

Dear Editor,

Reynolds and others have made significant improvements to their manuscript since the last round of edits. At this stage, I do not have major comments, but a substantial quantity of minor comments that I feel would improve the readability of the manuscript, as well as a few clarifying comments, that I will list below. The larger comments tend to have sub-points a), b), etc.

We appreciate the commendation that the manuscript is significantly improved as well as the additional recommendations and corrections below, which we respond to in line.

Note that all line references for the main text come from the difference file unless otherwise specified.

1. In going through the response to reviewers, as well as using control F, I see there is a discrepancy between what lines are added to the difference file (presumably red and blue) and what is in the original pdf - <https://egusphere.copernicus.org/preprints/2024/egusphere-2024-2424/egusphere-20242424.pdf>. Please confirm if the difference file contains all new changes between rounds, and if not, then what differences are not marked with a new color.

To our knowledge and intent, “track changes” was used for the entire editing process between the first preprint and the first revised submission. The one exception to this we are aware of is added citations which are controlled by the automated citation manager and appear black (removed citations are tracked). We generally added the citations recommended by both reviewers and confirmed this in the responses to the first round of revisions. If there are other mismatches, we apologize for the error but would need examples to address the concern.

2. Line 64: My understanding is that it would be the impact of crevasses on force balance, not just water pressure in a basal crevasse. It seems that any new boundary condition, whether hydrostatic water or air, might modify the force balance in that framework.
 - a. Line 170: same point.

Thank you for this correction. We have updated old line 64 as:

OLD: Second, the horizontal force balance method (Buck, 2023) maintains the assumption that ice has no tensile strength but considers the impact water pressure in basal crevasses on force balance.

NEW: Second, the horizontal force balance method (Buck, 2023) maintains the assumption that ice has no tensile strength but considers **the impact of reduced ice thickness from surface and basal crevasses, air or water pressure in surface crevasses, and** water pressure in basal crevasses on force balance.

We have updated old line 170 as:

OLD: As surface crevasse depth and basal crevasse height increase, force is carried by a smaller cross section of ice (here termed the ligament), and basal water pressure adds force that must be counteracted by additional force from ice deformation.

NEW: As surface crevasse depth and basal crevasse height increase, force is carried by a smaller cross section of ice (here termed the ligament), and basal water pressure **as well as air or meltwater pressure in surface crevasses** adds force that must be counteracted by additional force from ice deformation.

(**bold** indicates the added text)

3. Lines 215-221: First, I am glad that you mention this important point. However, I found this text quite confusing, and found it moderately confusing after reading it with the response to reviewers and the mentioned textbook. I would recommend suggesting that you put in a discussion similar to what you responded to reviewers with. The first assumption - that τ_{yy} is approximately 0 - is roughly upheld in various parts if τ_{yy} is viscous. The second part, about the influence of young's modulus, is considering the stress state after fractures open, which would imply that validating crevasse depth theories requires the stress state incipient for fracture, not after fractures develop. LEFM (and force balance) begin to think about the stress state after fractures open, which I believe is a point that Anderson was discussing. Either truncate after the added sentence in line 215, or enhance the clarity of this paragraph.

Our understanding: Rate of crevasse formation relevant. If τ_{yy} is 0 and crevasse formation is rapid, then, the resulting yy direction stress should be controlled by ice elastic response through poisson ratio and youngs modulus. If τ_{yy} is not 0, the plane strain assumption is violated as originally noted. If the crevasse formation is slower, then the yy direction stress could relax to whatever the background state. In this case, even if the background is 0, the yy direction stress will not match the stress corresponding to a linear elastic response. But in this case LEFM may not apply anyway as raised in the conclusion of Jiménez & Duddu (2018).

Updated text 215 and on aimed at raising the issue while remaining more general.

