Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-322-RC1, 2018

Geoscientific Model Development Discussions

Interactive comment on "A rapidly converging spin-up method for the present-day Greenland ice sheet using the GRISLI ice-sheet model" *by* Sébastien Le clec'h et al.

D. Pollard (Referee)

pollard@essc.psu.edu

Received and published: 26 February 2018

General comments:

This paper applies a simple method of adjusting basal sliding coefficients to obtain realistic ice thicknesses in an ice sheet model of modern Greenland. Similarly to previous simple methods used for Antarctica, the paper shows how the iterative method converges towards basal coefficient maps ("beta") that yield best-fit ice distributions. The method requires relatively short integrations, making it feasible for more complex models. The analysis is detailed and substantial, showing that the method functions well and yields meaningful results, and the paper will be of considerable interest to the

modeling community. My main concern is that, as described in the paper, there are large interior regions where ice thickness errors cannot be corrected due to internal deformation flow being too large, which detracts from the primary results. Additional runs to correct this are suggested below.

Main specific comment:

Much of the paper's primary analysis in section 4 concerns the progress of the procedure as the overall length increases (increasing NBcycle). For given NBiter and NByear, the rms thickness error "dH" tends more or less monotonically to a minimum (Fig. 5), but total volume error "dV" overshoots zero and becomes more unrealistic again (Fig. 6a). The analysis (sections 4.2.3, 4.2.4, Table 1) is mainly concerned with finding values of NBcycle and associated NBiter,NByear, at which dH, dV (and dV/dt, but see below) are qualitatively the best (small) if the procedure is stopped at some point.

I think these results are not the most useful or meaningful, because there are substantial regions in the east-central Greenland interior where internal deformation flow is too large, producing too small ice thicknesses even with zero basal sliding. This prevents the dH and/or dV metrics from both converging to zero together as the procedure is extended indefinitely, and causes the "overshoots" in Figs. 6. This is fully described in the paper's section 5, but only after the primary results of section 4 are presented.

It would be better to address and fix the problem from the start in section 4, which would yield more meaningful results. The existing results, regarding the particular NBiter/NByear/NBcycle values where the dV and dV/dt metrics cross the zero lines, just reflect the influence of the problem region with excessive internal flow. Also they depend on the choice of initial beta(x,y), which is arbitrary (as shown nicely by Fig. 3c), but if chosen further from the final state, needs more NBcycle cycles to reach the same point of evolution.

The problem is fully recognized in the paper's section 5.1, and a possible solution is implied in section 5.2, by trying different values of the enhancement factor Ef. My main suggestion is to repeat the procedure of section 4 for a range of Ef values, say Ef = 0.1, 0.5, 1, 1.5, 2, 2.5, 3. Hopefully just one long procedure would be sufficient for each Ef value, with just one set of NBiter,NByear values, and a large NBcycle of $\sim$10 or 15 (see below).

I would anticipate that for the smaller Ef values, the persistent thickness errors in the Greenland interior can be corrected by adjusting local beta's, so both metrics dH and dV (and dV/dt) will converge towards zero and not overshoot (but see "basal temperatures", below). The main outcomes of the new section 4 would be (i) the value of Ef below which this occurs, and (ii) how long the overall procedure needs to be continued (how many NBcycle's) to reach acceptably small dH and dV. (Possibly the rate of convergence may be quicker for different ratios of NBiter and NByear, but I suspect not, and for the smaller Ef, everything depends just on the total number of years (NBiter+NByear)*NBcycle. Note that if dH converges on zero, then dV and dV/dt must too.

This would of course require significant re-running of the model for the other Efs, and reorganizing sections 4 and 5, but would yield more useful and less arbitrary results in my opinion. One encouraging sign that it will work is how much better Fig. 10 looks (Ef=1) compared to Fig. 7b (Ef=3). (Much the same adjustment of Ef was done in Appendix B of Pollard and DeConto, The Cryo, 2012, called PDC12 here, but was not as important because their main results used a relatively low Ef).

