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The Cryosphere Discussions

# *Interactive comment on* "Effect of higher-order stress gradients on the centennial mass evolution of the Greenland ice sheet" *by* J. J. Fürst et al.

**Anonymous Referee #2**

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GENERAL COMMENTS

Furst et al. summarize an ambitious effort to implement a number of different lower-order approximations to the nonlinear Stokes equations for ice flow within a single modeling framework, and use those different models to simulate mass loss from the Greenland ice sheet for several dynamic perturbation experiments. The resulting spatial and temporal patterns of mass loss are then compared and contrasted between models, and for different grid resolutions, in order to better understand the differences in ice sheet evolution that result from the different dynamic approximations and grid resolutions used. There are two main conclusions from the paper, (1) models with increasing levels of sophistication at including membrane stresses increase the rate at which marginal perturbations propagate inland (while decreasing the overall timescale



for the ice sheet to adjust to the perturbation) and, (2) overall, the long-term (100 yr timescale) mass loss from the ice sheet predicted by models that include membrane stresses differs only slightly from the mass loss predicted by models that do not include membrane stresses. The first conclusion is probably not that surprising, as this has been pointed out as a deficiency of standard "shallow-ice" models numerous times during the last $\sim$5 yrs (e.g., in the last IPCC report). The latter conclusion, which the authors attribute to the fact that the ice sheet response to dynamic perturbations is dominated by the diffusive inland propagation of changing ice sheet geometry, is a bit more surprising and potentially of more significance (e.g. with respect to current model development efforts that are inching continually closer to using Stokes models as the norm).

In general, this is an interesting and worthwhile study with conclusions that are relevant to the large-scale, prognostic modeling of ice sheets (e.g. it begs the question, are shallow-ice models "mostly" adequate for simulating the large-scale, centennial scale evolution of ice sheets?). There are a few technical issues of concern, which are discussed further below. These can probably be addressed by the authors during the revision stage. Of somewhat more concern to me is that the paper is quite long and difficult to follow at times, containing repetition and what seems to be a lot of unnecessary equivocation. The writing is often very verbose, obscuring the meaning of a sentence. For example, from the conclusions section:

"The inclusion of membrane stress gradients in various ways in fact reduces the volume response by 20% at most from different inland propagation of marginal perturbations."

This is followed by,

"Models that include direct horizontal coupling are capable of attenuating perturbations faster by instant upstream propagation."

I understand the second sentence, but not the first. Is the first just another way of stating the second? If so, is the first even needed? There are numerous similar examples throughout the text and after a while it became a bit maddening to try and sort out what was important to read from what was not. The paper is not poorly written, but it does seem to be "over" written in many places. My suggestion for fixing this would be to have a native English speaker go through the paper carefully and subject it to some very heavy-handed editing. I suspect the length could be reduced by 20-30% while simultaneously making the paper much clearer and easy to read.

I found the way that the different models were categorized confusing and non-intuitive. For example, the short-hand labels (DR SIA, ME HO, etc.) seemed backward to me. Shouldn't the overall stress approximation used by the model come first, followed by the basal boundary condition approximation (e.g. SIA DR, HO ME, etc.)? More importantly, some of the combinations seemed nonsensical to me. I believe that the "SR SIA" variant includes the shallow-ice approximation within the ice but uses a sliding law requiring the full 3d, higher-order solution. Certainly one *could* do this, but why would one want to? Similarly, one *could* simply use the driving stress in the basal boundary conditions for a higher-order model (DR HO), but if you've gone through all the trouble of creating a higher-order model, why would one then skimp on the basal boundary conditions? One could argue, as in the paper (although not very clearly), that the motivation is to parse out the different aspects of the horizontal coupling (i.e. how much is due to coupling in the sliding vs. how much is due to coupling in the internal deformation), but my impression was that the conclusions when trying to do so were a bit muddled. I would have been happy to see the discussion limited to models that are actually used in practice, which I believe are DR SIA, ME SIA, and SR HO. This would also greatly simplify the terminology, since one could refer to the models as simply "shallow ice", (something like) "Bueler and Brown" (ME SIA), and "first-order" (or "Blatter-Pattyn"), which are the more commonly used descriptions for these same models.

