# Response to Reviewer 1

**Blue font:** comments from Reviewer 1
**Black bold font:** Authors' responses to the Reviewer's comments

**Note 1:** The Reviewers recommended major revisions, including restructuring the text and redoing certain measurements. The paper is almost entirely rewritten and highlighting these changes would significantly hinder the manuscript's readability. Therefore, the revised version does not highlight the corrections introduced.

**Note 2:** The line numbers provided here correspond to the revised version of the manuscript.

GENERAL COMMENT:
The preprint "Importance of ice elasticity in simulating tide-induced grounding line variations along prograde bed slopes" by Maslennikova et al. investigates the impact of viscoelastic processes to establish relations between grounding zone width and ice speed, ice thickness, and bed slope. The authors use a combination of SAR satellite data and a numerical model in their work.
This work presents significant and novel knowledge, building on recent efforts to better understand the impact of elasticity on processes occurring at the grounding zone. I have very few comments regarding the content and methodology of this paper, which I think is of high quality and surely required a lot of work. However, I have several concerns about how methods, findings are presented and discussed, which, in my opinion, need further work before this preprint can be published. Overall, my biggest concern is that this paper lacks a robust discussion regarding the physics of what is modeled, which is briefly mentioned in the conclusion section. There is also some discrepancy in the various sections, where methodology is provided in the results section and a proper discussion is only included in the conclusion section. I think a rearrangement of these sections would really be beneficial to this preprint.
I've made generic and specific comments below to this point, which, in my opinion, would further strengthen the manuscript.

**We thank the Reviewer for the valuable feedback. We believe the implementation of the Reviewer's comments has significantly improved the quality of our manuscript. Below, we provide a line-by-line breakdown of the Reviewer's comments, and a description of the corrections made in response to them.**

## INTRODUCTION

I find that the introduction contains a lot of information, but at times, it is unclear how this benefits the paper's goals.

**We have rewritten the introduction focusing on how the provided information benefits the paper's goal.**

The authors include many references after each statement, which makes the reading experience slow and confusing. I recommend citing only papers that are directly related to the context. Furthermore, a researcher outside of the peer review team even commented that a couple of citations (Dempsey et al. and Beldon & Mitchell) are completely off-topic. I looked at these papers and agree that they are not relevant to this work, but please correct us if we are wrong. Lines 37-38 cite 16 references (!), and I am not sure how some of these are relevant to the sentence they are attached to.
**Agreed, we acknowledge the Reviewer's concern that some references may appear excessive. As a result, we have revised the reference list:**

- **Line 31-31: (Dempsey et al. and Beldon & Mitchell) citations were removed**
- **Line 34: 16 references were revised and three of the most relevant citations were left to give the reader an idea about the most recent advances in the field:**
  (Cornford et al., 2020; ~~Durand et al., 2009b, a; Favier et al., 2014;~~ Gagliardini et al., 2016; ~~Gudmundsson et al., 2012; MacAyeal, 1989; Muszynski and Birchfield, 1987; Pattyn et al., 2012; Pegler et al., 2013; Pegler and Worster, 2013; Robison et al., 2010; Schoof, 2007b~~; Seroussi et al., 2014; ~~Thomas and Bentley, 1978; Weertman, 1974~~)
- **As the Reviewer suggested, now we cite only the references directly related to the context and ensure every statement has no more than 3 references (except lines 37-38 where all 5 references are necessary to provide a comprehensive literature review):**
  - **Line 25:** (Friedl et al., 2020; Haseloff and Sergienko, 2018; ~~Schoof, 2007a~~)

- **Line 27:** (~~Davis et al., 2023;~~ Davison et al., 2023; Holland, 2008; ~~Marsh et al., 2016~~)
- **Line 29:** (Goldstein et al., 1993; Schoof, 2007; ~~Friedl et al., 2020~~)
- **Lines 31-32:** (Albrecht et al., 2006; Coleman et al., 2002; ~~Freer et al., 2023; Warburton et al., 2020; Dempsey et al., 2021; Beldon and Mitchell, 2010~~)
- **Line 34:** (Cornford et al., 2020; ~~Durand et al., 2009b, a; Favier et al., 2014;~~ Gagliardini et al., 2016; ~~Gudmundsson et al., 2012; MacAyeal, 1989; Muszynski and Birchfield, 1987; Pattyn et al., 2012; Pegler et al., 2013; Pegler and Worster, 2013; Robison et al., 2010; Schoof, 2007b~~; Seroussi et al., 2014; ~~Thomas and Bentley, 1978; Weertman, 1974~~)