Related to main comment:

One complication involves the basal temperature field, i.e., frozen vs. thawed basal areas. Where the base is frozen, the procedure of adjusting beta is ineffective in reducing ice thickness errors of course. This is mentioned in the paper (pg. 11, lines 1-3), but because of its importance, I suggest showing a map of model basal temperatures Tb(x,y), perhaps near the top of pg. 11 where basal temperatures are discussed, and

assessing it versus other established Greenland Tb maps (such as the recent modeling synthesis in MacGregor et al., JGR-Earth Surface, 2016). Also, it would help to mention this point in the description of the procedure itself on pg. 7. In the suggested new runs above, the model's basal frozen areas will prevent the beta-adjustment procedure from fully reducing the metric dH to zero (and dV). This can be assessed in the new results.

With simple adjustment procedures (as here, and in PDC12), there is a valid concern that the problem is under-determined, i.e., there are more adjustable parameters than observed constraints, so errors due to one parameter may cancel errors in another parameter or in the model physics. Multiple combinations of Ef and beta(x,y) can produce the correct ice thickness H at a given point, and this is compounded by possible errors in model ice temperatures, both basal and internal (which affect ice rheology). One alternative for this study would be to fix all ice temperatures at some best-fit or at least modern spun-up state. That would (i) reduce total integration times for the procedure because of slowly varying ice temperatures, and (ii) somewhat alleviate concerns of under-determinedness.

Another possible way to improve the underdetermined aspects would be to quantitatively compare with observed surface velocities (as done qualitatively in Fig. 8 and pg. 12, lines 12-17, see comment below), and somehow combine that comparison automatically into the adjustment procedure for beta(x,y) and Ef. This is just a suggestion for future work (not for this paper!), and connections could be made with other optimization techniques that fit to observed velocities (pg. 2, lines 24-25). Another step for future work could be to add a regularization term for beta(x,y) (Pattyn, The Cryo, 2017).

Other specific comments:

pg. 2, line 10, regarding "Three main classes of initialisation techniques have been developed:". Some of the text on this page blurs the distinction between initial conditions (model variables at start of integration) and boundary conditions (externally prescribed quantities). Techniques #1 and #2 discussed on this page are intrinsically concerned with initialization, but I would argue that beta is a boundary condition, and procedures to adjust it are a distinct type from #1 and #2. (For instance, #3 could first be used to produce a map of beta, and then #1 or #2 could be used with that map to produce an initial model state).

pg. 2, line 30, or elsewhere: Note that, as well as PDC12, Pattyn (The Cryo., 2017) applied the method in his Antarctic model, using it both with Weertman sliding (as here) and Coulomb friction laws. Also note that linear sliding (n=1, Eq. 2 here) is not a requirement, and the procedure can be applied essentially as is to non-linear sliding (n>=2), as in the above papers).

pg. 2, line 29-30. The preceding text on this page mentions disadvantages of methods # 1 and 2. Disadvantages of the simple inverse method could also be mentioned here: (a) there are (probably) cancelling errors in the model physics hidden by errors in the basal coefficient map, and (b) the method as in sections 3 and 4 cannot fix ice thickness errors where the bed is frozen.

pg. 4, line 5: In most places, beta is appropriately called a "basal drag coefficient", i.e., larger for stickier beds, smaller for slipperier beds. Here it is called a "basal sliding coefficient" which suggests the opposite sign. To help readers, check that "drag" is used throughout.

Pg. 7, Eq. 5: ... + Uˆsli is in error, I think, should be ... + Uˆdef.

pg. 7, Eqs. 3-7, and Fig. 4: After careful reading, I think I understand the procedure details, but am not sure. First, it would help to state earlier whether NBiter, NByear and NBcycle are years, or number of iterations (on pg. 7 around line 18; it is done at top of pg. 8, but earlier would help). As a suggestion, a numbered list of sentences might help to communicate the procedure, something like:

(1) Eqs. 3-7 are applied at the end of every model timestep, adjusting beta iteratively for the next timestep. The model is run in this way through NBiter years.

(2) The model is then run in "free" mode, i.e., with beta unchanged from its state at the end of (1), through NByear years.

(3) Steps (1) and (2) are repeated NBcycle times.

