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The Cryosphere Discussions

Interactive comment on "Greenland Ice Sheet contribution to sea-level rise from a new-generation ice-sheet model" *by* F. Gillet-Chaulet et al.

Anonymous Referee #3

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General comments

General Note: I reviewed a previous version of this paper and I think The Cryosphere is a more appropriate venue for the publication of this paper. However, because that version of the paper was very similar to the present version, many of my comments and concerns about the paper remain the same.

The authors present prognostic simulations of the Greenland ice sheet over the next century conducted using the new (next?)-generation ice sheet model Elmer-ice. An introductory review of current generation models and their limitations serves to motivate the new developments of Elmer-ice reported on here. Note that this discussion should

really be explicitly aimed at circa AR4 models, since there are currently a half dozen or so models that could also be called "next generation". The authors also discuss Elmer's capabilities, their initialization procedure, and the model output from a number of prognostic experiments accounting for perturbations to both surface mass balance and ice dynamics. The output from these perturbation experiments are presented as changes in ice sheet mass loss over time (and the resultant change in global sea-level) relative to a control run for which climate (surface mass balance) and dynamic forcing (basal sliding coefficients) are held steady at their initial values.

The capabilities of the model showcased in this paper are impressive and overall the model represents a significant technical achievement. The combination of meshing capability, solver capability, and data assimilation capability are all first rate and are currently matched by only one other modeling group in the world today. While the experiments reported on in the paper are certainly useful for probing the parameter space of future ice sheet behavior, it seems like that fact could be stated a bit more explicitly in the paper. Instead, it seems like the authors are trying to "sell" the results from the experiments as representative of particular "realistic" future scenarios for Greenland's evolution. For example, the primary thing we learn from the paper is that discharge from the ice sheet increases depending on how slippery the ice sheet bed might become in the future. This in itself is not surprising. But rather than trying to sell the results from these rather arbitrarily forced experiments as realistic possible futures for Greenland – which is very difficult, because the perturbations are largely arbitrary (e.g. how does a doubling of sliding everywhere correspond to some kind of realistic physical forcing, like a change in meltwater lubrication at the ice sheet bed?) – why don't the authors take the more honest and reasonable approach of treating the experiments as a sensitivity study designed for probing the possible bounds of future ice sheet mass loss and sea-level rise from Greenland?

Another example is the conclusion made in the paper (and re-stated in the abstract) about the "increasing dynamic perturbation" experiment, whereby the model is forced by a decrease in the value of the basal traction parameter, everywhere, by 10x over one century. Since, as the authors admit, such a scenario is highly unlikely, why not argue that these results demonstrate exactly how drastic (and probably unrealistic) a dynamic perturbation is necessary in order to maintain long-term increasing rates of mass flux from the ice sheet? To me, the results of this experiment can be used to argue that such a dynamic change is highly unlikely, and thus can be used to place an upper bound on future dynamic mass loss and sea-level rise from the ice sheet. Further, it strongly argues against doing things like extrapolating the increasing rates of mass loss from the last decade into the future, because it demonstrates just what kind of dynamic forcing is required to justify doing so (that is, very unrealistic forcing). This conclusion seems equally or more important to me, and much more believable, than trying to convince the reader that the results represent a reasonable potential future for Greenland.

A related complaint is the statement from the abstract (and similar statements sprinkled throughout the main text) that "By conducting perturbation experiments, we investigate how current ice loss will endure over the next century". The implication is that the model initial condition is such that it captures current rates of observed ice sheet mass loss (thus, the model initial condition captures current-day transients) and that the applied perturbations are physically reasonable and/or constrained such that they are representative of what we expect the ice sheet to be subjected to over the next century. Neither of these is true, however, in terms of what is presented and discussed in the paper. Again, there seems to be plenty to learn here if the results are presented more honestly as coming from perturbation experiments aimed at bounding the likely future sea level rise contribution from Greenland (i.e., it seems unlikely that we can expect dynamic mass loss of >114 mm by 2100, because now we have some idea about what kind of dynamic forcing is required in order for that to happen).

The issue about basal boundary conditions resulting in mass loss of $\sim$10% of the total discharge through the basal boundary is a bit disturbing. This is not a small number of

mass that is not being conserved. I am not an expert on finite elements but I do know that there are methods for enforcing conservation both locally and globally. If the mesh is at fault for some reason, shouldn't a more high fidelity mesh be used? In general, this issue seems to be swept under the rug a bit too much.

