Response to the reviewer 2

August 17, 2020

All the equation and section numbers refer to unrevised version of this paper.

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 delay 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.)

Response: We have included about 65 references related to the subject in the paper and read them all. They are referred to in Introduction and in the other sections. It is true that some of the conclusions are found in other papers (e.g. about the damping of high frequency perturbations) but the analytical derivations and the explicit expressions are not found elsewhere. The effect of time variable perturbations in FS (Section 3.1.1) and SSA (Section 3.2.5) is new. There is a delay (or phase shift) in time when the full effect of a perturbation of the topography is observed in the elevation of the surface, see Figs 2 and 9d. This holds true for the friction in SSA too in Fig 9c. For an oscillatory perturbation as in Fig. 2, it is fair to call this a delay in time. The weights w_C and w_b are ≈ 0 for δC and δb at (x_*, t_*) in Figs 9c,d indicating that a sudden change is visible only later when $w_C \neq 0$ and $w_b \neq 0$. A perturbation in the summer is growing at the surface reaching a maximum in the fall. This is also illustrated in a new example with a step perturbation.

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.

Response: With analysis of the adjoint FS equations in Sections 3.1.1 and 3.1.3, we derive expressions for the influence of a time dependent and a time-independent perturbation δC on a velocity component and the elevation in (17) and (18). Explicit expressions for how δu and δh depend on δC and δb in the SSA model are given in (38) and (42). We believe that these results are new. The advantage with analytical results compared to numerical results is that the dependence on the forward solution (e.g. u and H) and parameters (e.g. a and C) is apparent as the reviewer remarks. This is not the case with numerical calculations. The expressions are compared to time dependent and steady state numerical computations with FS and SSA in the companion paper Cheng and Lötstedt 2020. The differences in the forward solutions with and without perturbations, e.g. $\delta u = u(C + \delta C) - u(C)$, agree very well with the predictions using the adjoint techniques and the explicit formulas. The results from Cheng and Lötstedt 2020 are discussed now in several places in the text as a response to Referee 1. In a revised version of the paper, we are willing to modify the title to better reflect the contents of the paper.

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, that is 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.

Response: By choosing F as in (12), the scalar functional $\mathcal{F}$ is $u_1(x, t)$, the x component of the velocity. If we are interested in the y component u_2 then F will be slightly modified. The same is true for the z component u_3. Later after (17), F is chosen such that $\mathcal{F}$ is $h(x, t)$. Both u_1, u_2, u_3, and h and the corresponding $\mathcal{F}$ are scalar variables. In SSA in 2D in Section 3.2.1, the velocity u is scalar. Suppose that u_1 is observed at discrete x_i and C is perturbed at discrete y_j. Then the relation between δu_1 and δC is $\delta u_{1i} = \sum_i W_{Cij} \delta C_j$ where $\mathbf{W}_C$ is a Jacobian matrix with elements $\partial u_{1i}/\partial C_j$ and can be determined by the adjoint approach. This is discussed in a new paragraph in Section 3.2.3 and a reference to Cheng and Lötstedt 2020 is made. For δu_2, $\mathbf{W}_C$ will be different. The relation between the sensitivity problem and the inverse problem is established in Section 3.1.2.

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.

Response: We have explicit expressions for the dependence of δu and δh on time independent parameters in SSA in (38) and (42). Using these formulas we derive an explicit expression (40) for how δu depends on the wave number k with much more detail than previously. As an example, the sensitivity to oscillatory perturbations increases as $1/H^4(x)$ when the ice is getting thinner closer to the GL and $x \to x_{GL}$. The expression is similar for δh, see the end of Section 3.2.4. The formulas (38) and (42) are valid for any type of perturbation, not just oscillatory ones. The time dependent weight function is approximated in (45) for an expression for perturbations oscillating in time. The sentences quoted by the reviewer are summaries in words of the precise formula (40) and the approximation (45). A comparison is made with Gudmundsson's results in the end of Section 3.2.3. There FS for an ice slab is linearized with frozen coefficients and $n = m = 1$. Using Fourier analysis as in Gudmundsson 2008, it is necessary to have constant coefficients in the PDEs in the analysis and the perturbation at a point x_* does not follow from the analysis. The coefficients depending on u and H are not frozen in our adjoint PDE (34) but vary with x. In our formula (40), the expression in the parenthesis varies with x_*. Depending on k and x_*/x_{GL} the first two terms may cancel each other and then $\delta u_* \sim 1/k^2$. Our results are for the nonlinear SSA model with any $m > 0$ and, as a result of the weak influence of the friction, independent of n.

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.

Response: Thanks for the reference. We refer to this paper now in Introduction.

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.

Response: We do not claim that the solution (35) of (33) is new but we need u and H in (35) to derive the solutions (38) and (42) to the adjoint equations. Three new sentences after (35) discuss Nye's and Weertman's solutions in relation to (35).

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.

Response: We have argued above that there are results in the paper that are new and unique (not only the time dependent adjoint equations) and would prefer to keep these in the revised version. Examples are the solutions to the FS equations in Sections 3.1.1 and 3.1.3 and the explicit SSA solutions in Sections 3.2.3 and 3.2.4. The editor and the other referee suggested no radical shortening of the paper. Our planned revised version will contain the same material (somewhat expanded as a response to the editor and the referees) as the original version.