NEW: For an elastic material in plane strain, the formation of a fracture causes a stress running parallel to the crack tip due to Poisson ratio. The additional plasticity from this stress increases the tendency to fracture (Anderson, 2005 Section 2.10). This state may be represented in glacial ice if crevasse formation is rapid and there is no far-field crevasse parallel stress. The latter assumption will be violated in some regions when applying LEFM to all strain rate states across ice sheet surfaces. Impacts of crevasse formation timescales are considered in Jiménez and Duddu (2018), Lipovsky (2020), and Clayton et al. (2024).

We have removed the discussion of the test sample, although the main tenant of LEFM is the ability to compare a crack tip state in the structure (or glacier) of concern to a laboratory test crack tip state thereby avoiding the difficulty of understanding the actual failure process at the crack tip.

4. Table 1: Here, and many places in this study, I do not feel that there is any consideration of how data availability, measurement methods in the field, etc. could contribute to the choice of using equation 23 or 24 (in general lacking a τ_2). It may be worth discussing whether data availability constraints influenced authors' choice of these formulations. Please provide some consideration of this as a possible circumstance.
 - a. Another place to discuss this is in section 5.2.

We have added discussion of this in the beginning of section 5.2. We also added a note in section 5.3 that measurement with stakes may not give crevasse parallel stress, but that so long as $\tau_2 \geq 0$, the effect is minimal as can be seen and referenced in Fig. 4e.

5. Lines 417-419: Perhaps consider the work of Surawy-Stepney and others in 2023, who show (and cite in their work) the growing evidence for crevasses and velocity change having a correlation. I think that a point could be made about coupling between flow and fracture: zero stress is fully uncoupled; damage is one-way coupled, and there are currently no theories that are two-way coupled (fractures and velocity co-evolve with equations that are simultaneously solved). I believe this may be a relevant limitation of all crevasse-depth equations that are inserted into flow laws through damage, as defined in Borstad's work.

We have cited Surawy-Stepney after the first clause.

- a. Lines 426-428: If I understand correctly, as discussed in the last point, the zero stress approximation and LEFM assume that fractures have no impact on the viscous component of the stress field, and are an uncoupled byproduct of an englacial stress distribution. If so, I think this is a relevant point to make.

Our understanding is that LEFM equates the viscous stress field with an elastic stress field and considers the increase in stress on a reducing cross section. That said, to avoid a potential falsehood, we have noted that zero stress would neglect this and have left LEFM unmentioned.

6. Lines 454-456: There are certainly problems with using SSA while considering fracturing, but you may want to add that it is among the tools at present to try to assess the validity of crevasse depth predictions, and include in your next paragraph.

We have added the following note after old lines 454-456.

OLD: ISSM is run with the shallow shelf approximation (MacAyeal, 1989).

NEW: ISSM is run with the shallow shelf approximation (MacAyeal, 1989), which cannot represent individual fractures but can be used to study the bulk rheology impact of crevasses.

7. Line 476-478: You didn't say if it's Mean Squared Error (I assume). Additionally, I always recommend including all relevant equations, for example the damage model you use and cost function, to be written out somewhere in the paper or supplement, so that a reader does not have to switch papers.

We have noted that the inversion uses mean squared error and have added the cost function equation to our supplement (Section S9).

8. Lines 481-491: In general, if something is mentioned in your paper, please give a brief explanation or takeaway-message. In this paragraph, you mention that you study other flow-law exponents, and that you study force balance. On result robustness given rheological uncertainty, later in the paper, you mentioned that your main result regarding using planar effective strain rate versus full remains with different flow law exponents. If this is the main point, I would include it again here. Similarly, your supplement shows that the force balance approach has mixed results, with better and worse nodal velocity misfit on two different ice shelves. These points would be useful to set expectations for your readers.

We have added clauses to the sentences mentioning $n=4$ and force balance that note the main takeaways.

9. Figure 4:
 - a. Make a statement somewhere about why A isn't included, even if you find it obvious.

We added a parenthetical in the caption that calc. A is the one not included based on it not using maximum principal direction stresses.

