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Geoscientific Model Development Discussions

Interactive comment on "Towards a better ice sheet model initialisation and basal knowledge using data assimilation" *by* Cyrille Mosbeux et al.

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I think this manuscript could be published in Geoscientific Model Development after a few changes. This paper provides a comparison between two different approaches for estimating the initial state and parameters of an ice sheet model. The paper provides an illustration of simultaneous inversion for bed slipperiness and bed elevation using adjoint methods. This part is not an especially novel endeavour in itself, but it is used here to provide a reference for another method, the combined adjoint/nudging method. The merits of the latter approach lie more in its ease of implementation than its theoretical justification. Nevertheless, if adjoint/nudging is shown to be competitive with more complicated approaches, as seems to be the case here, this would represent a valuable service to those ice sheet modellers that presently have the wherewithal to invert for basal drag coefficient, but have not yet considered the shape optimisation

problem of recovering the basal topography.

The paper is well-structured and clearly written. The figures are useful and clear. The two parameter adjoint approach has perhaps been described better elsewhere, but I think the examination of the combined adjoint/nudging approach as described here is probably still worth publishing in GMD. The results are sufficient to support the interpretations and conclusions. The authors make clear which parts are new, and which have previously appeared in the literature. The title is OK, although the new feature of the paper is the combination of adjoint-based inversion and nudging and this is not prominent in the title. The abstract is fine. The mathematical presentation is clear enough. The number and quality of references are OK.

The simulations used to illustrate the comparison are undoubtedly highly simplified: a simplified approximation of the stress state is used, a 2D flowline rather than a 3D ice sheet is considered, and all the measurements considered in this manuscript are synthetic. These simplifications are expanded upon below. However, to my mind, these do not detract from the central purpose of the manuscript, provided that it is recognised that this paper provides a necessary test that should be passed by the adjoint/nudging method, rather than a sufficient test that would guarantee its usefulness by other models in more general circumstances. In short, this paper might motivate readers to consider the adjoint/nudging method for initialising their models, but each modeller will still need to demonstrate that the method works for their model, in 3D, not just 2D, and each modeller would preferably test the approach with real observations as well as idealised 'twin' experiments.

The simulations use the Shallow Shelf Approximation SSA. This is the shallow aspect-ratio limit appropriate for flow over a very slippery substrate. For shearing flow over non-slippery sediment, another commonly used limiting approximation, the shallow ice approximation (SIA), which is not used here, would be more appropriate. Nowadays, the practical initialisation problem for ice sheets is more likely to be performed with a more sophisticated stress-balance using a vertically integrated 'hybrid' blend of SIA

and SSA stress states, or a depth-resolved higher-order model, or Stokes flow. In these more sophisticated models, the transition from slippery to non-slippery substrate poses no special complications, while for the SSA approximation used here, the accuracy of the model will deteriorate whenever the assumption of extreme slipperiness is violated. The paper would be improved significantly if similar twin experiments were performed using the adjoint/nudging approach for a hybrid model, a higher order model, or a Stokes flow model. This would be especially valuable if it turned out that the bed recovered from the inversion was shown to depend on the approximations used in the momentum equations. The chief selling point of the combined adjoint/nudging method is that it would be easy to apply to more complicated models, so I am not sure why this is not done in this paper. As it stands, the paper points to the promise of this approach for initialising more complicated models, but without a relevant example, it is hard to know whether this is real promise or false promise.

I have fewer concerns about using a 2D flowline simulation for illustrating the two methods, but some readers will wonder whether the two methods would still perform comparably in 3D. The paper is still quiet compact, and a 3D example would make for a fuller investigation. The paper states that the methods can be applied in 3D, but it would be better to show an example.

The use of exclusively synthetic observations represents a limit to the information provided by these simulations. To the authors credit, the data used are based on a real flowline, so the bed inversion at least can be checked. There are a number of regularisation parameters in the inversion (λ_α,λ_{z_b},T,k). Inevitably, these parameters represent rather vague prior information and are quite poorly constrained (Arthern R.J., J. Glaciol., 61 (229), 947-962, 2015, doi: 10.3189/2015JoG15J050). At least it would be good to include a table showing how much the inversion of the bed can vary from the 'true' bed when these are varied.

For ten year simulations with the forward model, it should now be possible to test the evolution of the surface against real altimetric observations. This is perhaps too much

to ask of an initial demonstration paper such as this, but time series of elevation data for Jakobshavn are available and it would be interesting to see how well the different methods reproduce the actual behaviour over ten years.

In summary, this paper makes one point quite nicely – that the adjoint/nudging approach can work well for the SSA, for flowline models, as judged by synthetic 'twin' experiments, but it still leaves many avenues to be explored.

Minor points:

Line 46: Replace 'the' with 'then'

Line 49: Replace 'constrain' with 'constraint' and 'are solution' with 'are a solution'

Line 125: Are unweighted least squares cost functions such as these appropriate, or should error covariance weighting be applied? Might be worth some discussion.

Line 175: Give more details of the Gaussian used to define k. How do results depend upon this choice?

Line 210: Jakobshavn is misspelt

Line 267: Sometimes the 'L' of the 'L-curve' is very clear, sometimes not. It would be good to show the two cross sections through the 'L-surface' at the chosen values using a log-log scale.

Line 305: Since T has in effect become a regularisation parameter it would be good to comment whether this trial of a few values is consistent with the treatment of the other regularisation parameters – is there an equivalent to the 'L-curve' for choosing T.

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