I am confused about the lateral boundary conditions implemented in the model and in the experiments discussed. My understanding from what is written is that, for all mod-



els, the velocity at the margin of the ice sheet is specified according to a shallow-ice like solution (that is governed by the local geometry only). This is explained as an attempt to maintain a similar boundary forcing for all models. But having spent too much of my own time implementing more complicated lateral boundary conditions in models (i.e., that for a grounded or floating marine margin), I'm immediately suspicious that this choice was made simply because the alternative can be quite difficult (and in some cases, quite touchy and non-robust for simulations using real geometry data). Further, I am concerned that the somewhat similar model responses shown here could be an artifact of this overly simple treatment of the lateral boundary conditions; the velocity at a grounded calving front or freely floating ice tongue is going to be dominated by horizontal normal stresses, not vertical shearing stresses. I can imagine how the latter might "damp" the response of a marginal perturbation unrealistically. For example, if sliding is increased near the margin by reducing the basal friction, the ice at the margin will accelerate, thin, and flatten. The latter two effects will greatly reduce the marginal velocity calculated using a shallow-ice approximation, but they would not necessarily reduce the velocity by the same amount for ice at a marine margin, which is deforming largely as a result of horizontal normal stresses. I'm concerned that in their attempt to compare "apples with apples", the authors really are comparing apples with apples by having "turned off" one of the important mechanisms that differentiate models with membrane stresses from those that do not (i.e. horizontal stress transmission at and across lateral boundaries). A simple set of test case simulations would put my mind at ease about this issue. That is, one could compare a prognostic simulation using DR SIA and SR HO (or ME SIA) for which the boundary velocities were specified from the shallow-ice model (as is done in the paper if I'm understanding correctly), with a similar simulation in which SR HO (or ME SIA) use a more appropriate lateral boundary condition, for which horizontal normal stress gradients are applied (e.g. the standard stress boundary condition applied at the front of an ice shelf). If the concern is that the two boundary velocities do not initially match one another closely enough, an additional "backstress" term could be applied to the shelf boundary condition to bring the two marginal velocities into closer agreement. Note that this could all be done for a synthetic and idealized computational domain (e.g. 2d vertical slice with flat bed, simplified ice geometry). Because the majority of the dynamic discharge from the ice sheet in these simulations is through marine terminating outlet glaciers, it seems appropriate that at least some of the perturbation experiments apply a lateral boundary condition appropriate for that physical setting. Either that, or the reader should be convinced that the more simple boundary condition applied gives similar or reasonable results (e.g. through some kind of simple comparison like that outlined above).

SPECIFIC COMMENTS

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line 14-18: sync. speed up vs. regionally linked vs. more eratic behavior – these all seem mutually exclusive, but the way this is written it sounds like they ALL apply. Re-write more clearly?

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line 19-20: I think it is safe to say that long. stress grads. are not suspected of leading to efficient horiz. coupling. I think it is safe to say that this is known at this point (from numerous studies over the past ~5 yrs).

Line 20-23: Are you confusing "plane-flow" with "plane strain" or "plane stress" here? I don't find "plane flow" anywhere in the literature, but plane stress or plain strain have specific meanings. Which one is it?

2965

line 5-6: "...long. coupling length is expected to increase ...." Provide some refs for this statement? e.g. Joughin et al. paper on diff sliding laws applied to Pine Island Glacier, Price et al. paper on inland propagation of outlet glaciers and ice streams?

Section 2.1: This section is a bit wordy/long. I think it could be trimmed down quite a



bit.

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line 11-14: Not sure I follow this or why it is relevant here. Why not just state farther up that you are using a Cartesian coord. system.

Section 2.2: Is the model serial? Parallel? Solution methods? Does it pass standard tests, benchmarks? Some of this could probably go in the appendix if necessary.

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line 2-4: "... assessment of the range of dynamic mass loss in the future." Clarify what you mean here, the actual expected range or the range due to the use of different model approximations?

Line 6: The first sentence is not actually a sentence.

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section 3.2, line 24 -: Not clear how the spatial extent for the sliding perturbation is applied. Is it a box? An elevation contour? A speed contour?

2970

line 3: "signal transmission" is awkward and used throughout the paper. Be more specific about what you mean here.

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line 3-5: I think we need some more information for how surface mass balance is incorporated. Is it a spatially varying field that is held fixed in time? Is it a function of ice sheet surface elevation (e.g. through lapse rates)? SMB feedback is mentioned, but it is not clear how or why there should be a feedback between SMB and elevation.

Line 11: doesn't "s.l.e." usually refer to "sea-level equivalent"?