- b. In lines 518-522, you give beautiful explanations of the different x-axis points of 4e. If possible, it would be fantastic to put a small, <5 word version of these limits as text on the plot, so that readers can see "ice tongue", "1HD flow", "pure shear", etc.

Thank you. We have added names of these points and an example of where they occur to the figure.

10. Line 698-699: Please elaborate on why inverting with no damage but viscosity prefactor tuning can produce the best results, and what that implies for the reliability of your results. Specifically, it would be nice to understand as a reader,

We have made some small verbiage changes to improve clarity of why we think it makes sense that inversions can do best – namely including all factors in bulk rigidity (spatial temperature, flow law error, crevasses).

- a. why the bulk temperature can have such a large effect, and

Added discussion:

NEW: Bulk temperature is a strong tuning factor because of ice rheology's high sensitivity to temperature; however, we did not need to tune bulk temperature outside of reasonable values. The tuned depth-averaged temperatures are close to the surface temperatures, which is not unreasonable because of the advection of cold ice as can be seen from borehole measurements at the Fimbul and Amery ice shelves (Humbert, 2010; Wang et al., 2022). The tuned temperatures for the Scar Inlet and Pine Island Glacier ice shelves are discussed in Section 4.4.1 and 4.4.2.

- b. why readers should retain confidence in your results working for the “right reasons” given the approximations/limitations discussed in the introduction, when misfit minimization may suggest closer agreement to observations but for the “wrong reasons” (unphysical temperature tuning).

The temperature tuning is unphysical in that it's being tuned not directly modeled. We believe the resulting temperature to be reasonable enough based on the added text above.

11. Section 5

- a. Please use numbers or bullet points for your recommendations. Further, provide the evidence in your paper that supports each claim. That will be very clear for everyone, and say what sets your recommendations (nodal velocity misfit, or unphysical crevasse depth prediction, etc.). One idea could be to make it chronological with sections of the paper, which gets to the point of why there are only two stress calculations studied with nodal velocity misfit in the main text.
 - i. Example: Line 680: Because EF performs better than EP across all metrics, including the final metric of nodal velocity difference, I think it is the most well-defended point of your paper, as it is stated as the primary takeaway in Line 695. This could be the first/last point, with the corresponding figures that defend this claim.

Thank you for this suggestion for improving clarity and the strength of our recommendations. We have re-written and re-ordered section 5.1 to better follow the order in the paper: 6 calculations -> remove flow dir based on missing shear margin crevasses -> remove 2d Rxx based on over-predicting shear margin crevasses -> calc F over calc E based on

modeling. We did not number recommendations as the recommendation is simply use calc. F with some caveats noted.

- b. You should also note in this section that you analyzed results based on the zero stress approximation only in the main text, and all the results that depend on this approximation.

Added to the second to last sentence of 5.1.

- 12. Lines 875 and 830: both discuss the physical basis of the zero stress approximation. Could you elaborate on what you mean by this? Do you imply that other crevasse theories are not physical, or that studies have not implemented the zero stress approximation properly?

- a. For example, if the implication is that other crevasse theories are not physical, I would consider the following. In a simple width-averaged ice sheet model, with basal crevasses below surface crevasses, a zero stress crack depth would not result in calving, as the stress required would be twice that of the ice front.

We did not intend any comparison / claims about the physical basis of zero stress vs force balance vs LEFM. Our meaning is more consistent with the latter statement, “studies have not implemented the zero stress approximation properly.” We also do not claim that studies have implemented LEFM improperly, but we do raise the point that these stress calculations will yield large differences in LEFM workflows that more or less follow the differences that would occur for the zero stress approximation.

Our claim is that the zero stress approximation seems to work best in accordance with its physical meaning (crevasse ends where the max principal full stress term reaches zero (noted in old 830-831)) rather than any of the other forms that show up in literature.

