

Interactive comment on “A stabilized finite element method for calculating balance velocities in ice sheets” by D. Brinkerhoff and J. Johnson

Anonymous Referee #2

Received and published: 15 September 2014

The paper entitled “A finite element approach to ice sheet balance velocities” by Douglas Brinkerhoff and Jesse Johnson presents a new approach, based on the use of the finite element method, to solve for balance velocities of ice sheets.

I reviewed this manuscript in April 2013 for another journal. I liked the paper but thought that it was out of the scope of this specific journal, so I am happy to see this manuscript submitted in GMDD, which is much more appropriate. However, I was disappointed because this “new” version of the manuscript is entirely identical to the one I reviewed almost a year and a half ago. The authors did not change a single word, did not address any of my comments/suggestions, even the typos are still there! My review is consequently very similar to the one I submitted last time.

Overall, this manuscript is interesting, introduces a new approach to solve for balance
C1767

velocities and is worth publishing in GMD. I found that this paper lacks mathematical rigor here and there, but that should be easily corrected in the next version. The authors should take advantage of the fact that there is no limitation in space (compared to the journal this article was first submitted to) to provide a more in-depth analysis of their application. Though relying on the mass conservation equation to derive balance velocities is not a new idea, the technique presented here is novel and has some significant advantages over previously published methods (e.g., Bamber et al. 2000).

I don't have any major concern and recommend this manuscript for publication in Geoscientific Model Development after some revisions (which will, I hope, be taken into consideration this time).

1 Main points

One of the main issues is that, although this method is applied to the Greenland Ice Sheet, no significant scientific advances are apparent. We do not learn anything from a science point of view. It would have been interesting to discuss the regions where the balance velocities do not match the surface observations and investigate whether these inconsistency are related to ice sheet imbalance, or to numerical artifacts of the method.

Second, the results obtained with this new technique are not of significantly higher quality than the ones from Bamber et al. 2000. Some ice streams are poorly represented (e.g., North East ice stream), some others are artifacts of the method, and ice flow in the South East coast is not realistic: velocities are too high compared to Moon et al. 2012 or Rignot et al. 2012.

2 Minor points

As the authors must know, there are several new ice thickness datasets that are much more realistic and include many more features than the one from Bamber et al. 2001 that is used here (e.g. Bamber et al. 2013, Morlighem et al. 2014). I highly recommend to update the results with one of the new datasets, and even maybe assess whether these new datasets make a difference compared to the old one of 2001?

Page 5185 line 8, the authors mention that balance velocities can be used to fill the gaps in surface velocity observations. I don't believe that this statement is mathematically correct since. As the authors mention later on, only one constraint per flow line can be applied (page 5187 line 17). We cannot constrain the velocity all around a "patch", where observations are not available as this would constrain the velocity twice (or more) per flow line.

The Leibniz integration rule applied to Equations (2) and (3) does not give (4). This is because Equations (2) and (3) are not correct, (2) should be:

$$\frac{\partial S}{\partial t} + u(S) \frac{\partial S}{\partial x} + v(S) \frac{\partial S}{\partial y} - w(S) = \dot{a} \quad (1)$$

and same goes for the equation for B (3).

- p. 5186: I don't think Morlighem et al. 2010 use this equation
- p. 5186: define norm 2
- p. 5187 line 21, I am not sure to understand what it means to say that the equation is symmetric. The bilinear operator that appears in the weak formulation is symmetric, maybe this is what the authors meant?

C1769

- Eq. 10: you assumed the following boundary condition here:

$$\int_{\partial\Omega} \phi \cdot (lH)^2 \nabla \tau_s \cdot n \, dS = 0 \quad (2)$$

which translates into $\nabla \tau_s \cdot n = 0$ on $\partial\Omega$. It is ok but this has to be mentioned.

- Eq. 11: you have not integrated your equation by parts and no boundary condition is specified, so the solution of your equation might not be unique. You have not defined the solution space either (in what space are \bar{U} and λ).
- p. 5188 l. 11: λ is not an operator, but a test function, and cannot be "bilinear". I guess the authors meant piecewise linear? But that would make the assumption that we are using P1 finite elements on a triangle mesh, and the method could be applied with other types of finite elements.
- p. 5192 l. 1: should read "Lambert Glacier-Amery ice shelf"
- references: lots of missing capitals (e.g., Greenland, GPS, Antarctica, etc.)
- Fig 1: define \mathcal{L}^2 norm, why \bar{U} (2^5)?

3 References

Bamber, J., R. Hardy, and I. Jouglin, An analysis of balance velocities over the Greenland ice sheet and comparison with synthetic aperture radar interferometry, *J. Glaciol.*, 46, 152, 2000.

Bamber, J., R. Layberry, and S. Gogineni, A new ice thickness and bed data set for the Greenland Ice Sheet 1. Measurement, data reduction, and errors, *J. Geophys. Res.*, 106(D24), 33,773–33,780, 2001.

C1770

Bamber, J. L., et al., A new bed elevation dataset for Greenland, *The Cryosphere*, 7, 499–510, doi:10.5194/tc-7-499-2013, 2013.

Moon, T., I. Joughin, B. Smith, and I. Howat, 21st-Century Evolution of Greenland Outlet Glacier Velocities, *Science*, 336(6081), 576–578, doi:10.1126/science.1219985, 2012.

Morlighem, M., E. Rignot, H. Seroussi, E. Larour, H. Ben Dhia, and D. Aubry, Spatial patterns of basal drag inferred using control methods from a full-Stokes and simpler models for Pine Island Glacier, West Antarctica, *Geophys. Res. Lett.*, 37(L14502), 1–6, doi:10.1029/2010GL043853, 2010.

Morlighem, M., E. Rignot, J. Mouginot, H. Seroussi, and E. Larour, High-resolution ice thickness mapping in South Greenland, *Ann. Glaciol.*, 55(67), 64–70, doi:10.3189/2014AoG67A088, 2014.

Rignot, E., and J. Mouginot, Ice flow in Greenland for the International Polar Year 2008-2009, *Geophys. Res. Lett.*, 39, L11501, 1–7, doi:10.1029/2012GL051634, 2012.

Interactive comment on *Geosci. Model Dev. Discuss.*, 7, 5183, 2014.