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The Cryosphere
Discussions

Interactive comment on "The transferability of adjoint inversion products between different ice flow models" *by* Jowan M. Barnes et al.

Anonymous Referee #1

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After reading the abstract, I was highly enthusiastic about this study, but felt it somewhat lacking after reading the entire paper.

Using rather vague language, the abstract suggests that in fact the inversions can transfer well, but in fact the results suggest something quite different. Simulations that after 40 years differ by a factor of 2 in predicted sea level rise can hardly be called successful, even if the bar has been set low by earlier studies. Likewise, diagnostic simulations that appear to differ by more than 2000 m/yr in places (see Figure 6).

The paper goes reasonably well through section 4.3, though I would argue that similarities and differences between the solutions are not that interesting. Even for a given model/inversion method, the correlation coefficient should be quite different for differing amounts of regularization (e.g., different members along the L-curve but with similar

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Discussion paper

misfits).

Once the paper gets in the diagnostic and prognostic simulations it becomes really not that helpful. What is shows is that one model, Ua, produces vastly different results with the different inversions, which may say more about Ua than about the generally transferability of these inversions. Is Ua highly sensitive to its tuning parameters? Hard to tell without seeing the other model results.

To really produce a robust finding, we need to see what the diagnostic simulations looks like with the other models (e.g. Fig 6 should have 9 panels, 3 models x 3 inversions). Same with the prognostic simulations (9 curves per figure). These are fairly low res simulations and not that difficult to run. Given that the paper includes co-authors from all 3 groups who have already done the work to setup the inversions, there is no reasons these additional simulations could not be run (I really feel this is essential for publication of this paper). It's in the authors best interest to do this, because the at present the spread in the results casts significant doubt on all 3 models, particularly Ua.

I could see if the models where of different order that results would be different, because the tuning process can compensate for some of the differences. But Ua and ISSM are both shallow shelf and both performed inversions on grids of similar complexity. The results should not be this different unless there are differences in the implementations of the same basic equations that really need to be elucidated. While some attribution is made to the implementation of the grounding line, these differences should not be that visible more than about 10 ice thicknesses inland, and as Figure B1 indicates, they extend more than 200-km inland. In figure 6 c, the really fast blobs in the interior seem to indicate some re-interpolation artifacts that should be fixed.

I would really like to see Ua turn off B for the grounded ice. I suspect a large part of the better fit is really a consequence of having twice the degrees of freedom at each point (B and Beta vs just Beta). I suspect some degree of over-fitting on the part of Ua because the residual is better than the velocity errors (even if the formal errors indicate otherwise, they are not that good).

Specific Comments Line 235 – are these velocity (sqrt((u-uobs)ˆ2 + (v-vobs)ˆ2)) or speed (Vdiff) numbers. I would assume the former, but the context suggests the latter.

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