To clarify this, we have switched to “mathematical consistency” above old line 830, have changed “physical basis” to “physical meaning” in old line 830 and old line 875, and have changed from “physical basis” to “physical consistency” in the final paragraph of the conclusion. We hope this clarifies that we are not claiming the zero stress approximation has the strongest physical basis but that calculating stress as calc. F is consistent with continuum mechanics for an incompressible, isotropic fluid (which of course ice is not).

- 13. Supplement S7: I do not feel the authors gave an adequate description of the results. There is a focus on the velocity misfit at the ice front on Scar Inlet, but no speculation as

to why force balance appears to do better than the other two inversion results. Please elaborate on this point.

We have added a small final paragraph in the section discussing the results from Pine Island Glacier ice shelf.

14. Supplement Line 131: Please include contours on your plots of where force balance is applied versus the shear margin areas in which it is not. The same goes with the complete shear margin failure result in the other two calculations, described in lines 134-136.

We did not manually delineate shear margins but used flow direction buttressing rather than maximum principal direction buttressing. This gives the desired effect (removes shear margins) without manual delineation because the flow and maximum principal stress directions are similar in the center of flow but misaligned in shear margins. We therefore cannot easily include the suggested contours and have left these figures unchanged. However, to improve clarity we added the following sentence as second to last in the first paragraph of the force balance method section (S7):

“The flow direction and maximum principal directions are similar in the center of flow but diverge in shear margins.”

15. Speculation in lines 139-145: There are two pieces of this argument that I would like to question.
 - a. First, if I understand correctly, the stress increase in the unbroken ligament (at depth in the ice between crevasse tips) within force balance is not necessarily equal to increasing the “local” stress field one would measure at the ice *surface* with remote sensing products O(kms) away from the cracks. Another theory with a stress increase in the unbroken ligament is LEFM, as the stress at a crack tip would theoretically be infinite, and fall off with radial distance into the unbroken ligament. As such, please consider if you would make the same argument with respect to LEFM, which does indeed indicate that fractures modify the elastic component of the stress field.

We concur with this point and elaborate below. Stating it as “double counting” was too strong as we agree that the remote-sensing-measured strain rate is unlikely to be amplified to the strain rate in the ligament. On the other hand, if it is higher, because of the ligament’s increased strain rate and some surrounding region, then a “partial double counting” will occur which is our new verbiage.

- b. Second, let us suppose that there is indeed a damage feedback mechanism, where fractures influence the viscous flow of ice which in turn influence more fracturing, so on and so forth. In reality, we have observations of stress fields

some time after crevasse fields have formed, often with very large strain rates in these fractured areas (e.g. your figure S1b). Additionally, our simple crack depth theories all (zero stress, LEFM, force balance) assume either an initial unfractured state or that cracks don't modify the background stress field (zero stress). If this is the case, I would think that we don't have the correct data to validate our theories in this paper, where you'd want the time-dependent stress fields that lead to crevasse formation as in Surawy-Stepney and others, 2023. In sum, I think it is a slippery slope to suggest that one of these simple theories would be invalid due to a damage feedback mechanism apparent in the remote sensing data, as it invokes further questions about the existence of damage feedbacks and the well-posedness of the problem you are studying.

Again our full, conceptual response is below. Here we'll be clear that we did not intend to claim force balance (or one of the other two) is invalid. For calculating what crevasse sizes would form starting with unbroken ice, we are convinced that force balance is more appropriate than the zero stress approximation (though this matters significantly only when crevasses become large and is an aside to the purpose of this manuscript). Our findings do leave us with a potential problem: force balance applied in this workflow, which makes two temperature assumptions that would tend to decrease crevasse size (-2C for the full basal crevasse and -2C for calculation of buttressing number), predicts complete crevasse penetration and thus a large speedup of the front. We provide our theory for why this occurred.

