

Letter to the Editor and Response to the reviewers

November 27, 2020

Dear Editor,

Our manuscript with the title ‘Sensitivity of ice sheet surface velocity and elevation to variations in basal friction and topography in the Full Stokes and Shallow Shelf Approximation frameworks using adjoint equations’ submitted to The Cryosphere has been revised following the all suggestions and comments by the referees. In particular, the abstract has been changed and the Introduction has been expanded somewhat.

The second referee suggests that the title of the paper should be changed. We would prefer to keep the present title since we consider the sensitivity results as the most interesting ones to the glaciological community. The time-dependent adjoint equations are a tool to derive these results. If the Editor prefers the title proposed by the second referee, we are willing to change it.

We hope that after these revisions the paper can be accepted for publication. We wish to thank the referees for their work and their detailed reading of the manuscript which helped us improve the presentation.

Best regards,
Cheng Gong, Nina Kirchner, Per Lötstedt

Report 1

Thank you for addressing all my comments. The only remaining item that needs a bit more attention is question 19. Thank you for explaining the sources of uncertainties. However, not sure what the point of the authors is if these uncertainties and their affect on the solution(s) are not formally quantified.

Response: Eq (42) gives the relation how the uncertainty is propagated from the parameters b and C to the solution u , or mathematically speaking δu is a function of δb and δC . As long as this relation is known, it is straightforward to quantify the effect of parameter uncertainties to the solution. An example is added in the discussion after (42).

Report 2

I still feel the authors are presenting number of results as new, that really are not. Let me put this in a positive way. By repeating well-know aspect of glacier flow in, for example, the last three sentences of the abstract, they are underselling their work by suggesting they merely rediscovered old facts rather than stressing the methodological advances that this work. What they do provide is a general numerical framework for studying the relationship between bed and surface properties.

Response: We have changed the Abstract as suggested by the reviewer by stressing the new framework and the time dependent analysis: “*Here, we present a general numerical framework for studying the relationship between bed and surface properties of ice sheets and glacier. Specifically, we use an inverse modeling approach and the associated time-dependent adjoint equations, derived in the framework of a Full Stokes model and a Shallow Shelf/Shelfy Stream Approximation model, respectively, to determine the sensitivity of ice sheet surface velocities and elevation to time-dependent perturbations in basal friction and basal topography.*” One sentence about the damping of high frequency perturbations has been removed.

The emphasis on the finite-time it takes for a perturbation to be expressed at the surface appears misplaced to me. There is a direct and instantaneous effect. This of course is well known and does not need to be stressed.

Response: This may be well-known from the physical perspective, but not completely clear mathematically or numerically. Both effects are present. A change in C in time is directly visible in u at the surface (Eq (22), Fig. 3, Fig. 9a) and there is a delay in time between the change in C or b and the observation of it in h at the surface (Eq (23), Fig. 3, Figs. 9c,d). The time delay is well illustrated in particular in Fig. 3. This is mentioned in Conclusions. We believe that the physical process is well represented by the numerical model, which confirms what is expected from the physical perspective.

What the manuscript is missing is an introduction over previous work. Either such section must be included, or the authors change the focus of the work to the methodological advances they make. This focus of the current introduction on global warming and RCPs’ etc, is too general and does not really introduce the subject of the paper.

Response: We concluded from the initial review that no major changes were requested to our Introduction, but have now added a few sentences to the part of the Introduction focusing on the inverse problem where we discuss other sensitivity studies briefly and also in the end. However, most of the comparison to other sensitivity results is contained in Section 3.2.2.

This is clearly a very good piece of science and it deserves to be published. The authors have mostly avoided/refused to address my previous statements and continue to sell this as a new study on the relationships between bed and surface. While this is partly true, the real strength of the work is the development of the numerical adjoint approach. I feel that a title such as ‘A general numerical framework for studying the relationship between bed and surface properties on

glaciers', is much truer to the actual focus of this study.

Response: We have underlined the development of the general framework for studying relations between bed and surface properties by adding a sentence in the Abstract. We are hesitant to changing the title of the manuscript at this stage because firstly, it is rather uncommon to do this so late in the review process, and secondly, and also more importantly, because we believe that the title in its present form is accessible to a broader audience interested in glaciological problems. We welcome the editor's advice on the issue of changing the title of the manuscript, and will follow it.

The manuscript is somewhat unevenly written. Have the co-authors spent enough time in helping with the formulation? It feels the manuscript needs some fresh eyes to go over it in detail and streamline it.

Response: All authors have carefully drafted, edited, read and re-read the manuscript several times, responding and taking into account the reviewers' comments. We have aimed to keep the text as accessible as possible to a broad audience, and which may be (mis)interpreted as not being streamlined to a more narrow recipient group. We are aware that especially numerical experts may have appreciated a more specialised or technically streamlined narrative. For the latter, we are happy to refer e.g. to our companion paper in *The Cryosphere* 14, p 673.

- I feel the citation of SSA should be to the original work by Morland, or alternatively to the first paper by MacAyeal (this is cited.) No need to attribute this idea to any later authors that then used this idea in later studies.

Response: Citations of SSA in Sect. 2.2 are removed except MacAyeal.

- SSA in two dimensions might be misunderstood. I think you are referring to a flowline situation, ie to one horizontal dimension.

Response: Thanks. We change it to 'The flowline model of SSA'

- Not sure Weertman included longitudinal stresses in his 1961 solution as stated in the manuscript. Is this correct? Do check this, not sure it is.

Response: Yes, Weertman introduces the longitudinal stress in his formulas.

- L14 p 8 : 'calibrated' → scaled or normalized

Response: Change to 'normalized'.