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.

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-2024-2424.pdf](https://egusphere.copernicus.org/preprints/2024/egusphere-2024-2424/egusphere-2024-2424.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.
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.
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 tau_yy is approximately 0 - is roughly upheld in various parts if tau_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.
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 \tau_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.
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.

    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.

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.
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.
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.
9. Figure 4:
    a. Make a statement somewhere about why A isn't included, even if you find it obvious.
    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.
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,
    a. why the bulk temperature can have such a large effect, and
    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).
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.
   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.
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.
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.
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.
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.
    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.

16. Typos
    a. Line 192: typo: Mode I is load.
    b. Line 196: typo: however, crevasse typically
    c. Line 827: typo: using calculating
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.
    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.
    c. Line 309-310: Consider rewriting this sentence for grammatical correctness and clarity.
    d. Lines 98-99: consider rewriting this sentence or turning it into two sentences.
    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.
    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.
    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.
    h. Line 409: include the details provided in the response to reviewers, such as second order central finite differencing without filtering, etc.
    i. Line 95: Cite the paper from which you are using a damage model in ISSM in this sentence.
    j. Line 803: Some modeling studies … -> please cite directly which ones you are referring to.
    k. Supplement: various citations missing with Error! Resource not found.

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.
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