Specific comments

p.2789

line 7: "... only represent rapid ice flow in an approximate fashion." Be specific about what you mean here. What does "approximate" mean? What is the specific problem? Resolution? Wrong gov. equations? Simplification of boundary conditions? All of these?

Line 11-12: The statement that one needs to be solving Stokes is not necessarily true. Depends on lots of things, like resolution, bed roughness, etc. In many places lower order approximations are appropriate (e.g. even SIA is ok in the interior).

Line 18-19: "...if destabilizing processes continue ...". This is misleading relative to what is presented in the paper. Yes, this was the result of one of the experiments but by your own admission that experiment is unrealistically extreme. This makes it sounds like increasing rates of mass loss into the future are not unlikely but I think the more reasonable interpretation of the experiment results is that the are unlikely.

p.2790

line 13: water filling crevasses does not soften them, the latent heat from water does.

Line 24- . "The lack of skill of current generation ..." This discussion is a bit dated and disingenuous, since there are a number of new models now that have shown good skill in reproducing observations of rapid change (and these models have been published on as well).

p. 2791

(i) – (iii): It sounds like you are condemning all approximations to Stokes (e.g. even 1st-order, LIL2, etc.) as inappropriate. This is not true, as they are still valid approximations over large areas of the ice sheets. I agree that Stokes is required in some areas, but not everywhere all of the time.

Line 25: "We present the first model ..." Is this statement entirely true? ISSM has many of these same capabilities.

Line 26-27: The reference to Seroussi et al. is out of place or incorrect here. The anomalous surface mass balance implied from the initialization procedure used here is only partly due to errors in ice flux data (as discussed in Seroussi et al.). The bulk of it is due to the fact that your initialization procedure is tuning a single model field, the basal sliding parameter, when in fact there are many other inconsistencies between your initial model state and the velocities you are trying to "fit" (e.g. incorrect internal temperatures and ice softness).

p.2792:

Section 2.1: Logically, it seems like the governing Stokes equations should be presented first, since they are generic, followed by the constitutive law, which is not (currently, this order is reversed).

Section 2.1: Somewhere in here, mention where the non-linearity comes from explicitly. That is, from the strain-rate dependence of the viscosity.

Line 20 - : Save the information on how you get the temperature field for the discussion of initialization? It seems awkwardly force in here. It would suffice to say that you use a constant temperature field for A(T) and that you discuss the details further below. Also, it would be good to state why it is justified to hold the temperature field fixed over time. Presumably because it will change little on a $\sim$100 yr timescale (which I agree with, but it should be stated).

p.2793:

line 23-24: The equation for the boundary condition should be written out instead of just given in writing.

p. 2794:

line 3-6: Overall, I don't think enough information is given as to why the flux is not conserved at the base (e.g. why the no-penetration bc cannot be enforced). There are methods to do so, why aren't they implemented here? If the amount of mass loss was trivial, it might not matter, but 10% of the discharge flux is a pretty large number.

Line 6-9: clarify that you are applying "hydrostatic pressure" as a b.c. at the ice-ocean lateral boundary.

Line 17: The "metric tensor" is mentioned in terms of constructing the mesh but you haven't really explained what it is or why / how it is used. My impression is that the Hessian of the velocities is used as a weighting factor for deciding where to focus resolution, and maybe the description would be more informative if it was simplified to a statement of that nature.

In terms of the meshing, it is not clear here if YAMS does the mesh optimization or if that is something the authors came up with. A few more references on the meshing might be adequate to cover questions like this.

p.2795: line 16-17: Again, the ice flux divergence anomalies are not entirely the result of the issues discussed in Seroussi et al. They are largely the result of the fact that your initial state, even though optimized to match observed velocities, are not in equilibrium with the current ice sheet geometry because of, e.g. incorrect or missing model transients, incorrect/unknown rheological properties, etc. By referring to Seroussi et al., it sounds like you are arguing that the mismatch between the flux divergence and the surface mass balance – resulting in very noisy and unphysical vertical velocities at the surface - is entirely due to uncertainties in the geometry data for the ice sheet. This is not true.

