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Interactive comment on "Parameter sensitivity analysis of dynamic ice sheet models-Numerical computations" *by* Gong Cheng and Per Lötstedt

Gong Cheng and Per Lötstedt

cheng.gong@it.uu.se

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Response to Anonymous Referee #1

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The authors consider a very important and interesting problem, ie the (inverse) problem of estimating the sensitivity of basal flow parameters to surface date. In fact, this is such an interesting question that it has been addressed many times in many publications in glaciology before. I have a positive view of this work. However, I think the best approach forward is to ask the authors to rework their manuscript and provide much better context and comparison of their work with previous work. Below I give some references to papers that the authors might find useful in this respect.

Response: The new references suggested by the reviewer have been added and more comparisons are made with earlier work in the Introduction and at other places in the paper.

The formulation of the adjoint equations for the time depended SSA case is, I believe, done here for the first time. I found it next to impossible to follow the derivations in the paper. However, reading (Cheng & Lötstedt, 2019) this all became much easier to understand. I wonder if it might not be a good idea to focus the paper more on the relevant message to the glaciological community and either offload some more of the technical details to appendixes or just refer to the arXiv manuscript.

Response: More discussion and conclusions are found in the final two sections. The description of the SVD is moved to a subsection in the Appendix.

I found it very nice how w_{ub} and w_{uC} are determined from the solutions of the adjoint problem ϕ and v. This is actually a straightforward application of the adjoint method, but at least in glaciology I have not seen this done so often, although possibly (Martin & Monnier, 2014; Monnier & des Boscs, 2017) may have done this already. This is a clever way of estimating the sensitivity of, for example, velocities at one given location to any perturbation in C. (But are not a brackets missing in Eq. 20 and 21?). I suspect that this can easily be done in any modern ice-flow model by just modifying the cost function to include surface data from only one location at a time.

Response: Yes, the weights can be computed for any ice model not just FS and SSA. We discuss the mentioned papers and do not think that Monnier et al did it in this way. The convention is sometimes that the brackets are not needed around a sum in the integral.

I found the transfer matrix approach also to be very interesting. As this approach has been used before by (Gudmundsson, 2008; Gudmundsson & Raymond, 2008; Pralong & Gudmundsson, 2011; Thorsteinsson et al., 2004) it would have been valuable for the reader to be able to understand to what the differences are with respect to those previously published studies. Since the authors mostly consider the case $m = 1$ they can compare this with previously published analytical transfer functions (note that the $m > 1$ solutions by (Gudmundsson, 2008) contain an error, but the $m = 1$ case is OK). It appears that the main differences are that this study is numerical and the sensitivity matrices Q_{ub} and W_{uC} evaluated numerically. This gives great flexibility, but makes it more difficult to arrive at general conclusions. The work seems related to (Martin & Monnier, 2014) who also used a purely numerical approach.

Response: The transfer matrices are evaluated both numerically and analytically for SSA. We have written a paragraph in the Result section about the interpretation of the formulas in the Appendix.

The authors state that previously 'The time dependent height equation for the moving upper surface is not included in the inversion.' While this may be true for some inverse models, there are a number of publications that use ds/dt (s being the upper surface) information in the inversion. This has been done for example by using the kinematic boundary condition at the upper surface or the vertically integrated mass conservation equations. To my knowledge, all the modern ice-flow models (i.e. ISSM, Wavy, Ua, BISICLES, Elmer/Ice) allow for this option. The dh/dt (h ice thickness) is, for example, used to determine ice thickness in BISICLES and ISSM and when solving for basal slipperiness and ice rheology parameters in Wavy and Ua. See for example (Kyrke-Smith et al., 2018; Monnier & des Boscs, 2017). However, the authors are I think right in stating that the adjoint equations have not been derived for the transient SSA equation before. However, I believe that in effect Dan Golberg has done so previously using automated differentiation (Goldberg et al., 2015).

Response: Goldberg's work is discussed. It uses autometic differentiation, numerical approximation in time, and transient data but the analytical adjoint is not derived. The analytical adjoint equations allow us to draw conclusions about the solutions for FS and SSA (e.g. in the Appendix) and the sensitivity in the more theoretical paper in arXiv by the authors and in a new paragraph in Section 3.2. Another conclusion is that the adjoint height equation and its solution is important for height perturbations but not for velocity perturbations (see e.g. Conclusions). Yes, time dependent data are permitted in modern codes but without including the time derivative of the height in the differential equation. For instance, in Monnier and des Boscs for the extended SIA model dh/dt is subtracted from the surface mass balance and a stationary problem is solved (7).

I must confess that I found most of the conclusions and the points addressed in the discussions rather weak. It is always going to be difficult to make any general statements about parameter sensitivity using a numerical approach. I think the approach the authors use is excellent if looking at some specific domains and for some specific model studies. I could for example imagine this to be a useful exercise when looking at particular parts of, for example, the West Antarctic Ice Sheet.

Response: Numerical evaluations of the transfer matrices are the only possible option for complicated geometries and nonlinear equations. Under certain assumptions, the analytical expressions for SSA in the Appendix tell how the sensitivity varies with u, h, C, and b. More discussion and conclusions have been included in the new version of the paper regarding this issue.

A side issue that I have with the general approach is that an inverse problem never explicitly defined. Often in inverse theory one states that the objective is, for example, to evaluate to conditional probability $P(C|u)$. This then allows one to define all kinds of clearly defined properties such as the number of resolved model parameters as function of the number of measurements and measurements errors, etc. etc. I understand that the authors are here only interested in parameter sensitivity, but this somewhat narrow viewpoint of an inverse problem makes the findings arguably less interesting.

Response: The relation to the inverse problem is discussed in the new Section 2.2.5 now. There is also a discussion of this matter in the arXiv paper.

Overall, I have a very positive view of this work. It is highly competent and I enjoyed reading the paper. I would suggest making more references and links to existing work. Also, consider taking some of the technical aspect and put them into appendixes. Especially since the computations cannot really be understood without reading authors previous paper on this subject.

Response: Thank you for the review. More references have been added and discussed and the SVD account is now in the Appendix.