## DATA and METHODS

I am not an expert in double differences to examine grounding zone width, but this section leaves me wondering how you can estimate the minimum and maximum grounding zone extension within a tidal cycle if you have only one day of repetitive acquisitions. If you have 24hours of difference between acquisition, wouldn't the tide level being approximately the same. I may have completely misunderstood this section, and if so, I sincerely apologize. However, I still think that even someone without a background in differential interferometry (like me) should be able to quickly understand the physical processes by reading the methodology of this paper. I am aware that substantial past work has used 1-day repetitive acquisitions to study grounding zone migration, and I know that access to sub-daily SAR images is challenging. I am not doubting the quality of this approach; I would simply appreciate a bit more background on this.

**We agree with the reviewer. By using different orbits (ascending and descending) we can acquire data at different times of the day and increase the sensitivity to the tidal spectrum, particularly to the 24-hour tidal cycles. Calculating interferograms from orbits acquired at different times allows us to capture a broader range of tidal frequencies. This statement has been added to the updated version of the manuscript (lines 149-150).**

## VISCOUS AND VISCOELASTIC MODELS

I appreciate the background information in the modeling perspective, but I am unsure what Equations 1-5 and 9 really contribute to this paper. These are mostly large simplifications of any dynamic system subjected to boundary conditions, and Equation 9 represents a simple tolerance criterion. While I understand that this may be a matter of personal preference, I would consider removing them or perhaps substituting them with the actual PDEs that are resolved (viscous vs. viscoelastic, i.e., Appendix A, eq A1-A3 and A6), which would enrich the reading experience with theoretical background. Equations 1-5 and 9 could go in Appendix A.2.3, where the authors describe boundary conditions. On the other hand, Equations 6-8 are important and useful to the reader. Again, I want to stress that this may be a personal preference, but I do think that Equations 1-5 and 9 can be easily summarized in the text.

**We greatly appreciate this comment, which closely aligns with the opinion of Reviewer 2. To address the concerns of both reviewers, we have revised the 'Viscous and Viscoelastic Models' section (lines 193-365) and moved the model formulation from the Appendix to the main text.**

**We have not changed the contents of Table 1 from the original manuscript, as we believe it provides a concise summary of the model formulation. Additionally, we find it useful to retain this table because it informs the reader about the computational time required for both models. However, after relocating the model formulation to the main text, we felt it was unnecessary to keep the table in the main text. Therefore, Table 1 has been moved to the Supplementary Materials (now Table S4 in the revised manuscript).**

## RESULTS

This section is really hard to read. There are a lot of numbers, which makes it confusing. I also find hard to distinguish whether model results, model set-up and observational data are discussed. Would dividing this section (data, model) into two subsections help? Finally, authors discuss the simulation set-up in this section, although it should rather go in the methodology section. Results section should only present the outcome of the simulations and not details about simulations themselves.

**We have significantly rewritten the content of this section. We wrote new subsections including measured glacier's parameters, modelled glacier's parameters (and separate subsections addressing the role of thickness, velocity, grounding zone widths and bed slope) and the evaluation of model performance.**

## DISCUSSION

This section is also quite challenging to follow. In my opinion, this looks more of a results section rather than discussion.