To respond fully, we can consider two end members:

- 1) The presence of crevasses has no effect on the remote-sensing-measured strain rate: in this case, force balance would be most appropriate.
- 2) The presence of crevasses causes an increase in measured strain rate corresponding to the seracs between carrying no load: This is the assumption of damage as applied by Sun et al. (2017). In this case, even though the size of the crevasses is amplified by the effects force balance considers, those effects have fully modified the measured strain rate such that the zero stress approximation just yields the thickness of ice that would be in tension. "If ice were continuously load bearing here, it would be in tension so crevasse is predicted." This is consistent with the trial stress idea introduced in the manuscript following reviewer #2's comments.

We think reality falls between those end members meaning that (if ice has no strength were the perfect failure criterion), the zero stress would somewhat underpredict and force balance would somewhat overpredict when working from remote-sensing products in crevassed regions.

With this explanation, we have included recognition that the remote-sensing strain rate is likely to be increased due to including the elevated strain rate at and around crevasses in each "pixel." And called the effect a partial double

counting and added more verbiage clarifying this is our understanding of a possible explanation.

16. Typos

- a. Line 192: typo: Mode I is load.
- b. Line 196: typo: however, crevasse typically
- c. Line 827: typo: using calculating

All corrected. Thank you!

17. Grammar, rephrasing, and potential citations

- a. Line 35: I personally think of pinning points as another source of buttressing, but if there is literature that does not call it as such, it is fine to exclude from your definition of buttressing.

Our intent was to include pinning points within the definition of buttressing but we were not clear. Rewritten as:

OLD: Ice shelves restrain upstream ice flow via buttressing, backstress from shear load transmitted to embayment walls, or from compressive loading caused by pinning points (Fürst et al., 2016; Gudmundsson, 2013; Schoof, 2007).

NEW: Ice shelves restrain upstream ice flow via buttressing, backstress which comes from shear load transmitted to embayment walls or compressive loading caused by pinning points (Fürst et al., 2016; Gudmundsson, 2013; Schoof, 2007).

- b. Line 65: This sentence is grammatically correct but rather dense; you might consider rephrasing for clarity. I feel that it can be improved or removed.

Sentence removed.

- c. Line 309-310: Consider rewriting this sentence for grammatical correctness and clarity.

Rewritten.

OLD: This is incorrect when considering stress before crevasse formation but could potentially apply once crevasses have formed violating incompressibility.

NEW: Neglecting vertical strain rate is incorrect when considering stress prior to crevasse formation, but it could be argued that incompressibility no longer applies once crevasses exist.

- d. Lines 98-99: consider rewriting this sentence or turning it into two sentences.

Split into two sentences:

OLD: While the viscous flow of ice is driven by deviatoric, the component of the Cauchy stress that does not cause volume change during deformation, brittle failure is driven by the Cauchy stress itself.

NEW: The viscous flow of ice is driven by deviatoric stress, the component of the Cauchy stress that does not cause volume change during deformation. Brittle failure is driven by the Cauchy stress itself.

- e. Introductory paragraph with lines 50-60: while the final sentence is removed, I still do not consider this paragraph to have a conclusive end. Put overly simplistically, the paragraph could flow as: "A commonality ... is the importance of ... crevasses. A damage feedback, calving, cliff failure, hydrofracture, ..., all depend upon the modeling of crevasses (cite many papers). And yet, in the simplest theoretical cases, there remains disagreement upon one physical theory for predicting crevasse depths." This would tie it to the next paragraph.

We have added to the end of that paragraph:

NEW: With this need for crevasse depths in modeling these processes, researchers have proposed several physical theories for making crevasse depth predictions.

And replaced the sentence at the start of the next paragraph:

OLD: There are three primary methods for calculating crevasse depths from stress.

NEW: The three primary theories for crevasse depth predictions vary in their assumptions about ice's strength and the effect of a crevasse the surrounding stress field.

- f. Line 221: I believe there is reason to truncate this sentence, as it's difficult to quantify the magnitude of this error, particularly given that ice sheets/shelves are often modeled purely viscously.

