentioned earlier in the review and by the other reviewer. We include the reflectivity analysis here for completeness.

L511-512: "For both methods, we define the full thickness as 100 m below the surface to 85% of ~ the ice thickness to avoid the direct arrival and low signal-to-noise deep in the ice column."

This is a fundamental problem with any attempt to use englacial reflectors to generate an attenuation correction for the bed, and probably contributed to the unreliability of your results. The problem is that, for typical ice sheet thermal profiles, the warmest ice (and hence the most powerful attenuation) is found near the bed. So an analysis using only englacial reflectors can't estimate the attenuation rate of the most important part of the ice column! This is in addition to the other problems you mentioned (such as layer slopes).

This is true, and is why we do not include the deep ice.

Appendix C: Both age models assume steady state, however, a key point of this paper is that this region is not in steady state. You should consider running some tests to see what the effect of unsteady agedepth profiles might be. In addition, the Dansgaard-Johnson model does not include a basal melt rate, which doesn't really make sense considering that the basal melt rate was one of the two key parameters you tested for the Nye model. See my major comments above for more thoughts about the age-depth modeling.

In response to comments from both reviewers, we have run some new tests for the depth-age model.

Appendix D: What is the point of discussing the constant basal drag model? As with the reflectivity analysis in Appendix A, it did not end up being reliable and you didn't end up doing anything with it. Again, is the point merely to document everything you tried, regardless of whether or not it worked? To my mind there is no point including the constant basal drag model in this paper.

It shows that resistance in this area is very dependent on the traction at the bed. This is not true in all locations. For instance, the internal shear margins of Thwaites Glacier have been linked to the basal topography.

L603-604: "All of the simulations presented in the main text are steady-state solutions that solve for the velocity associated with different assumptions for the ice thickness, boundary velocity, and basal strength."

We've changed this to the stress-balance snapshots.

Figure D3: “Black lines in elevation perturbation experiment show changes in height above flotation relative no change in height above flotation.”

We appreciate the gentle suggestion and have made this change.

L31-33: Names of projects and references. It feels weird to have both the names of the projects and the literature references grouped together in the same parentheses. In particular, it is jarring that there is no break after SWAIS2c to separate the project names from the author names. I don't know what the right style guideline is here, but consider changing this so that it reads easier.

We have changed this sentence to separate collective projects from resulting studies. We are also open to removing the project names and alternatively focusing on the studies that have come from these projects.

L41: (+/-0.5° C) Remove the parentheses and use a real \pm sign

Done. Thanks for the suggestion.

L404: “as ice sheets widen” Should be “as ice streams widen”.

Thanks for the suggestion!

Major Comments #2

Firstly, you performed a large set of model experiments to investigate the sensitivity of the ridge to three prescribed perturbations. You included very few visualizations of the results in the text, but a large amount of plots in the appendix. What I am missing is a synthesis plot of the ensemble, which quantifies the sensitivity of the ridge to these perturbations in your model. For example a scatter plot that explicitly shows the required conditions to reach the crevassing von Mises stress in your model.

The experiments are each distinct, and we include all experiments in the supplement for clarity. The simulations are summarized in Figure 8.

Secondly, I was wondering how your steady-state model results would transfer to transient simulations. In the text, you do not clearly specify that this is all in steady-state, while you are studying ‘the least stable of the Ross inter-ice stream ridges’, which can be a bit confusing. In case you ran non-steady-state simulations this would be very interesting to include, otherwise I understand that this would be a lot more work, maybe outside the scope of this paper or reserved of another paper.

The simulations are not necessarily in steady state, nor do they rely on steady-state assumptions. These simulations are just *diagnostic simulations* (momentum balance) of the velocity consistent with the prescribed thickness and assumptions for viscosity and basal resistance. They do not incorporate surface mass balance or external forcing terms and therefore are not intended to represent a climatic steady state. In the mathematical sense, steady state would require the rate of thickness change to vanish—that is, a balance between

velocity divergence and surface mass balance—which would require a *prognostic simulation* and additional forcing information not available for this site.

