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The Cryosphere
Discussions

Interactive comment on "Sensitivity of basal conditions in an inverse model: Vestfonna Ice-Cap, Nordaustlandet/Svalbard" *by* M. Schäfer et al.

Anonymous Referee #2

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My recommendation is that this paper is published in The Cryosphere after modifications listed below.

The paper describes glaciological modelling applied to the Vestfonna Ice-Cap. The main innovative aspect is that a very good attempt is made to constrain the basal drag coefficient in the sliding law, providing a much more realistic flow pattern than could be obtained otherwise. This is accomplished using a recently published inverse method for the incompressible Stokes equations that allows the basal drag coefficient to be recovered from measurements of velocity provided by satellite interferometry.

This paper represents an important demonstration that the inversion approach can be

applied for Stokes flow in a three-dimensional, real-world situation, with uncertainties in velocity data, digital elevation model, internal temperatures etc. The treatment of uncertainty is generally good. By varying different aspects, such as the temperature profile, the finite element mesh, and the digital elevation model, the authors make a good case that their inversion is robust to likely errors in the data, and to changes in model resolution.

The weaknesses in the paper are mostly in the mathematical description of the forward model, the numerics, and the inverse method. Numerous errors appear in the equations presented here. I suspect that most of these are typographical errors, and that these are not the equations that were actually solved. Nevertheless, the paper cannot be accepted in its present form until these are addressed. To alleviate some of these concerns, I think the authors should perform a test calculation with a known solution, perhaps repeating a synthetic 3D inversion similar to that shown by Arthern and Gudmundsson (2010). This would not need to be reported at length, but a sentence or two describing the outcome of such a test would certainly give the reader more confidence in the results.

Once these changes are made this study will provide a very useful case study of how to set up an ice flow model to give a realistic flow field, and how to investigate the sensitivity of the flow to uncertain quantities.

Substantial Corrections

Section 2.3 The velocity data are referred to as horizontal velocity, and are used in the inversion as such. There needs to be slightly more information about how displacements in the look-direction from InSAR were resolved to give the horizontal component of velocity. How many independent look directions were typically available? Is there an assumption that the flow vector is parallel to the surface?

Section 3.1 Equation 4: Check sign on pressure in equation 4. By usual convention extensional stresses are positive. Equation 4 is inconsistent in this respect with the weak form presented in equation 12.

Equation 11: There needs to be much closer attention paid to notation here. $\nabla\mathbf{v}$ is not the same as $\nabla.\mathbf{v}$. Shouldn't the test functions in equations 11 and 12 be different, with a scalar test function in equation 11 enforcing $\nabla.\mathbf{v} = 0$, and a vector one in equation 12, enforcing the momentum equations?

Equation 12: Again, more attention needs to be paid to notation here as this is hurting clarity. It needs to be clear when the dot product is implied (e.g. in the gravitational term). Notation associated with the first term needs to be checked: is this supposed to represent a Frobenius product of the two tensors?

Introduction to section 3.2.1 should make clear that this section is summarising the earlier paper by Arthern and Gudmundsson (2010).

Line 20: Could be more explicit. Is $\tau_{zz} = 0$ imposed at the surface in both cases, or is zero normal stress on the surface imposed?

Equation 14: This is wrong. Not sure where the β came from.

Equation 14: Notation needs sorting out. Tractions on surface are sometimes written as dot product of stress tensor with normal vector (e.g. Eqns 10, 12) sometimes not (Eqn 14).

Equation 14: Strictly speaking the theory applies to a cost function derived using the full stress tensor and the full 3D velocity vector. Here the horizontal velocities and the deviatoric part are used in the surface integral. It needs to be made clear whether this is an approximation, or an exact consequence of fixing τ_{zz} at the surface in both simulations. Since the gradient applies to the original cost function, it would be better to introduce the original cost function, then show how it has been modified, and what assumptions have been made to do so.

Equation 15: The order of Dirichlet and Neumann have been reversed relative to Arthern and Gudmundsson (2010). Similarly, wrong order is used for line 10 'Beta should

be updated as ...'.

Section 3.2.1. Line 13. It would be worth pointing out here that this update rule can be rewritten in the original form for a particular (spatially varying) choice of α_β, and that this α_β is guaranteed to be positive, and that it tends to zero as the new stepsize parameter ν tends to zero. Hence the new rule is not purely empirical, but has some theoretical justification.

Minor Points

Abstract Line1: Change 'is' to 'are'.

Introduction Line 6: Change Navier-Stokes to Stokes.

Section 2.3 Line 12: replace 'are used to calculate' with 'were used to calculate'.

Interactive comment on The Cryosphere Discuss., 6, 427, 2012.