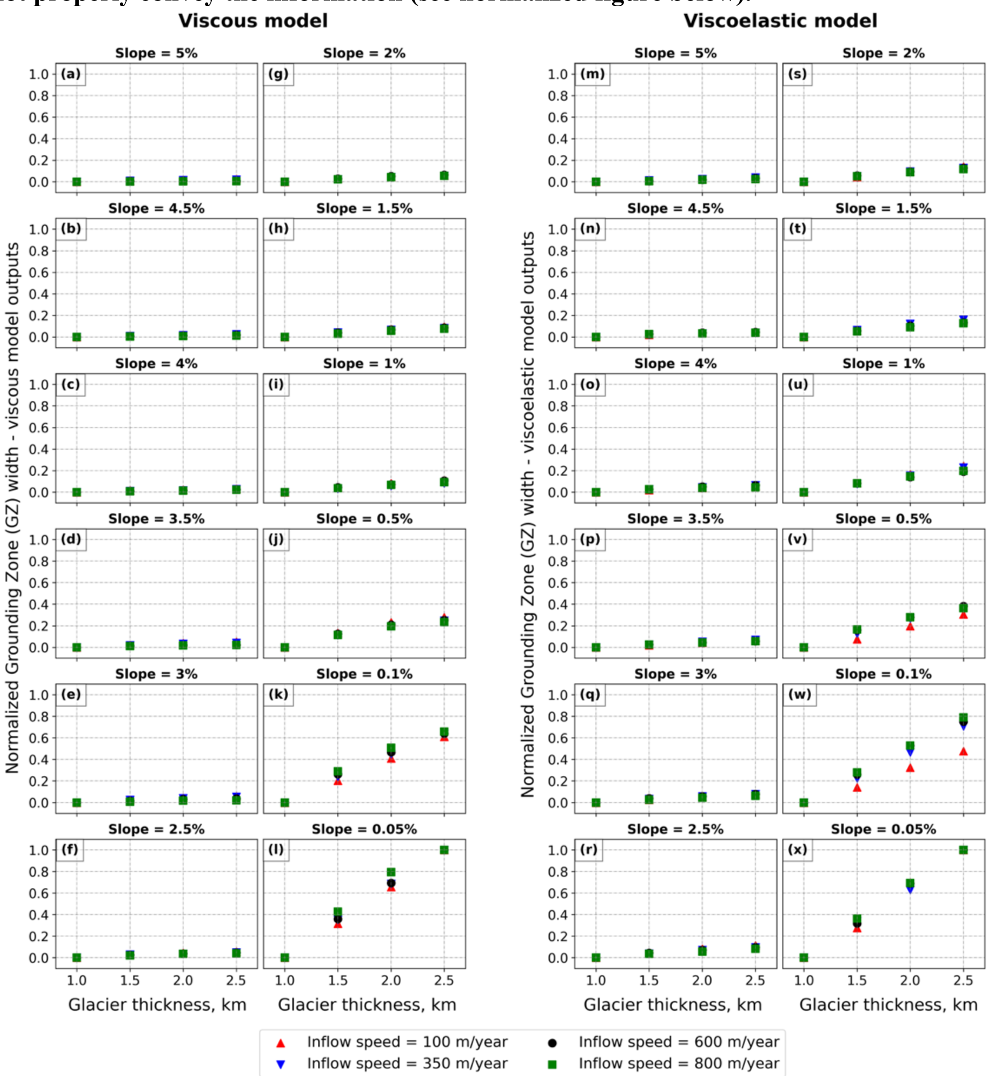
**We have significantly revised the contents of this section.**

I am completely lost in paragraph 272-286, and it took a while to actually understand the series of inequalities that are reported here. These are much more easily visualized in figure 3, and I am not sure if verbatim reporting them is any beneficial.

**Agreed, we removed the set of inequalities in the updated version of the manuscript.**

I think that figure 3 is perhaps the most important, but it is also hard to read, since the y-axis changes for each sub-plot. The reader cannot compare results from viscous and visco-elastic model if the y axis is different. Would a normalized GZ width (0-1) improve things? After doing so, it should be easier to merge panels a-l with panels m-x and improve the figure.

**We tried to normalize the results in Figure 5 (formerly Figure 3) as the reviewer suggested, but we realized the figure did not properly convey the information (see normalized figure below).**




**Normalized Figure 5 (former Figure 3).** The grounding zone values generated by the viscous model (subplots (a) – (l)) were normalized using the viscous model's outputs for a 0.05% bed slope, while the grounding zone values generated by the viscoelastic model (subplots (m) – (x)) were normalized using the viscoelastic model's outputs for a 0.05% bed slope.

**Both models show that for bed slopes between 1% and 5%, glacier thickening affects the grounding zone by less than 1 km, while significant impacts are observed at 0.05% and 0.1% slopes (see Table S3). Based on this, we decided to exclude most of the slopes and retain only 0.05%, 0.1%, 0.5%, and 5% slopes as the most impactful (see Figure 5, line 430). This way, Figure 5 now shows that: 1) the viscous model predicts grounding**

**zones twice as large as those of the viscoelastic model; 2) the evolution of grounding zone width versus glacier thickness is dependent on bed slope; 3) the grounding zone widens with decreasing bed slope. Additionally, the marker shapes were changed to make the figure colorblind-friendly.**
**We also provide the results for all the slopes in the supplementary material (Figure S5).**

