Manuscript prepared for Geosci. Model Dev. Discuss.
with version 2014/05/22 6.83 Copernicus papers of the LaTeX class copernicus.cls.
Date: 20 February 2015

Author response for: *A stabilized finite element method for calculating balance velocities in ice sheets*.

Douglas J. Brinkerhoff[1] and Jesse V. Johnson[2]

[1]Geophysical Institute, University of Alaska
[2]Group for Quantitative Study of Snow and Ice, University of Montana

Correspondence to: Douglas J. Brinkerhoff (douglas.brinkerhoff@umontana.edu)

1 Response to anonymous reviewer

Once again, we thank the anonymous reviewer for their comments. The reviewer's emphasis on mathematical precision is particularly helpful in clarifying the specifics of this work.

1.1 Major Points

The reviewer notes that our statement that isotropic smoothing can approximate 'longitudinal stress' in a glacier is confusing, because it would seem that longitudinal stress is a distinctly anisotropic concept. A more precise phrase would be 'horizontal plane' stresses, which could include true along-flow stresses (what we think the reviewer had in mind for longitudinal stress), as well as lateral drag. We have updated the manuscript to account for this change.

1.2 Minor Points

- Agreed, our notation is erroneous. We have updated these equations to include indications that the vectors and gradient operator should only be considered in the map plane where appropriate.
- See above.
- We disagree with the reviewer's assessment here. The equation in question is first order, and admits only one additional condition to specify integration constants. As such, the boundary at which the influx or outflow boundary condition is not specified receives no special treatment relative to anywhere else in the domain. In this sense, there really is no boundary condition, not just a zero Neumann condition.
- Indeed, the presence of the spatial dependence of H makes our claim false. Otherwise the operator would be the Laplacian, which would be self-adjoint. We have updated the text to reflect this change.

- While we argue that H_0^1 is standard notation, the reviewer is correct that we directly contradict this assumption in Eq. 9. We have changed this to reflect the fact that we are not applying homogeneous Dirichlet boundary conditions.

- Indeed, the derivative of λ must be integrable. Updated text to reflect this.

- Text updated to reflect reviewer's suggestion.

- We are not sure what 'good' would be in the context of the reviewer's suggested metric. This section is an exploration of the influence of smoothing length, not an attempt at quantitative comparison with InSAR (as is done in Section 5). Nonetheless, we have removed this point, since it is somewhat distracting.

- There is no confusion about what the components of the objective function represent. The reviewer's point about minimization versus finding a saddle point is well taken. We have updated the relevant sections to discern between these two processes. Our procedure uses symbolic differentiation to find the Gateaux derivative of a specified functional, and then we specify the adjoint and derivatives of that functional. These are then used in gradient-based optimization methods. What we have illustrated, automatic differentation aside, is quite literally what our algorithm does, and we assert that it is relevant and should not be removed from the manuscript.

- We agree that λ should be inside the integral, and the text has been updated to reflect this.

- We do not use 'state' here in the way that the reviewer interprets it, but we have changed the text as a means of clarification.

- Agreed. We have made the relevant changes in the text.

- We fail to see how this equation is unclear. Indeed, we think that the labeled underbraces state plainly what each term in the computed Gateaux derivative represents.

- We agree, and have changed the text in accordance.