I cannot remember when I have had more helpful reviews than those given by the two people who carefully went through my paper. The fact that they both noted some of the same issues made it certain that I had to fix a few things. I describe the planned changes in detail after each of the related reviewer comments which are italicized.

I did have to abandon my effort to make the paper totally analytic in that I had to do numerical integrations to show the effect of non-linear temperature profiles. Since this took considerable effort, I may not have addressed all the other comments as well as I should. The most important changes are:

1) To try to make the model idea better I plan to add a description of viscous bending to the introduction and in section 3.3 on the thin plate approximation of the flexural wavelength.

2) To illustrate the asymptotic behavior of the analytic model I added a panel to Figure 5 showing predicted internal bending moments versus the log of the e-folding length for viscosity variations.

3) I analyzed the errors in the analytic approximation by carrying out numerical integration of the stress differences for the assumed ice flow law. The difference between the full and approximate solutions for the internal bending moment depend on the assumed flow law parameters and surface temperature, but are less than 3% for the most extreme cases illustrated in the figures.

4) A new section on "Effects of nonlinear temperature variation with depth" will include (1) a figure showing temperatures from 3 boreholes on the Ross Ice Shelf and 1 from the Amery Ice shelf, and (2) calculation of steady-state temperature profiles for a range of rates of surface accretion and basal accretion or melting (after Robin, 1955) and results of numerical analysis of internal bending moments for those temperature profiles. The Figures are given below.

Reviewer 2

Summary:

The manuscript extends the theory first developed in Reeh. 1968 and convincingly proposes a new theory that can explain upward ice shelf bending without requiring the introduction of geometric features that aren't always observed, as required the "bench" model. The paper is well-written and impactful while managing to remain brief. I do think some improvement in clarity could be offered by increasing the level of detail for some conceptual ideas. The paper, overall, is excellent and warrants prompt publication.

Suggestions:

1. *In the conceptual model section, bending generated by vertical variation in horizontal stress and associated internal moments is discussed. However, I am a little confused about the direction of the bending moment. If "there is a decrease in extensional stress with depth," would this not correspond to a top out sense of bending? I am sure I am misunderstanding this in a perhaps amateurish way, but I am also sure that other readers will have this same confusion. I think the manuscript would benefit from a bit more detail explaining, from a*

conceptual perspective, the direction of internal moments generated by horizontal stresses that decrease in magnitude with depth, and the direction of the equivalent edge moment. Right now, it feels like clarity is sacrificed a bit in exchange for brevity.

I had to think about this for a while and this is what I have come up with so far:

"The effect of internal moments in layer with zero external moments can is equivalent to applying external moments of the opposite sign and same magnitude as the internal moments to a layer with zero internal moments. The problem of bending boundary conditions is analogous to the simple problem of uniaxial stress. Consider a finite length elastic bar with stress free sides. One end of the bar is fixed and the other end can be pushed or pulled. If the bar is initially unstressed and then pushed with a horizontal compressive force F_C the end will be pushed in a distance Δx and the internal horizontal stress will be uniformly compressive. The deformation is the same if the layer was first pulled with an extensional force so that the initial internal force $F_E = - F_C$. Setting the boundary force to zero would result in compression of the layer and movement of the layer by a distance Δx. So the deformation is the same if a force opposite to the initial internal force is applied to the and unstressed bar."

Detailed comments:

[1] I feel that the introduction could flow slightly better if the first paragraph started broad and then narrowed (i.e. ice shelf breakup is important because it can enhance sea level rise -> one of the processes that cause breakup is bending -> bending causes breakup in the following two ways). This is just personal preference and does not need to be addressed unless the author agrees.

Good idea. I will do that in the revised introduction.

[40-41] Could be a good idea to mention a few ice shelves that show bending consistent with the typical downward bending and which ice shelves show the opposite sense of bending.

I will do this and describe observations in Scambos et al. (2005).

[51-57] This is written with panache and is fun to read!

I was surprised to find the line in Reeh (1968) buried in his model description and neither in the introduction nor the conclusions.

[65] Presumably the gray lines are flow lines, but it might be good to mention this briefly in the caption.

As noted in the response to reviewer 1 I now describe the figure more completely and note that the grey lines are flow lines (see above).

[68] For context, it could be good to say something like "Ice shelves that are not heavily buttressed are under extension (Weertman, 1957). While ice shelves are typically assumed to

have negligible vertical gradients in horizontal stress, significant vertical variation in viscosity generates vertical gradients in horizontal stress that cannot be neglected."

Good suggestion. I will make that change.

[74] After this, we look at a couple examples in other geophysical settings. It could be good to end this paragraph, or start the next, with a sentence like "Similar physics are observed in several geophysical settings." Right now, the jump is a little abrupt feeling.

I'll add sentences noting the origin of the internal stresses in these cases, saying something like: The internal stresses in the Haxby and Parmentier (1989) model consider an increase in extensional stresses with depth arising from thermal contraction combined with shallow yielding. In the Buck (2001) model the lithosphere is accreted by addition of magmatic dikes that widen with depth in response to ridge axial stresses. This build lithosphere with intrinsic curvature that flattens with distance from the spreading axis. The flattening results in surface extensional faulting consistent with the observed growth of fault offset with distance from the axis."

[75-80] This paragraph is a little confusing. I would explicitly describe what causes vertical variation in horizontal stress in the lithospheric case. "Those authors note that gravity prevents lithospheric bending that is of much longer wavelength then the effective flexural wavelength of the layer. At very long length scales gravity prevents the layer from bending." is redundant. Is gravity generating the vertical variation in horizontal stress? I would just explain this analogous case with a bit more detail to improve clarity.

Yes, reviewer 1 also noted the redundancy and I'll cut the 2nd sentence.

[81] Remove "that that."

Yes yes.

[84] Should be "is equivalent" not "are equivalent."

Will change

[84-85] I understand the general concept that uniform internal loading is equivalent to remote loading (common in fracture mechanics, for instance, when considering stresses acting on the edges of a cracked plate and stresses acting on the interior crack walls). It is not entirely clear to me why the equivalent on the end of a layer has opposite direction, however. A bit more explanation could be helpful. This ties into the broader comment above.

See my response to "Suggestion 1" above.

[110] This may just be how the draft version is typeset, but this figure is a little confusing to see before the reference horizontal stress has actually been defined.

I will move the figure to be after the definition of the reference stress.

[143] “water pressure” not “pressures”

Will change

[196] Quick half sentence explaining e-folding could be good. Most readers will probably already know, but clarity is always to be encouraged, especially because z_0 is an important parameter moving forward.

Yes, I’ll add something like: “(i.e. the distance over which the viscosity drops by 1/e)”

[340] “may allow new constraints to be placed” not “places.”

It will be so placed.

Added references:

Craven, M., Allison, I., Fricker, H.A., Warner, R.: Properties of a marine ice layer under the Amery Ice Shelf, Antarctica, J. Glaciol. 2009; 55: 717-728. doi:10.3189/002214309789470941

Sartore, N. B., Wagner, T. J. W., Siegfried, M. R., Pujara, N., and Zoet, L. K.: Calving of Ross Ice Shelf from wave erosion and hydrostatic stresses, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-571, 2024.

Thomas, R. H. and MacAyeal, D. R.: Derived cahracteristics of the Ross Ice Shelf, Antarctica, J. Glaciol. 1982; 28:397-412. doi:10.3189/S0022143000005025