p.2797:

line 5: Not clear to me why you are discussing the ice sheet surface topography here.

p.2798:

line 4-5: When discussing the regularization, note why it is necessary; to enforce a "smoothness" constraint.

p.2799-2800:

Section 3.1.4 - 3.1.5: Note somewhere in this section whether or not the optimized values of Bˆ2 are consistent with the basal thermal conditions (i.e. does sliding take place if and only if T = T_pmp?).

p.2801:

(ii) Note that optimization on multiple parameters simultaneously (e.g. a 3d enhancement factor) could improve the fit between model and observations.

p.2802:

line 14-16: Again, I think you are assigning to much blame for the unrealistic surface vertical velocities (that is, the result of the flux divergence) to the uncertainties in ice thickness, when most of them are due to the uncertainty in other parameter values that result in, e.g. and incorrect vertical velocity structure.

Line 19-23: The problem discussed here is probably also due to the fact that the thicknesses for many outlets are poorly known and underestimated (i.e., they need to thicken up in order to evacuate the ice flowing to them because, in reality, they actually are thicker (deeper) than the current geometry data allow for).

p.2803:

line 9-10: somewhere here, tell us where your surface mass balance comes from (Ettema et al?).

line 10: The ice extent (lateral I assume?) does not evolve much ... but can it? Isn't the mesh extent fixed ahead of time? Can the margin advance and retreat freely? Only retreat (e.g. formerly ice filled cells become ice free)?

Line 20-23: Again, most of the uncertainties that require the surface relaxation (or alternatively, would require a "synthetic" surface mass balance, given by the flux divergence field) here are NOT due to uncertainties in the bedrock elevations, but due to uncertainties in the model initial conditions.

p.2804:

Section 4.1: Somewhere you need to give appropriate credit to the SeaRISE effort, who designed and organized the data for at least a few of the experiments reported on here. Either reference the website (e.g. for the data), or the publications that have been submitted, or both.

Section 4.1: Note that the in the first beta forcing scenario (halving its value), the change is implemented as a step function.

Line 18-20: But this last scenario IS unrealistic. Decreasing basal friction temporarily and in a few locations (e.g., I doubt that anyone things that all of Greenland is going to surge all at the same time) is a lot different than decreasing it by an order of magnitude everywhere. The more honest way to report this would be as a sensitivity analysis, as discussed above.

p.2805:

line 10-12: As discussed above, the 10% of discharge mass loss through the basal boundary seems substantial and problematic. This is not a small number, and one fundamental requirement for an ice sheet model would seem to be conservation (at least to a much smaller tolerance than this).

p.2806:

line 7-10: I think that what is written here could be interpreted incorrectly. That is, one might read this as implying that one needs to reduce the friction parameter by ∼1/2 everywhere from your initial condition in order to bring your model in line with current observations. Is that what you mean?

Line 15-17: "This pattern is expected . . .". This is not an entirely honest statement, as there is at least as much evidence arguing that excess runoff will have very little effect on ice sheet mass flux (as efficient drainage systems will buffer the impact of increased melt on basal sliding).

Line 20-: See discussion above about how this experiment is interpreted and reported on, which I think could be made substantially better (and more honest).

p.2807-2808:

The section describing the comparison of observed vs. modeled rates of surface elevation change doesn't add very much and seems like a distraction. I would suggest removing it from the paper.

p.2808:

line 5-9: You are really only partially satisfying one present-day initial condition, that of approx. matching the observed velocities. I'm not saying this is trivial, but there are certainly other conditions you are not matching (e.g. geometry, observed rates of change, internal temperatures, etc.). You might be more specific about what you mean here by "present-day conditions".

Line 14-16: We already know that surface mass balance AND discharge will govern the ice sheet's future evolution (this was a conclusion from AR4 afterall), so this is a somewhat trivial conclusion to be making here.

p.2808-2809, line 25-line 2: It's not clear to me how the "extrapolation" was done here and how it is being compared to the value from Price et al. I think the latter value they are referring to (∼90 mm of SLR by 2100) contains approximately equal parts SMB

and dynamic discharge. Again, unclear if/how these two values should be compared, or how the extrapolated value reported on here is obtained.

p.2809, Line 2-8: The "continually increased" sliding perturbation scenario is discussed here as if it is realistic. Again, it seems like the more honest way to report these results is to note that, in order to come close to the Pfeffer et al. upper bounds (that are quoted here), you have to do something fairly crazy and unrealistic to the model (decrease friction everywhere by an order of mag. over a century). Thus, both the Pfeffer kinematic arguments and the modeling conducted here are in approx. agreement on what might be an upper bound for dynamic mass loss from Greenland over the next century. This seems like a more honest way to discuss these results (as opposed to hinting that this kind of behavior might actually be expected).

