

Interactive comment on “MPAS-Albany Land Ice (MALI): A variable resolution ice sheet model for Earth system modeling using Voronoi grids” by Matthew J. Hoffman et al.

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Received and published: 15 May 2018

General Comments

This paper is a complete and quite detailed description of the the newly developed MPAS-Albany Land Ice model, which will likely see substantial applications over the next few years, so I expect to see this paper cited often. The model itself makes use of well known process models and robust computational infrastructure, and differs from other models perhaps most in its use of FV discretization on Voronoi polyhedra (cell -centred) for conservation of mass etc, complemented with FEM discretization on Delauney meshes (node-centred).

C1

It seems well suited to GMD, and is well written overall, with a series of tests including convergence studies though I do have a few comments to make.

Specific comments

I suggest removing all section 8. The authors acknowledge that Antarctic ice dynamics are not well described at 20 km resolution in other (conventional) models, and agree that probably applies to MALI too (which is conventional in its treatment), and their own results in earlier sections (especially MISMIP3d) are not distinct from other models in that regard. Yet they claim that the results might be considered ‘reasonable’. On what basis? Given that we know that the GL issues are leading order

Don’t get me wrong, I appreciate that a realistic example is a good way to show that all the parts (inverse problem, analysis tools etc) work to a reader more interested in ice sheets than models, but how happy would the authors be to have others cite them in future as saying that Antarctic simulations at 20km are reasonable (i.e suggesting that it gives a rough answer, accurate if not precise, rather than an utterly misleading answer).

Is the authors are keen to have a realistic example, and don’t have the capacity to demonstrate that MALI actually works well for Antarctica at the required resolution, then perhaps ar regional model might be more suitable.

Technical corrections and minor comments

P2L24. ‘adjoint capability’ seems like a made up / slang phrase to me. What is needed is the ability to compute gradients / Hessians / Jacobians (depending on what you are trying to achieve) and the ability to solve problems involving the adjoint of a particular operator is a key part of that. I’d say ‘differentiation capabilities’ if anything.

P6L9. The Jacobian of the residuals of the discrete PDEs, presumably?

P9L16 ‘The scalability of nonlinear solvers’. Missing definite article? See also L19 re linear solvers.

C2

P9L31 'blue layer interfaces'. Not sure what has happened here, but there seem to be some extra 'blue's scattered about.

P10L21. I think PISM does/did treat SIA explicitly.

L11L11. 'Conservation of mass is used to conduct...' seems awkward. Mass is conserved: ice is transported accordingly.

P12L5. Can you comment on the choice of first order Euler? It's true that other models often end up being first order in time (and space), sometimes without being aware, but few would pick Euler as the first choice.

P12 (all of section 4.4) Aschwanden 2012 described a polythermal energy transport model that is more natural than the cold ice model used here and not much more work to implement. Why cold ice?

P16 Section 5 - 'Additional model physics?'

P22, L19. I'm pretty sure there are no optimization methods in Cornford et al 2013. They are in Cornford 2015.

P23. footnote 6 - you *do* have different scalar constants (α_{β} and α_{γ}) in eq.55. Perhaps you mean something different, but since beta and gamma have such different typical values, you must be scaling the $|\text{grad}(X)|^2$ somehow, no?

P25 'The order of convergence *of* 0.78'. of -> is, but more seriously, in what sense is 0.78 consistent with 1. 1.02, or 0.98, maybe, but 0.78 ?

L29: L13. Convergence does not as a rule 'occur' when there is an error of $O(h^n)$ so this seems like sloppy language. You mean that, at $h < 500\text{m}$, your results appear converging to be converging at some satisfactory and uniform rate, and are separated by some acceptable tolerance yes?

P29: L17 - you note here that Leguy 2015 had a Blatter-Pattyn model working for this problem, but elsewhere - including the abstract, you claim to be the first.

C3

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-78>, 2018.

C4