The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-108-RC2, 2020

The Cryosphere
Discussions

Interactive comment on "Sensitivity of ice sheet surface velocity and elevation to variations in basal friction and topography in the Full Stokes and Shallow Shelf Approximation frameworks" *by* Gong Cheng et al.

Anonymous Referee #2

Received and published: 22 July 2020

This is generally speaking a good paper and clearly in terms of the numerical aspects a highly accomplished work.

Largely I have a very positive view of the manuscript, but the manuscript is not particularly well written or structured. My main worry is that the authors appear to have forgot to start their work by reading previous papers on the subject. In fact many of the statements presented in the paper as new findings, are not. For example the last three sentences in the abstract could have been in a number of previous papers, and arguably really just reflect common knowledge. Although, the sentence 'There is a de-

lay in time between a perturbation at the ice base and the observation of the change in elevation' is actually not quite correct. (The surface topography responds immediately, but obviously it takes finite time for a finite-sized surface bump to be formed at the surface.)

The study is essentially numerical in nature. Similarly to other such numerical studies, this approach cannot really give a proper overview over the transformation of bed properties to the surface. Inherently such studies will be limited to giving some (typical) examples and to provide a flavor of what can be expected. On the other hand, this numerical allows for all non-linearities and finite-amplitude effects to be considered. I suggest that the authors do some rewriting and focus on the real strength and the novelty of their work. Fundamentally this a methodology paper where new time-dependent adjoint capabilities are developed and tested. This represents important progress in the field and is definitely publishable and of interest to the TC community. However, this is not a new theoretical study of study of the 'Sensitivity of ice sheet surface velocity and elevation to variations in basal friction and topography in the Full Stokes and Shallow Shelf Approximation frameworks' as a reader might be lead to believe based on the title.

The paper should be refocused and shortened. For example the introduction is very general and does not give the reader a feel for what the paper is really about. The adjoint approach does not give the sensitivity of velocities, topography, etc to a basal perturbations, thatis it does not give the derivatives du/db where u are surface velocities and b basal topography. It gives the derivative dI/db where I is a (scalar) cost function. In this paper the I is referred to as the Lagrangian function and is, for example, defined as the integral over surface velocities multiplied by a delta function in time and space. This limitation is inherent in the methodology used. In fact the adjoint method can be thought of as a computationally efficient approach to calculate dI/db without having to calculate the sensitivities du/db.

Arguably this makes the approach use less suitable for providing general information about du/db than a calculation/estimate of dI/db. I can't see that the authors obtain any general results on the bed-to-surface that expand over and above what we know already from papers such as Gudmundsson, 2008. This is not to say that the paper does provide many new and valuable insights. However statements such as 'Perturbations in the friction coefficient at the base observed in the surface velocity determined by SSA are damped inversely' are arguably less specific that some previously published results. And a further example 'proportional to the wave number and the frequency of the perturbations in (40) and (45), thus making very oscillatory perturbations in space and time difficult to register at the ice sheet surface.' is not a particularly precise or informative statement. If the authors want to make statements about bed-surface relationships, forward or inverse, then they should consider replicating some of the previous work first, and then maybe expand on particular aspects.

I feel the authors missed a few citations. For example:

Monnier, J. and des Boscs, P.-E.: Inference of the bottom properties in shallow ice approximation models, Inverse Probl., 33(11), 115001, doi:10.1088/1361-6420/aa7b92, 2017.

I doubt the solution to (33) is new. I believe the same idea, and almost identical solutions, have been published many time before. For example see Eq. (8) in Weertman, 1961, and Nye 1959.

In summary, this manuscript should be shortened considerably and should focus on the development and testing of new time-dependent adjoint capabilities. I think this may actually not be that difficult, and may ultimately make the paper more readable and focused.

Citations Nye, J. F.: The Motion of Ice Sheets and Glaciers, J. Glaciol., 3(26), 493–507, doi:10.3189/S002214300001724X, 1959.

Weertman, J.: Stability of ice-age ice sheets, J. Geophys. Res., 66(11), 3783–3792,

doi:10.1029/JZ066i011p03783, 1961.

Gudmundsson, G. H. and Raymond, M.: On the limit to resolution and information on basal properties obtainable from surface data on ice streams, Cryosph., 2(2), 167–178, doi:10.5194/tc-2-167-2008, 2008.

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