Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-322-RC2, 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.

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Received and published: 23 March 2018

SUMMARY

This paper presents a detailed study of a proposed method for providing optimized initial conditions for ice sheet models. The method attempts to formalize ad hoc approaches proposed and applied in a number of previous studies. Because the method does not use a formal PDE-constrained optimization framework (hence the description as "ad hoc"), it can be expected to be applicable to, and potentially used by, a wider range of ice sheet models (e.g., adjoint-based methods are not required for calculating gradients and minimizing cost functions).

In the manuscript, the authors do a generally good job of 1) carefully explaining the method (although some confusions remains in parts – see below), 2) interpreting how and why the method works, 3) demonstrating the overall success of the method as applied to a realistic Greenland ice sheet application, and 4) exploring the sensitivity to various aspects of the method. Overall, the method shows promising results and the authors are honest about its shortcomings.

While I have some possibly significant points for the authors to consider and address in revision (noted below in more detail), overall this paper is interesting, well written, presents significant and useful findings, and clearly falls within the scope of GMD.

MAJOR COMMENTS

Where applicable, page and line numbers in comments below are referred to as "x, y:", where x = page number and y = line number.

1,11: "spin-up parameters" – this terminology, "spin-up" and "parameters", is confusing, and used throughout the paper. "Spin-up" is first referred to as an existing, standard method for initializing and ice sheet model (on p.2), then later it is used interchangeably to describe the new method described here. I think the two should be clearly distinguished throughout the paper. Similarly, "parameters", unless clearly distinguished, are generally going to be thought of as belonging to the dynamic ice sheet model (e.g., the sliding coefficient is often referred to as a tunable "parameter"). The method proposed here is really more of a nested iteration, and some coefficients used to specify the number of iterations that take place in each loop (more comments on this below). Starting on p. 4, section 3, it seems like it might make sense to refer to this as something other than a "spin-up" method, which has historical associations with your "free spin-up" description. Call it an iterative minimization, or something like that?

2,10-30: Here, methods 2 and 3 are discussed as distinct from one another. But in reality, does anyone ever do just 2, or do 3 without doing 2 first? It seems like these are most often combined into a single method: use a fixed topography to spin-up the temperature (and maybe also the velocity field, so that the temperature and velocity are internally consistent), and then use that temperature field along with an inverse method to calculate velocities that better match observations.

4, section 3: Somewhere in here, you might discuss or mention the work of Perego et al. (2014, JGR Earth Surf., 119, p.1894), which has very similar overall goals to that discussed here, but using a formal minimization framework (e.g., your Figure 2b is analogous to their Figure 1, although the timescales are different).

6, 4-5: "...performance in terms of trend and error in simulated ice volume compared to observations". While you do somewhat address the mismatch between observed velocities and /or ice flux later in the paper, I think it would make more sense to bring it up here. Or even earlier, when you first discuss the metrics you are going to use here. I kept wanting to see some discussion on that and felt like it was being ignored. It would have helped if you had stated early on that you were going to look at this topic later on in the paper.

6, Figure 4: I found this figure a bit confusing. A couple of ways that might help to improve it include 1) tying it to the discussion in the text more clearly (and vice versa – refer to the steps in the figure when you are describing them in the text) and, 2) drawing it as a set of nested loops instead of a left-to-right flow chart. It seems to me like what you describe is two back-to-back loops (Nb_iter followed by Nb_year) that both sit inside of a larger, outer loop (Nb_cycle). A different figure might capture that better (it could still include parts of what you have here).

7, steps 1 and 2: Note that what you describer here in steps 1 and 2 is essentially identical to the iteration described in Price et al. (2011; PNAS, 108(22) – see "methods" and SI for more details), except that they are using observed and modeled velocities rather than observed and modeled ice thickness to adjust the sliding coefficient). Also, it took me a while to figure out exactly what "Nb_iter" was. It's not immediately clear why this is >1 (i.e., what are you iterating on?). Eventually, I guessed that you are allowing

the new sliding coeff. and the model velocities to come into some sort of equilib. with one another. If that is true, you should state it explicitly!

Figures 5 and 6: The labeling of the legend should be changed here to "Nb_year" rather than "Nb_iter". It's too easy to confuse what you are varying here as currently labeled. It takes careful reading to understand that Nb_iter is actually held fixed while you vary Nb_year. You could use Nb_year instead and just mention in caption that the value of Nb_iter is the same for all.

End of p.9 to start of p.11 – It took me a few readings to understand the explanation here. I think it could be written a bit more clearly. The point is that the volume metric needs to be used carefully because it cannot discern compensating errors (overall too thin in the interior and too thick at the margins cancels out and looks like a good match), and thus one either needs to look at the spatial pattern of thickness errors or include some other metrics.

12, 12-17: This discussion of the model fit to observed velocities is appreciated. I think it would make sense to mention much earlier in the paper that you are going to look at this. The lack of discussion of the importance of getting both the thickness AND velocity state and trends correct (and hence the flux correct) early on in the paper made me wonder how useful the method could be. At the same time, while the fit to observed vels looks good by eye, I think it would be appropriate to give a slightly more quantitative measure for how well the final initial condition matches observed velocities (e.g., RSME of speed). I don't think a relatively poorer match to the velocities (relative to the thickness) really speaks poorly of the method as there are times when having a near steady-state initial condition might be more important than matching the velocities better. But overall, it would be good to know how easily a good match to velocities follows a good match to the thickness / volume.

Section 4.2.4: Do you have any physical explanation for the lack of sensitivity to the value of Nb_iter, or why Nb_iter is better at smaller values?