Technical corrections

The word "enhancing" is used often throughout the paper. It would be better to use a specific word, e.g. "enhancing . . . sea-level rise" —> "increasing . . . sea-level rise".

p.2789

Line 12: "unstructured mesh" —> "variable resolution unstructured mesh" ?

p.2790

line 8: "The flow of an ice sheet . . .". I think this should be a new paragraph, but at the same time, this statement doesn't seem relevant here. Shouldn't it be moved to where the gov. equations / model are discussed?

p.2791

(iii) "The most uncertain parameterization . . ." —> "The most uncertain process"

line 18: provide a general ref. for "Elmer/Ice code".

p.2792:

Suggest a new paragraph at the end of this section (before section 2 starts). For example, "We first discuss the governing equations, the model, and our meshing strategy, followed by a discussion of our initialization procedure. We then discuss the model perturbation experiments and the result of those experiments" (or something like this).

p.2793:

line 22: "accumulation: —> "accumulation rate"

p.2794:

Line 6-9: clarify that you are applying "hydrostatic pressure" as a b.c. at the ice-ocean lateral boundary.

Line 19: "YAMS" = . . . ?

Line 24-25: provide a link or ref. for the searise dataset used here?

p.2795:

line 10: Omit "full Stokes" (the inverse methods in the discussion don't need to be limited to only Stokes models).

Line 12: "basal friction field" —> "basal friction parameter" ? You aren't constraining the friction field with your initialization procedure, you are constraining the parameter relation basal velocity to basal traction/friction.

Line 20: "main outlets" —> "main outlet glaciers"

Line 23: Here you refer to the "basal friction coefficient", which is how it should be referred to elsewhere in the paper.

p.2796:

line 15: the notation "d_B J_0" is not clearly described.

Line 16: the symbol "|.|" is not used with equation 10 but with equation 9 (and should

be discussed after equation 9, not 10)

p.2797:

Line 18: "To avoid unphysical negative . . ." —> "To avoid unphysical (e.g. negative) . . ."

p.2799:

line 1: suggest rewriting sentence as, " . . . using a spatially varying step size rather than the fixed-step size used in the original gradient descent algorithm of A&G (2010)."

Line 10: "reverse communication MODE."

p.2800:

line 2-3: Provide a reference for the "L-curve" method?

p.2801:

line 8: "twin experiments" is awkward. What does this mean?

(iii) "Unsufficient" —> "Insufficient"

(iii) " . . . and of the data as, for example, the ice thickness . . .". This should be a new topic (item iv?) as it doesn't have anything to do the other topic in item (iii).

p.2802:

line 11: "essentially" —> "especially" ?

line 13: clarify that by "climate" here you mean "surface mass balance"

p.2804:

line 3-4: Even though one of the two methods can't be conclusively argued for, note which one you use for the rest of the simulations reported on here.

Line 9: clarify that "climate forcing" here is only surface mass balance, since you are holding temperatures fixed.

Line 18-19: Instead of “enhanced”, be more specific and just say that beta is reduced linearly by one order of magnitude over 100 yrs.

Line 22: “thereafter” —> “hereafter”

p.2805:

line 24: “other retro-actions” is awkward. What does this mean, other feedbacks?

Tables / Figures

Table 3: Suggest adding a column of currently observed ice flux for these same outlets?

Figure 1: This figure should be made bigger. It is extremely difficult to discern any of the details of the flow structure (e.g., in order to compare them). Also, the colorbar labels are essentially unreadable.

Figure 2 (caption): “. . . with the Robin inverse method. Colored boxes show close-up views for various outlet glaciers of interest.”

Figs. 4, 5: Can’t read the colorbars here. Use white letters and larger font size?

Figure 9: Again, can’t read the colorbar here (too small / blurry).

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