While we agree quantifying this error would be challenging, we do not believe that to be grounds to neglect raising this violation that is guaranteed to occur when plane strain LEFM equations are used. We have added "in some regions" to allow the possibility that it's a small effect, as we (and to our knowledge, the field) do not know.

- g. Line 330-331: Please write this out in a more detailed manner. If this was the case, where the maximum or minimum principal strain rate was in the vertical

direction and not the horizontal plane, what would happen? E.g., when would this lead to horizontal plane fracturing?

- i. And second point here, please note that your maximum and minimum principal stresses are considering only the horizontal plane for the entirety of your study. Another place to say this could be lines 358-359.

In old 330-331 we updated to say that the vertical will “often” be the max or min, as it would be the min for longitudinal extension. We also added sentences explaining when vertical will be max or min and noted that in our study we always mean max and min of the surface terms.

NEW after old 330-331: If the surface terms compressive, the vertical stress will be the true maximum principal stress and horizontal plane fracturing would be predicted. When the surface terms are tensile, the vertical term will be compressive and would be the minimum principal deviatoric stress. Throughout this study, maximum and minimum principal deviatoric stresses, τ_1 and τ_2 will always refer to surface components. `

NEW after old 358-359: ...again considering only the surface terms.

- h. Line 409: include the details provided in the response to reviewers, such as second order central finite differencing without filtering, etc.

Updated noting 2nd order and no filtering.

- i. Line 95: Cite the paper from which you are using a damage model in ISSM in this sentence.

We have added citations of ISSM and an ISSM damage application here (Borstad et al., 2012; Larour et al., 2012).

- j. Line 803: Some modeling studies ... -> please cite directly which ones you are referring to.

Cited: (Choi et al., 2018; Pollard et al., 2015; Sun et al., 2017; Wilner et al., 2023)

- k. Supplement: various citations missing with Error! Resource not found.

Thank you. We have fixed these broken references.

References

1. Borstad, C. P., A. Khazendar, E. Larour, M. Morlighem, E. Rignot, M. P. Schodlok, and H. Seroussi (2012), A damage mechanics assessment of the Larsen B ice shelf prior to collapse: Toward a physically-based calving law, *Geophys. Res. Lett.*, 39, L18502, doi:[10.1029/2012GL053317](https://doi.org/10.1029/2012GL053317).
2. Surawy-Stepney, T., Hogg, A.E., Cornford, S.L. et al. Episodic dynamic change linked to damage on the Thwaites Glacier Ice Tongue. *Nat. Geosci.* 16, 37–43 (2023).
<https://doi.org/10.1038/s41561-022-01097-9>

References for reply:

- Borstad, C. P., Khazendar, A., Larour, E., Morlighem, M., Rignot, E., Schodlok, M. P., & Seroussi, H. (2012). A damage mechanics assessment of the Larsen B ice shelf prior to collapse: Toward a physically-based calving law. *Geophysical Research Letters*, 39(18). <https://doi.org/10.1029/2012GL053317>
- Choi, Y., Morlighem, M., Wood, M., & Bondzio, J. H. (2018). Comparison of four calving laws to model Greenland outlet glaciers. *The Cryosphere*, 12(12), 3735–3746. <https://doi.org/10.5194/tc-12-3735-2018>
- Humbert, A. (2010). The temperature regime of Fimbulisen, Antarctica. *Annals of Glaciology*, 51(55), 56–64. <https://doi.org/10.3189/172756410791392673>
- Jiménez, S., & Duddu, R. (2018). On the evaluation of the stress intensity factor in calving models using linear elastic fracture mechanics. *Journal of Glaciology*, 64(247), 759–770. <https://doi.org/10.1017/jog.2018.64>
- Larour, E., Seroussi, H., Morlighem, M., & Rignot, E. (2012). Continental scale, high order, high spatial resolution, ice sheet modeling using the Ice Sheet System Model (ISSM). *Journal of Geophysical Research: Earth Surface*, 117(F1). <https://doi.org/10.1029/2011JF002140>
- MacAyeal, D. R. (1989). Large-scale ice flow over a viscous basal sediment: Theory and application to ice stream B, Antarctica. *Journal of Geophysical Research: Solid Earth*, 94(B4), 4071–4087. <https://doi.org/10.1029/JB094iB04p04071>
- Pollard, D., DeConto, R. M., & Alley, R. B. (2015). Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure. *Earth and Planetary Science Letters*, 412, 112–121. <https://doi.org/10.1016/j.epsl.2014.12.035>
- Roger Buck, W. (2023). The role of fresh water in driving ice shelf crevassing, rifting and calving. *Earth and Planetary Science Letters*, 624, 118444. <https://doi.org/10.1016/j.epsl.2023.118444>

