

Reviewer #1

This work presents a study of the dynamics of an ice melange within the framework of a granular material in the quasi-static regime. The problem is simplified by a double averaging in depth and width. This makes it possible to study the ice melange dynamics in the streamwise direction by leveraging a non local granular rheology. Particularly, the authors explored the sensitivity of the model to physical and geometrical parameters to understand the role of the ice melange on the buttressing force. Here, as I understand it, a significant advantage of implementing the granular fluidity model is that it allows for a more accurate estimation of the ice melange thickness at the calving front. Consequently, this leads to a more precise calculation of the buttressing force. Then, the authors investigated two cases: a steady state where the calving flux equals the flux of icebergs at the outlet of the ice melange and a quasi static state where there is no calving and no flux at the end of the ice melange. In the quasi-static case, the velocity is found to be constant along the fjord and the thickness profile is supposed to be exponential along the fjord. In the steady state, the buttressing force depends on surface and basal melting rate, decreases with fjord width and increases with calving flux. For the latter, it is found that it is because the buttressing force, which depends on glacier thickness, increases more rapidly than the force needed to capsize icebergs. It is therefore suggested that glaciers with small calving fluxes would be less influenced by buttressing than the glacier with high calving fluxes.

I would like to thank the authors for this interesting paper. Indeed, I truly appreciate the combination of granular physics and glacier modeling, which I believe offers a more nuanced consideration of the mechanisms within the ice melange. This approach could significantly enhance our understanding of the role of ice melange in calving dynamics, as well as the velocity at which icebergs are released from the fjords. The simplified model represents a crucial initial step toward achieving these objectives, and I would encourage the authors to further develop their modeling in the future. Specifically, I suggest exploring the development of a multi-phase flow model to facilitate the incorporation of the effects of seawater and wind while considering the full 3D granular velocity field.

I found the modeling part of the paper to be interesting, well-written, and comprehensive. Therefore, I have only a few minor comments to offer. However, I have more suggestions regarding the sensitivity analysis, which I found to be occasionally briefly discussed or explored. This is understandable given the wide range of scenarios you decided to explore. It seems one must choose between thoroughly exploring all sensitivities of the model or focusing on a specific aspect for a more detailed treatment. Due to the extensive range of parameters you are exploring, I found it sometimes difficult to follow. This is also because figures you are discussing are not mentioned clearly. Therefore my comments are essentially linked to the clarity of the different messages.

We would like to thank the reviewer for their encouraging comments and thorough and thoughtful review. We also agree with the reviewer regarding future model developments.

1 Comments on the model

1. Line 60, you define $\sigma_{ij} = R_{ij} - \tilde{p}\delta_{ij}$ with the pressure positive in extension. In Cuffey and Paterson's book, it is rather $R_{ij} = \sigma_{ij} - \tilde{p}\delta_{ij}$ with the same sign convention. Is it a mistake or do you define the overburden pressure differently? Make sure that it is correct and that it doesn't change expressions. In particular, with the formulation in line 60, I cannot recover equation (5).

Thank you for this comment. I think the confusion here is related to whether we are discussing resistive (tectonic) stresses or deviatoric stresses. With regards to resistive stresses, our definition is consistent with both Cuffey and Paterson (equations 8.48) and van der Veen (equations 3.14 and 3.15). If this was not the case then we would not arrive at the same vertically integrated stress balance equations. However, you are correct that there was a negative sign error in equation 5. In troubleshooting, we discovered another negative sign error that essentially canceled out the error in equation 5. Following the common convention in glaciology, we had defined the isometric pressure as $P = \text{tr}(\sigma_{ij})/3$. However, the granular mechanics literature, from which our rheology comes, defines $P = -\text{tr}(\sigma_{ij})/3$. This means that our definition of pressure was inconsistent with the granular rheology. Instead of modifying the rheology, we now adopt the latter definition of pressure and corrected the negative sign error in equation 5. The upshot is that the model equations remain unchanged; there were just some minor changes to the text to correct the derivation.

In addition, we added a couple of equations (see equations 9-11) to better describe how we arrived at the final rheology.

