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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.

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Ice sheet model initialization is a crucial step to ensure that a model is as close as possible to the current state of the ice sheet. In this paper, the authors propose to infer two poorly known parameters (the basal friction and the stiffness or viscosity of the ice) in three different models and evaluate the differences between these models right after inversion and after a prognostic simulation, when transferring an initial state from one model to another.

The paper is relatively clear and I enjoyed reading it. The problem they set out to address is well-introduced with appropriate references. I found that the methodology and results were detailed well, although some sections and technical choices were

harder to follow due to back and forth between the main text and the appendices and sometimes lacking references to the appendices (see specific comments). The authors rightly recognize that the three models they use show significant differences (especially for the rate factor B) but that the large-scale distribution agrees well. While this is true for ISSM and STREAMICE, I am concerned by the results of Ua, which show particularly lower misfit between observed and model velocities. In this regard, the authors investigate various possibilities (in the Appendix) for explaining the variations between the models, but I would have liked to see more discussions on the L-Curve analysis. I detail this in a more specific comment but the difference in the misfit between the models brings up some questions about the regularization parameters used in the different models, e.g., are the three initial states really the minimum of each L-curve? Since an inversed state is particularly sensitive to the regularization parameters, it would be interesting to see the L-curve distribution and the location of the initial state picked for each model.

The paper then evaluates the transferability of Ua, ISSM and ICESTREAM initial state in Ua and with a coarser version of the Ua mesh. They conclude that this process is not straightforward and leads to substantial variations, but lies within the range of intercomparison experiments such as initMIP. However, in initMIP, the prognostic models are all different, which means that the differences are not only due to the initial state but also the physics of the transient models themselves (GL parametrization, etc.). Also, the models had various complexities while here, ISSM and Ua both use the SSA. In this regard, it is surprising that STREAMICE (L1L2) and ISSM (SSA) behave closer to each other than Ua (SSA) and ISSM. I think that the differences between the 3 prognostic simulations are relatively high and make it hard to believe that the transferability of initial states is a success. Comparing initial state transferability to the effect of different friction laws on sea level projections (Yu et al., 2018) is also a bit misleading since the latter involves changes in the physics of the model rather than difference in the numerical implementation (especially when Ua and ISSM both use the SSA). I would therefore recommend to temper these conclusions. In addition, I think that studying the

effect of the transferability from initial states to the other two models (Ua and ISSM to STREAMICE, Ua and STREAMICE to ISSM) could greatly benefit the study. This is a substantial effort (and the authors already did a significant number of sensitivity analysis) but it would provide a more comprehensive idea of the real transferability of initial states in the context of multi-model experiments like ISMIP6 (Serrousi et al., 2020), where one initial state could be provided to all the models.

Regardless of my concerns, the paper certainly deserves to be published (after revision) and will be useful to the community. The ability to use a similar initial state in different models for intercomparision experiments or to speed up some fastidious and repetitive initialization phases is of great interest to me. This paper shows the difficulty of the process and the remaining challenges we face in doing so.

You will find more specific comments in the attached document.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-235/tc-2020-235-RC2-supplement.pdf

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