The 1D model layer modeling does however rely on steady-state assumptions, and we recognize this is a strong assumption but our application in this case is to date near surface features. We appreciate your comment, and refer the reviewer to our response to the first reviewer on these assumptions and the very clear explanation of this model as a qualitative tool.

Figure 2. We are plotting data transparently on top of the SAR image. The SAR image is designed to serve as a reference for the other images.

We are plotting data semi-transparently on top of the SAR imagery. The SAR image is designed to serve as a reference for the images making it possible to locate changes/features from one panel to another. The colormaps themselves are perceptually uniform (intended for folks with colorblindness), and it's our view that perceptual uniformity is more important than the colors used and is the official preference of the journal. We can change these figures depending on the views of the editor.

Figure 3. This figure does not bring much information. Panel a: we have already seen the model domain in Figure 2, and you can describe the boundary conditions in the text. It can eventually go in the appendix, maybe merged with Fig. D1. The units and fonts between panel a and b are different. Panel b: the bounds of the colorbar are too large compared to the displayed values. If you want to keep figure 3 for illustrating the experiment, it would be nice to see as well one plot of the velocity perturbation. Instead of the gray and black contours (which I guess are already results for this experiment), replace with the velocity perturbation contour.

We think that it helps to label this image that focuses on the model domain separately from the image in Figure 2. We think it's important to be very clear about the boundary conditions that are applied in the model and using colored boundaries is helpful in this regard. In response to the reviewer's comment, we have moved this figure to the supplement.

Figure 4-6. Panel a) is a repetition of a plot we have already seen, it does not bring more information except the bed roughness, which is not easily interpretable anyways. But you show the bed roughness along the transect on panel b) (and Appendix) so you could get rid of panel a). I understand that you still need to show the location of the transects. I don't know if this can be combined with figure 2. Or a smaller map not labeled in the corner of the figure, showing only the region contours, surface speed and the radar line location. This will give more space for the radargrams.

Panel A is repetitive, but we believe it shows where the plots are. We think this is important, especially in a journal like the cryosphere where we are not restricted by page number or figure requirements.

Section 3.3, paragraph 2: You only describe the figures here which should be in the legends and the captions of the figures. For example [344-345], you could add a legend box on figure 8 with

'- - : 30m.yr-1 contour'. This would make the text more concise, and you could provide a more quantitative analysis of the sensitivity in this section.

This paragraph does describe the contents of the figure, and we've restructured and deleted some text to streamline the content. We also appreciate the suggestion to include a legend with the dotted contours in the figure. We have made these changes to Figure 8.

356-360]: Those results are interesting, but only shown in the appendix. Also it's not so evident to see the difference in Figure D5, supporting the idea of a quantitative plot in this model result section as I mentioned in major comments.

Figure D5 shows the velocity and stress response of the ice for different thinning configurations. We included three of the most important simulation results in Figure 8 (b-d) and have made this more clear in the text. Figure 8 was designed to be the summary figure that the reviewer is requesting, and this is why it includes an overview of the ridge (8a). In this panel, we show that velocity changes in conjunction with thinning and/or weakening change the surface stresses that could promote crevassing, which we see evidence for in the radar observations.

Figure 8: There are figures that we have already seen (a) and h)). Furthermore, we do not see much the differences between b), c) and d). Maybe a single bigger plot with color coded/labeled dashed line, showing the retreat of the promontory against thinning (potentially including more scenarios). I like e), f) and g), it zooms on the region and use a clear color threshold. However it looks much smoother than your model resolution in Fig D1?