Sun, S., Cornford, S. L., Moore, J. C., Gladstone, R., & Zhao, L. (2017). Ice shelf fracture parameterization in an ice sheet model. *The Cryosphere*, 11(6), 2543–2554.

<https://doi.org/10.5194/tc-11-2543-2017>

Wang, Y., Zhao, C., Gladstone, R., Galton-Fenzi, B., & Warner, R. (2022). Thermal structure of the Amery Ice Shelf from borehole observations and simulations. *The Cryosphere*, 16(4), 1221–1245. <https://doi.org/10.5194/tc-16-1221-2022>

Wilner, J. A., Morlighem, M., & Cheng, G. (2023). Evaluation of four calving laws for Antarctic ice shelves. *The Cryosphere*, 17(11), 4889–4901. <https://doi.org/10.5194/tc-17-4889-2023>

We again thank Dr. Duddu for his help in being as clear and precise with fracture definitions as possible. Our replies are in-line below.

I commend the authors for their comprehensive responses to both reviewer questions. Except for one minor comment / correction below about fracture modes that needs revision, the article can be accepted for publication.

Comment:

In response to reviewer 1, comment 1, the authors modified the discussion of mode I, II and III fracture. A key difference between mode II and mode III is not just it is simply in-plane and out-of-plane shear but also how the crack opens, in mode II the crack opens through sliding whereas in mode III the crack opens through tearing. One can have mode II (crack sliding) fracture in-plane or out-of-plane, after all the coordinate directions are simply constructs and depending on whether we consider a 2D flow-line or 2D shallow shelf, the out-of-plane direction is different. In shear zones of ice shelves, the 45 degree in-plane crack is mode I dominated (i.e. crack opening is along the maximum tensile stress direction) but can have a mix of mode I and mode II, where the mode II crack will be parallel to the flow line. In contrast, mode III requires forces out of the ice shelf plane in the vertical direction, for example, non-uniform ocean swell could cause tearing at the tip of a rift or tearing could also occur as shear margins due to buoyancy forces. As such a mode III crack will still be perpendicular to the flow lines as the tearing will occur out-of-plane of the ice shelf. Mode III cannot occur in-plane of the ice shelf because the ice shelf length and width are much larger than the ice shelf thickness.

Based on this understanding, I feel that the discussion on Lines 174 to 180 must be revised as follows:

-- "Mode I involves opening of the crevasse walls wide apart, Mode II involves sliding of the crevasse walls, as in a strike-slip fault, and Mode III involves tearing, for example, due to the rising of the surface on one side of the crevasse while the other side's surface lowers, and (van der Veen, 1998a)."

-- "This tendency holds in shear margins, where crevasses form approximately 45-degrees

from flow as Mode I crevasses, whereas Mode II fractures would strike parallel to the flow direction."

Thank you for this important correction. We have taken the suggestion verbatim but have changed the citation to van der veen (1999) which provides an explanation in terms more like this verbiage.

Also, another minor correction is that authors say plain strain multiple times in their response, instead of plane strain. In the manuscript it is correctly typed as plane strain.

Thank you for the double check on this, we have double checked that the usage is correct and consistent in the manuscript (and principle vs principal).