2. How you recover equation (2) from equation (1) is not entirely clear to me. Is it by integrating the equation line 66 with the definition of the gravity of equation (1)? In this case, the statement line 71 should be modified. Then, how do you integrate it? This is related to another comment: I have the feeling that a scheme of the problem considered with characteristic parameters could help the reader to figure out the configuration more directly. For example, line 154 to 158 it could help to visualize the boundary conditions etc...This is just a suggestion.

First, there was a typo in equation 1 that has now been fixed. The integration is then pretty straightforward, since $dp/dz = -\rho \cdot g_{\text{eff}}$ and the granular pressure is 0 at the top and bottom of the ice melange. We have added a sentence to clarify this.

We also added a schematic and couple of paragraphs to the beginning of Section 2 to help orient the reader, as also suggested by the other reviewer.

3. Equation (22) is not obvious to me. Could you please cite or explain from where it comes from?

Equation (22) (now equation 23) arises because the stress has to vary linearly across the fjord if there are no longitudinal deviatoric stresses (which is the case for shear-dominated flow). We added a sentence that described how to arrive at this equation and refer the reader to Amundson and Burton (2018), where we also derived the expression. Note that this is similar to the 1D shallow shelf approximation, in which the shear stress also varies linearly across the glacier.

4. From what I understand, μ_w is here an effective friction coefficient and not the usual threshold for movement, which depends on the properties of the material in contact. Is it correct? Then, it is not very clear how you calculate μ_w . From my understanding, you calculate the granular fluidity g_y to solve the averaged transverse velocity field of equation (24) and obtained by averaging equation (23) over the width. But how do you find μ_w since it is not a constant parameter?

That is correct: μ_w is the effective coefficient of friction along the fjord walls and not a classic coefficient of friction like you might encounter in introductory physics. It is the same coefficient that appears in the granular rheology, just evaluated along the fjord walls. We solve for μ_w as part of our minimization procedure. At the beginning of Section 2 and also in Section 2.3 we discuss that we simultaneously solve for five variables (velocity, granular fluidity, μ_w , thickness, and length). Most of the equations that we are solving contain two or more of these variables. Using updated equation numbers, Equation (25) contains the velocity and μ_w , equation (20) contains the velocity, granular fluidity, and thickness, ... So essentially we have a system of coupled equations that we have to solve simultaneously. Note that this is also discussed at the top of Appendix C.

2 Comments on the analysis

5. Line 231 you stated $dL/dt = 0$ with a full derivative. What is the reason? What are the parameters that affects L ? Is it only the ice thickness?

The length only varies with time (as a result of the evolution of the ice mélange thickness profile), so it doesn't make sense to write it as a partial derivative.

6. Line 238 to 246 I really like the discussion but you should may be refer the reader to the different subfigures you are analysing. It would make the analysis straightforward to understand. Same for line 262, you should cite figure 2b.

We considered adding references to each of the panels in Figure 1, but decided that it would be too cumbersome since we would have to refer to a different panel after every half-sentence or so. However, we did add two parenthetical remarks to indicate that the solid lines refer to the steady-state limit and dashed lines to the quasi-static limit.

Regarding line 262: We have added references to the various figure panels of Figure 2 throughout this section.

7. Line 244, the “roughly exponential thickness” for the quasi-static limit is not obvious to me. Why is it so different from the steady state?

We have tried to clarify this by pointing out the profile is different in the quasi-static limit because shear stresses are lower (which leads to thinning) and there is no melting (which allows the ice mélange to lengthen).

8. In the sensitivity analysis I have the feeling that you could sometimes discuss more the behavior of the ice mélange in light of the physical model. It could allow one to better identify the processes at play. For example, Line 267, you say: “Increasing b makes the ice mélange more stiff and extensive”. First I am not sure to understand what you intend by “stiff and extensive”. Then, you could maybe discuss a bit more why is that so? Is it because b diminishes the local source term in the diffusion equation of the granular fluidity (equation 14), therefore there is less capacity to diffuse fluidity in the mélange?

Thank you for this suggestion. We have now done this for each of the four parameters. Essentially, the parameters affect the local granular fluidity, which is the fluidity in the absence of flow gradients (b , μ_s), the cooperativity length, which essentially describes a stress coupling length (A), or both the local granular fluidity and the cooperativity length (d).

9. Line 277, it is mentioned that ice mélange geometries and velocity profiles appear to be roughly consistent with field observations. In figures 2a to 2e, would it be of interest to add a profile from field observations allowing for a rapid comparison? I agree that it is not easy to determine the best calibration as there are different possible combinations of values for the parameters to recover field observations. It would require a deeper analysis that seems to go beyond the purpose of this work.