These results in figures 8a and 8h are designed to serve as a summary that brings the survey and the modeling together. We appreciate the reviewer's careful attention to repeated figures and content, but would prefer that panels a and h remain in this figure for readers who skip the introductory material and focus their attention on our results, which were motivated entirely by observations shown in a and h. Going back to the previous comment, this figure is designed as a summary with the main take-away points for the reader. It is our view that folks who are interested in the details of this region's sensitivity to prescribed thinning/weakening are better served by the comprehensive simulation figures in the supplement.

[368-370] if I understand correctly, you need both velocity perturbation and the HAF basal strength to exceed the von Mises stress? It would be worth adding a conclusion paragraph to this section, synthesizing the required conditions to reach the crevasse proxy threshold in the model.

This is not a requirement. We exceed the von mises stress with just a velocity perturbation; see figure.

Figure 9: It is a good idea to include such a schematic. However, we do not really see that the ice streams were larger 3kyr ago as mention in the caption.

The changes are subtle and focus here on the promontory, where we know flow was faster. We don't want to exaggerate these changes.

Conclusion: Here you conclude that your model was able to produce sufficient past conditions to create surface crevasses at the ridge promontory, through a phase of faster flow and thinner ice

streams. However, if I understand correctly, your model reaches those conditions only when using an HAF dependent basal strength. Maybe this should be mentioned?

We achieve conditions that could promote surface crevassing for simulations where there is faster flow and thinner ice without the thickness dependent basal strength. See, for instance, appendix Figure D4.

[109] & [113]: repeated citation for the Reference Elevation Model of Antarctica, use the acronym?

We've abbreviated the citation.

[130] traced traced

We have deleted the repeated traced. Thank you!

[159]: use active citation

The Smith et al., 2009 study was the first to document the activity of Subglacial lake Conway. We can include more studies (for instance, Fricker et al., 2009, Siegfried et al., 2014, and Siegfried et al., 2023). Is that what you are looking for here?

Fricker H. A., Scambos T. (2009) ,Connected subglacial lake activity on lower Mercer and Whillans Ice Streams, West Antarctica, 2003–2008. *Journal of Glaciology*. 55(190):303-315. doi:10.3189/002214309788608813

Siegfried, M. R., H. A. Fricker, M. Roberts, T. A. Scambos, and S. Tulaczyk (2014), A decade of West Antarctic subglacial lake interactions from combined ICESat and CryoSat-2 altimetry, *Geophys. Res. Lett.*, 41, 891–898, doi:[10.1002/2013GL058616](https://doi.org/10.1002/2013GL058616).

Siegfried, M. R., Venturelli, R. A., Patterson, M. O., Arnuk, W., Campbell, T. D., Gustafson, C. D., ... & SALSA Science Team. (2023). The life and death of a subglacial lake in West Antarctica. *Geology*, 51(5), 434-438.

[253]: 'and van der Veen Ice Stream'. Should it be Whillans Ice Stream? Maybe I am just confused about where van der Veen and Whillans merge. Reading the following text it sounds like it should be Whillans to me.

We've changed van der Veen Ice Stream to Whillans Ice Stream.

[269]: should it be 'southeastern ridge (region IV)'?

This should be southwestern ridge (not southeastern).

[271]: Fig. 2b is referred as ice thickness but it is bed elevation.

Thank you, good catch! We have rewritten this as bed elevation..

[295]: 'in Figure 4c km 50-72'. Should it be Figure 6c?

This should be Figure 6c. Thank you!

[325]: Should it be Appendix C?

Yes, good catch. See change.

[456] repetition of 'northern half of the ridge (region II)'

See change! Thank you!

[552]: 'features'

See change! Thank you!

[Figure D2] 2x 'to' in caption

I deleted the extra "to". Thank you!

[Figure D4,5,6, 9] are the green and blue dotted lines necessary? There are difficult to see on the computer and invisible on print. - Figure D6: there is a tool bar on panel Q

Thanks we have deleted the toolbar.

[Figure C1] These are polar stereographic coordinates. Thank you.

We've made all of the figures larger so that they can be more easily read. Thank you!