## CONCLUSIONS

There is a lot of information presented here for the first time rather than in the previous sections. The conclusion section should only wrap up the work and draw final arguments.
**We have revised the Conclusions section to only include the summary of the work done.**

As far as I can see, paragraphs 414-421 and 451-460 provide the only physical explanation of what is presented in the paper. I think this work needs a bit more discussion about the physics behind the modeled processes. Why is elasticity so important? It improves model-data agreement, but why? What is the physical reason? I may have missed something, and I apologize if I did, but the only explanation I could find in the text is: "Therefore, an element responsible for rapid deformations, or an elastic component, becomes necessary." To strengthen this paper, I would recommend a thorough discussion regarding the physics of elasticity applied to grounding zone migration. It looks like the model used is based on a previous publication (Stubblefield et al., 2021), so technically, this is not a presentation of a new model. If this is the case, I would appreciate more background on the physical explanation of why this modeling effort is conducted. Furthermore, these considerations should go into a discussion section rather than the conclusion itself.
**Agreed, in the results section we have now included a section titled 'Role of Elasticity'. While viscous models may be adequate for modeling long-term ice sheet evolution, they fail to represent important short-term phenomena such as tidal motion, seasonal cycles, and calving processes, which require the consideration of elastic responses. We also support our argument with existing cryosphere literature discussing this aspect.**

## SPECIFIC COMMENTS

Line 26: Davis et al 2023, how is this recent paper related to the sentence? Davis et al 2023 does not investigate glacier stability. Also, what does 'salient' mean here? This sentence and pretty much all of the following cite a lot of papers (line 31, 35, 38), which makes the reading experience very slow and confusing at times,
**We removed this reference and revised the reference list.**

Line 48: Gadi et al 2023. This paper investigates ice shelf melting using a numerical model. How is this paper related to "quantification of grounding zone width"?
**We removed this reference and revised the reference list.**

Line 52, Chen 2023, Chen 2023a and Chen 2023b are the same paper. Please consolidate.
**Corrected, thank you.**

Line 63: Please remove parenthesis when you are using a reference as a noun.
**Corrected, thank you.**

Line 162: Really nice figure.
**Thank you. In response to Reviewer 2's suggestions, we revised the original figure and split it into Figures 1, 2, and 3.**

Line 228, 236: This information is not a result but rather an explanation of the simulation setup.
**This information was moved to the newly created 'Model setup' section (lines 296-365).**

Line 260: These are results. I think the logical structure of this paper needs to be revised.
**The logic of the paper was significantly revised and the information the Reviewer is referring to was mode to the 'Results' section.**

Line 275: This and the following lists of inequalities are hard to follow, but easily visualized in figure 3. Is it really necessary to write them down here?
**As mentioned above, the inequalities were removed**

Line 288: Figure 3. I think this is an important plot, but the different limits on the y-axis make it hard to compare between simulations. Would using a normalized grounding zone scale help? Additionally, consider adding this plot in the supplement. Also, please use a color scheme that is colorblind-friendly or, alternatively, different marker shapes.
**Considering the importance of the figure the Reviewer is mentioning here, we did not move the figure to the Supplementary. However, following the Reviewer's suggestions, we significantly revised the figure.**

Line 313: I have a philosophical issue with the term 'validation.' I do not think that you can validate a numerical model; you can at best evaluate how well it agrees with observations. If this model works well in the area of interest, how can you be sure that it is 'valid' for other regions as well?
**Agreed, Section name 'Model validation with DInSAR grounding zone measurements' was changed to 'Evaluation of model performance using DInSAR grounding zone measurements'.**

Line 397: this reads more as a discussion rather than a conclusion.
**This section was revised and a significant part of it was moved to the 'Discussion' section.**

Line 451: This paragraph is the only part of the paper that is an actual physical discussion on the importance of elasticity. I assume that the model used here was already presented in another paper. This work is therefore an extension and an important application of an existing model. In the discussion section, I was expecting a thorough discussion on the theoretical meaning and implications of including elasticity in modeling of grounding zone dynamics, which, alas, is missing. I do not think that the discussion needs to be completely re-written, but some further physical explanation of what is novel here and the overall importance of these findings would really strengthen this manuscript.
**As noted above, we expanded our discussion on the elasticity importance (lines 513-546) and mentioned the physical explanation of elasticity importance in the Conclusions (lines 548-566).**

**We thank the Reviewer for the valuable feedback and look forward for receiving further comments.**