Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-151-RC1, 2018

Geoscientific
Model Development
Discussions
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Interactive comment on "Description and Evaluation of the Community Ice Sheet Model (CISM) v2.1" *by* William H. Lipscomb et al.

Anonymous Referee #1

Received and published: 23 September 2018

This is an impressive model developed by the authors, and a comprehensive writeup that addresses a great deal of questions that many such papers generally do not answer. The CISM 2.1 model is an impressive piece of computational engineering. The experiments they present are similarly impressive given the time scales involved, the speeds presented, and the depth of detail able to be resolved. I enjoyed reading the discussion on the Greenland experiment, and the tradeoffs between the efficiency and accuracy of DIVA.

I note that i was unable to install CISM as the only linux stations to which i have access are those maintained by my academic department, and it is a departmental policy not to allow root (sudo) access on these computers, which prevents the use of package managers. (this type of situation may not be uncommon and the authors may want to think about providing alternative installation instructions.)

I do not have any general criticisms for this manuscript and i found it to be well-written and comprehensive. I do however have a number of specific comments/questions that I feel the authors should consider before publication.

eqn 13: i think it would be better if the actual boundary conditions were stated, rather than/in addition to the form of pressure used in the BCs.

eqn 31: note that Arthern et al (2015) avoids the double integral by raising the (s-z') term to various powers.

p 12, line 20: and initial tau_b? and initial \eta?

p 14, line 14: are you in fact using the L2 norm, which is independent of resolution/number of nodes? or is the norm simply the root mean square of a vector, which i believe is called the l-2 (script lowercase l) norm?

p 14 lines 16-18: how is the user notified upon nonconvergence?

eqn (37): the text says this is solved in each column, but you show the full laplacian term, which you explain later as approximated as vertical only – but upon reading eqn 37 this is not yet stated.

eqn 44: i don't see how this follows from (42) and (43). i do see how such a relation would follow is effective strain rate is always proportional to effective stress, which it is, but this is not implied by (43).

p 18, line 12: how do you ensure T does not go above the melting point?

p 18, line 17: there is something confusing about this, why is a vertical remapping needed? is this an alternative to casting the equations in (x,y,\sigma) coordinates and simply solving the equations, which will already contain a vertical advection term relating to the coordinate transform?

eqn (62). I have wondered about this – is it proven this is actually a stability limit with BP or SSA or DIVA? and im not referring to a von neumann analysis as this only takes a linearisation of leading order terms into account. The instability occurs because flux of mass is proportional to the gradient of thickness; but when membrane stresses are present, no matter how strong the bed, if the surface is steep enough they will begin to matter, and i do not believe flux would grow unbounded. This is not something proven; but neither, i believe, is the stability criterion stated for the equations considered.

eqn 72. This is unclear, as it is stated as a thinning rate but is always positive.

eqn 76. Similarly the RHS is always positive, yet i think it is meant to refer to thinning.

eqn 79. What is the accumulation rate?

section 4.1 it would actually be interested to see how the results compare when the DIVA balance is used.

section 4.2. I have always thought that ISMIP-A and -C have always been a bit binary – flat and sliding, or bumpy and frozen. I understand if you do not have the time, but if you do I feel it would be very interesting to examine BP/DIVA comparison in a situation with *some* topography and *some* (if slow) sliding, as this is perhaps closer to conditions tested in realistic models.

p20, line 22. "outside the RACMO ice sheet footprint". unclear what you mean.

p 29, line 27-29. This isn't really a parameter change so much as an adjustment of model physics.

p30, line 32-34. there is a subtlety here and it is something not made clear from the description of DIVA. are you saying that the discrepancy is due to vertical variation of temperature? if so, then i would ask whether DIVA accounts for depth-dependent temperature. It would be easy to implement as viscosity is vertically resolved, and if not i would recommend implementing this for a better DIVA-BP comparison (though, of course, re-running your Greenland tests for this paper would likely be too difficult).

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