Figure 8: I am actually quite surprised to see that this method somehow “gets” the NEGIS in the modeled velocity field. Can you confirm if this is still the case when you start the iteration from a uniform value of beta? It seems like it would be very hard for the iteration to form this subtle feature in the model without some direct connection between the sliding coefficient and the velocity field (the topography is too subtle and it doesn’t seem like the metrics being used could possibly discern the necessary variations in the sliding coefficient based on the subtle changes in ice thickness). I’m curious if it is somehow a “relict” feature that exists primarily because of the initial sliding coefficient field you started with (which, for ice2sea, may have been tuned somehow to reproduce the NEGIS).

16, 5.2: I was also glad to see this section, as it seemed like a logical next step given the limitations of the method for adjusting the ice speed and ice thickness in the interior. However, I was expecting at least maybe the suggestion that one could combine the method of tuning the sliding coefficient with a similar method for tuning Ef where the ice was determined to be frozen to the bed. It seems like the exact same method could be used to iterate on the value of Ef that is used to iterate on the value of the sliding coefficient. Have the authors thought of trying this? It seems relevant to at least speculate on, or comment on as a logical next step.

17, 5-9: It would be interesting to see a 1:1 plot of the sliding coefficient values for the two different initial conditions. This would be a nice visual way of convincing the reader that there really is little sensitivity to the initial value of the sliding coefficient. As noted above, it would be very nice to see a comment here on whether or not the NEGIS is still an “emergent” feature when starting from a uniform sliding coefficient.

Summary and Conclusions:

There is the suggestion here that the method could work better at higher resolution. However, I don’t think this will actually be the case. This is because this method can only adjust the value of the sliding coefficient point-by-point; each grid point is ad-

justed independently of every other one. Once you get down to a grid spacing of a few ice thicknesses or less, this will cease to work very well, because the change in sliding coefficient at one grid point will lead to changes in ice speed at that point AND at neighboring points, via horizontal stress gradients. When this happens, the iteration ceases to make further improvements because it doesn’t have a way to avoid the “noise” that local adjustments cause at neighboring points (I have some experience with this problem, based on the similar iteration described in Price et al. (2011; PNAS paper prev. referenced). This is one reason that, at high resolution, it starts to become difficult to use ad hoc methods like this for very precise tuning and one may need to turn to more formal optimization methods.

Some speculation on future directions would be appreciated. For example, could you also include a metric on ice velocity, so that your iteration was scored by the weighted mean of the fit to thickness AND the velocity? This would also be a good place to speculate on iterating on the value of Ef in areas where the bed is frozen.

MINOR COMMENTS

1,6: “to infer reliable initial conditions of the ice sheet”. This is not really true. Most inverse methods applied to ice sheet models currently only really “work” well if you are only interested in a snap-shot of the ice sheet velocity. Without other considerations, you might get a model snap-shot that does a great job of mimicking observed velocities, but it will likely suffer very badly from the problem you aim to address here (that is, large, unphysical transients).

1, 11: “. . . to minimize errors in sea-level projections”. This is misleading, as it’s not really one of your criteria here. We can’t know that this will minimize errors in SLR projections can we?

2,1: Be explicit – the “unrealistic evolution” you are talking about is large, unphysical transients in ice thickness.

2,5: “GrIS characteristics” -> GrIS “state”?

2,5: “the major source of uncertainty” -> “a major source of uncertainty”

2,6: the vertical temperature profile is not part of the “basal properties”, as this sentence implies (probably just poorly written)

2,15-18: “significant mismatch . . . topography”. I would use “state” here insteady of topography, since it is much more than just the topography (velocity, flux, etc.). For “Such spin-up methods” it seems relevant to mention why only low cost models can do this, because the spin up is order 10,000-100,000 yrs long.

2,22: “inconsistencies between . . . “. You could be more explicit here. The problem is that the modeled flux divergence is nowhere close to being balanced by the sum of the surface and basal mass balance terms.

3, 10: Clarify that hybrid model refers to the momentum balance?

3,11: “velocity fields” -> “ice dynamics” ?

3.15: and equation 1 – clarify that U_bar is a 2d vector field?

3,20-21: Clarify that the SIA and SSA solutions are summed heuristically, and point to a reference where you describe what that heuristic is?

3,23: “linear till” -> “linear viscous till”; note that there’s a missing assumption here (in eq. 2) about the thickness of the till layer being uniform everywhere.

3,29: What is value of Ef used here?

3,32: The calving criterion is not clear as written. Do you mean that everywhere floating ice is <250 m is thickness it is assumed calved?

4,6: “either the simulated . . . velocities or the ice sheet geometry” . . . what above both? See comment above about Perego et al. (2014) paper.

5,2: “Our choice is motivated by . . . sea-level rise.” add, “without that sea-level rise sig-

nal being contaminated by unphysical transients from the initial condition.” (or something to this effect)

5,9: It’s not clear if you hold the temperatures fixed during the iterative process discussed here.

8, 5: 4.1 “is the spin-up needed” – again, suggest using something else to describe this (“iteration”?) rather than spin-up, to avoid confusion with the common understanding of spin-up.

12, 8: “RMSE” -> “thickness RMSE”

Table 1: I assume the commas are analogous to periods in the numbers listed? Is this standard? Should periods be used instead?

The paper is reasonably well organized (aside from some suggestions noted above) and written. There are a fair number of minor edits and corrections that could be made, related to English language use. I do not point those out here explicitly but instead suggest the authors enlist a native English speaker / writer to provide a careful editing before the submission of a revised version.

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