Line 13-15: Because the forcing scenarios are rather arbitrary, I don't think one can really say whether or not the resulting sea level rise is small or not.

Line 19-22: I don't see where the 66 mm s.l.e. in 100 yrs comes from. In equilib. you have 0.66 mm s.l.e. / yr, so multiplying that by 100 yrs gives 66 mm total in 100 yrs. Where does the "doubling" come in? Something is not being explained correctly here.

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line 21-23: Be clear here; more thinning leads to more runoff? "changes" is ambiguous.

Line 27-29: These last two sentences are confusing and awkward.

2974

line 2-3: This is a strange statement to make since it doesn't sound like you are applying a boundary condition that is appropriate for a calving front (i.e., the hydrostatic stress from the water column, as opposed to an SIA velocity, which I believe you apply). This leads to additional confusion about the lateral boundary conditions being applied.

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line 18-21: Again, you are not applying a "marine" boundary condition here (or at least it doesn't sound like you are), so the discussion about "marine terminated periphery" and "calving export" are confusing. If you are using SIA to specify ice flux across a "marine" boundary, you need to show/argue why that is a reasonable thing to do.

2976

line 20-21: "Direct far field signal transmission . . .". I'm not sure what this statement means. What is the meaning of "far field"?

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line 2-3: I'm not sure you've clearly explained why membrane stresses lead to faster attenuation of the perturbation. Intuitively, this is because membrane stresses distribute



the perturbation over a wider area, and thus the perturbation can propagate farther and faster than if it does so just through geometry adjustment, which is slow and limited by the viscous deformation of ice.

Line 24-25: "Yes the response behavior is rather prone to . . ." Wording choice is very vague. Be more specific here. More sensitive? Less sensitive? What?

2980

line 1-17: I'm confused about the resolution discussion. Perhaps a summary would help. Does it matter or not? Surely it must on some level.

2980-2983: The discussion section is very long. I think it could be considerably shortened, especially if it sticks to just covering topics discussed in, and relevant to, the material presented in the paper (e.g. the discussion about the details, differences, and failings of various sliding laws doesn't really add much, nor does the discussion of grounding line retreat, which this model can't do anyway because of the coarse horiz. resolution). Overall, parts of the disussion seem to veer off topic and removing them could make this section quite a bit more concise and relevant to the rest of the paper.

APPENDICES

(A1-A2) should be "del dot u" rather than "del u"; same for "del u" (divergence not grad)

2986

line 20-21: This is the 3rd time you mention something to the effect of "deviations from hydrostatic pressure cause deformation."

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Somewhere here, state more clearly where the nonlinearity comes in; the strain rate dependence of the viscosity.

2988

line 1-13: Add some refs for papers where the background for the "1st-order" approximation is layed out in more detail (e.g. Schoof and Hindmarsh, QJMAM; Dukowicz et al., J. Glac.)

(A9) This is NOT the most general form for the basal resistance, this is a 1st-order accurate version (i.e., this is not a Stokes form of the basal bc, which would be the most general).

(A9) Have all of these terms been defined, e.g. Tau_bi, Tau|| ?

Have you told us somewhere earlier on what "s" and "b" are in all of these equations?

(B3) clarify where this sliding law comes from. Also, more undefined terms introduced here (e.g. u_b, u_bi).

Somewhere in the appendix, the lateral boundary conditions, briefly discussed in the paper, should be discussed in more detail.

FIGURE / TABLES

Table 2: shouldn't the combinations that are not used in the study (e.g. ME HO) be noted here somehow?

Figure 2: The dashed blue lines are not clear in the figure. As noted in the general comments section above, I'm not sure if/why some of these model options need to be shown. I think it might make more sense to limit the figures to model combinations that might actually be used in real life.

Figure 3: Caption and vertical axis label – confusing here about whether the mass balance is increasing or the mass "loss" is increasing in time. Clarify the wording.

Figure 5: Just show the first three rows of figures (i.e. ignore the SIA SR and DR HO combinations)? Consider showing the spatial pattern of thickness change as normalized to the pattern from the SIA model (to make the differences more clear)?



TECHNICAL CORRECTIONS

I made a large number of technical / editorial corrections to this manuscript. However, I hesitate to include them all here, especially considering that one of my primary recommendations for revising this paper is that it undergoes a thorough and careful re-editing. I am happy to provide these technical corrections (by way of an edited .pdf file) to the editor upon request.

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Interactive comment on The Cryosphere Discuss., 6, 2961, 2012.