We agree that this is a good point, but instead of trying to plot field observations on top of our model results we instead refer the reader to data that is presented in Amundson and Burton (2018), Bevan et al. (2019), and Xie et al. (2019). Simply plotting existing data on top of our model results is not straightforward because we would have to adjust the model geometry and forcings to produce profiles that have correct length and thickness. The paper is already very involved and we are hesitant to make it even more complex; we simply want to indicate that the modeled profiles are reasonable.

10. I found section 3.1.2 Sensitivity to external forcings and fjord geometry difficult to follow. In

line 284 you say that you will investigate the impact of calving fluxes on ice mélange flow and geometry, but then, in line 288 it seems that you do the contrary: you investigate how ice mélange plays a role on calving fluxes and start to study the buttressing. Therefore you never discussed clearly your results in light of the different panels of figure 3. Also, I found the discussion from line 285 to 296 sometimes difficult to understand. Here are the main reasons:

This section explores both the impacts of external forcings on ice mélange flow and geometry as well as the feedbacks on calving. We see now that this wasn't clearly articulated. We have renamed the section "Sensitivity of ice mélange flow, geometry, and buttressing force to external forcings and fjord geometry" and revised the first sentence in the section accordingly. We also added some language throughout this section to improve the transitions between paragraphs.

- line 286: "The ice mélange becomes more extensive as the fluxes increase (Figure 3), implying that ice mélange produced by highly active glaciers is more likely to exert high resistive stresses against the glacier termini and to persist year round." By extensive you mean longer or thicker? From my understanding of equation (28) only ice mélange thickness plays a role. But on the contrary, to me the word "extensive" suggests longer but not thicker.

We mean longer and thicker, and have revised the wording to indicate so. They are connected, since a longer ice mélange will have more resistance to flow from side drag, leading to thicker ice mélange.

- line 292, it is not clear how you find these percentages? It increases compared to what?

The percentages refer to a comparison of the small glacier and large glacier scenarios. This paragraph has been revised accordingly.

- The issue linked with icebergs that capsize is not very clear. Is it because once capsized, an iceberg generates thinner ice mélange?

This has been revised for clarity. Basically the idea is that one way to estimate the force required to prevent large-scale calving events is by calculating the buttressing force that would be required to prevent a large iceberg from fully separating away from the glacier terminus (not just fracturing, but also capsizing and pushing off of the glacier).

I would suggest to rewrite this section more clearly to highlight the messages and be better articulated with the study of the buttressing in section 3.2 and 3.3. I have the feeling that it would make the end of the paper more straightforward to follow.

See comment above about revising the language in this section to better emphasize that we are also considering the impacts on ice mélange buttressing.

11. In the conclusion, line 361, you say that the NSMA produces realistic thickness and velocity profiles. I do not doubt about it but you never showed profiles from the field for a qualitative comparison. This is related to comment 9.

We have changed this to read that the profiles are roughly consistent with observations, and cite Amundson and Burton (2018), Bevan et al. (2019), and Xie et al. (2019).

3 Minor comments

1. I really like that you mention the estimated value for the inertial number. I do not know if this number is largely known in the glaciological community. May be you could define it rapidly and cite work like Gdr Midi 2004?

In the first paragraph of Section 2.1 we now define the inertial number and reference GDR Midi.

2. Line 253, would it be clearer to say “explore how adjusting these parameters affects the steady-state”?

Changed to “... affects the steady-state model behavior”.

3. Line 262, please cite the figure that allows you to say that “the size of iceberg allows the ice mélange to thin and advance”. This will be easier to follow the argument.

Done.

4. You should may be identify the different cases (small glacier, medium glacier and large glacier) in the caption of figure 3. I suppose the first case is for the small glacier, then the medium glacier and lastly the large glacier, but it would make the identification straightforward.

Done.

5 Suggestion: in the appendix, you could may be provide a scheme for the successive steps of your algorithm?

Although we appreciate this comment in general, it doesn't really make sense for our model because it would just be a single loop. As stated in Section 2.3, we simultaneously solve the

model equations using a fully implicit time step. We have also now reiterated this point in the first sentence of Appendix C.