(4) Finally the model is run for an additional 200 years in "free" mode with beta unchanged.

I am not sure if all the above is correct, especially step (4). Possibly the extra 200 years is run after every cycle of (1) and (2), i.e., as part of every NBcycle cycle. That seems to be implied by Fig. 4, because the upper black arrow for the NBcycle cycle includes everything including the 200-year integration. But if that were the case, it would be puzzling because it would be the same as tacking 200 years onto every NByear integration (my step (2)), i.e., just increasing the value of NByear by 200 and having no final step (4).

pg. 7, Eqs. 6 and 7: What if Ucorrˆsli in Eq. 6 is zero or negative, and so yields infinite or negative beta's in Eq. 7? Physically this would occur when the internal deformation velocity alone is greater than the required total velocity, so the sliding velocity would have to be negative. This is presumably handled by imposing maximum limits on beta, as mentioned later on pg. 15, line 7 (occurring in the Greenland interior where Ef is too high). It would help to describe the use of maximum (and minimum?) limits on beta in section 3 as part of the procedure.

pg. 10, Fig. 6, and pg. 11 line 10 to top of pg. 12. In my opinion the ice volume trend dV/dt is not fundamental. In the new suggested runs with lower Ef values (see main point above), I think the convergence of dV and dV/dt towards zero would be smooth, and the size of dV/dt would just indicate how far along (how many NBcycle's) the procedure has been run. If that is true (bearing in mind the caveat related to basal frozen areas above), then the final dV/dt can be made as small as needed simply by continuing the procedure longer (for instance to provide a near-equilibrated initial ice-sheet model state for subsequent experiments).

pg. 12, lines 12-17. Regarding Fig. 8, it might be worth pointing out that if ice thicknesses are correct, and if the surface mass balance is realistic, then for an ice sheet in equilibrium, total velocities must be correct. So a comparison with surface velocities is, in principle, just a test of the model's split between total and surface velocities.

pg. 11, Fig. 7: The narrow (red) bands with too thick ice around southern and central margins, where flow is in deep valleys and fjords through coastal mountains, are similar to errors in PDC12 over the Transantarctics. The discussion there about under-resolved bed temperatures may be relevant here, and a modified Tb based on sub-grid bed roughness may be a possible solution. (Related discussion is on pg. 17, lines 30-33).

Technical points:

pg. 2, line 13: should be "references".

pg. 3, last line 32: perhaps should be < 250 m, not > 250 m.

pg. 4, Fig. 1(a) caption, and also pg. 8, lines 7-8: Units for local surface mass balance should not be Gt yr-1, should be mass per area per time (?).

pg. 5, line 7: Maule et al. (2005) has geothermal heat flux maps only for Antarctica, not for Greenland, I think.

pg. 5, line 10: Should be Fig. 3a, not Fig. 2a.

pg. 6, Fig. 3a. Just for interest, where do the finely spaced N-S lineations in basal drag coefficients in western Greenland come from, in the GRISL1 ice2Sea simulations?

pg. 7, line 19: change to "let the model freely evolve".

pg. 9, Fig. 5: To be consistent with pg. 8, line 3, the labels in the key in the top right hand corner should be "NBiterˆ20 - NByearˆ50", "NBiter_20 - NByearˆ100", etc., (where ˆ means superscript). Same for Figs. 6 and 7. Also, for consistency throughout, use either NB... or Nb...

pg. 9, line 11: $\sim$10000 Gt: It looks more like -12000 to -13000 in Fig. 6a.

pg. 10, Fig. 6 caption: It seems a bit confusing to have total volume in Gt, and total ice volume trend in mm yr-1. (Presumably the latter is an average over all ice surfaces). It may be clearer to have the latter in Gt yr-1.

pg. 11, Fig. 7 caption, last line. Nbcycleˆ4 should be Nbcycleˆ5 or Nbcycleˆ7, I think, from Fig. 5.

pg. 14, Fig. 8 caption: Is there a reference for this RADARSAT surface ice velocity map?

pg. 16, line 4: Change to "allows us to...", or "allows the deformation to decrease and thus..."

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-322, 2018.