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Comment

Interactive comment on “Parameterising the grounding line in ice sheet models” by R. M. Gladstone et al.

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Review of “Parameterising the grounding line in ice sheet models” by R.M. Gladstone, A.J. Payne, and S.L. Cornford

General comments

This paper presents a summary of model experiments of grounding line behavior using a fixed-grid, 2d (x,z) ice sheet model. The goal is to assess the performance of a number of sub-grid scale parameterizations (for geometric forcing and boundary terms near the g.l.) with respect to g.l. motion given by a “benchmark” set of experiments (MISMIP). The results indicate that (1) sub-grid scale parameterizations are useful and may result in significant computational savings w.r.t. grid resolution, and (2) sub-grid

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scale parameterizations are not a cure-all for the grounding line “problem”; very high resolution is still needed in order to model g.l. behavior accurately in fixed-grid models.

In general this paper is a well organized and well written. My questions and comments below are minor and my suggested changes are mostly editorial. I suggest the paper be published subject to minor revisions.

Specific comments

The discussion of the dimensionless coordinate, λ , on p.1069, lines ~15-20 is a bit confusing. Specifically, is the “edge” of the domain the upstream edge or the downstream edge (I assume the former)? For the grid point x_i , where is the “distance” measured from . . . also from the upstream edge of the domain? I assume the idea is that, at the g.l., λ is equal to zero? If so, then it is confusing that λ is defined (line 14) as a real number existing on the interval from 0 to 1. Is λ only defined for the two grid cells that contain the grounding line?

In the discussion on how the parameterizations are constructed (section 3), it would be nice to have some idea for how often the linear extrapolation, cubic interpolation, and harmonic mean procedures “break down”, forcing reversion to linear interpolation. For the experiments conducted here, is it not, fairly, or very common that the procedures revert to linear interp?

Lines 5-10 in the “basal drag” section (3.2.2) – At least for the 2d case discussed here it seems like one could calculate the g.l. flux from the Schoof analytical solution, in which case one could use the thickness profile and the Schoof g.l. flux to (at least partially) reconstruct the “correct” velocity profile across the cell containing the g.l. This better constrained velocity profile could then be used w/ equation 29 when calculating B^2 at the midpoint (a half-baked idea that might be worth some more thought at some point).

p.1083 (“time evolution” section)

lines 15-18 – Can you confirm here whether or not the thickness change (local to the

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g.l. that is) also occurs in steps, or is it smooth and continuous in time (whereas the g.l. motion is not).

Lines 2-28 -The whole issue of whether or not these (or other) parameterizations can be improved any further seems to lie w/ understanding this “jumping” behavior. I’m still not sure I understand it. Is there a specific (numerical?) reason why the g.l. should get “stuck” near a grid cell? If the authors have ideas, perhaps they could speculate on them here? Perhaps they are speculating on them and I’m not entirely getting it . . . but it seems like a better understanding of this behavior is the key to this whole problem.

p.1085 – I don’t follow the explanation at the top of the page.

p.1086 - lines 2-3: Note why the non-linear drag law is a bit “easier” on resolution requirements and may show slightly faster convergence (relative to the performance Metrics that is) with increasing resolution; when $m=1/3$, the basal “stickiness” decreases as the sliding velocity increases, in which case it becomes increasingly more slippery (and the transition zone one needs to resolve widens) as one approaches the g.l. This is not the case for $m=1$.

p.1086 - Line 26 – I see where the factor of 8 comes from, but I don’t follow where the factor of 16 comes from. Perhaps it would be good to clarify where these numbers come from (e.g. you get $2 \times 2 = 4x$ increase for a doubling of horiz resolution (in 2d map view) and also (presumably) a halving of the timestep (assuming an explicit advection scheme and CFL limitation), which gives you another factor of 2).

Somewhere in the discussion/conclusions it would be nice to see an explicit statement about the resolution requirements (for this study, this model, these assumptions, etc.) to achieve an “acceptable” level of error using the “best” GLP used here. Is it 100m? 500 m? 1km?

Figures / Tables / References

The figures, tables, and associated captions are all relevant and complete in my opin-

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ion. All of the references in the text appear in the “references” section and vice versa.

p.1065 – Vieli and Payne ref is to “2005” – should be 1998? Same mistake occurs in other parts of the text.

Ref. for Schoof “grounding line dynamics” looks weird . . . lots of extra numbers at the end?

Technical corrections

Abstract

p.1064

Line 3 – “fixed-grid” (add hyphen?) Line 4 – omit “behavior” Line 5 – “not *yet*” achievable Line 8 – “how this profile impacts the gravitational . . .” Line 14 – omit “more” from “more self consistent” Lines 14-15 – “comparable to halving the grid resolution”. Do you mean “doubling the grid resolution”? Generally, higher grid resolution means a finer grid, and in the context of the sentence as written, it sounds like you mean that a better parameterization gives you the effect of higher grid resolution. Line 15-16 – “The approach to parameterising the grounding line presented here does not completely solve . . .” Line 16 – “it reduces the *computational* requirements . . .” Line 17-18 – write out “one dimensional”

Introduction

p.1064

line 21 – “the potential *for* . . .” line 22 – omit “in response to climate change”; g.l. retreat and “collapse” could occur in the absence of climate forcing due to internal ice sheet dynamics and/or other non-climate related forcing. Line 25 – Nothing against Katz and Worster but I think there are earlier and more relevant and significant papers that could be referenced on the subject here, which also include adequate review material (e.g. Schoof, JGR v.112, 2007).

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p.1065

line 3 – omit “very strong” line 5 – “very-high” (add hyphen?) line 11 – “. . . especially for the case of a full, three-dimensional ice sheet model.” Line 14 “sub-grid” (hyphen?)

Design rationale

p.1066

equation 1 – plane old rho is not defined (presumably “rho_i” = ice density) lines 6-9 – another way to say this is that no one has clearly demonstrated yet that you need full Stokes in order to model g.l. behavior correctly. Line 20 – “sub-grid” (hyphen needed? – I won’t mention it anymore but perhaps check hyphenation throughout!)

p.1067

line 1 – “Assessment of this approach is based on model convergence and the accuracy of model output with increasing grid resolution.” Lines 2-20 – This section is a bit hard to follow and could probably use a bit of reworking.

Model description

p.1068

line 4 – more generally “surface mass balance” as opposed to “net surface accumulation”? line 15 – this line should be connected to the end of 14 rather than starting a new paragraph? Confusing, because then the description for viscosity is given, and the current paragraph/sentence structure suggests that this has something to do w/ B^2 . Also, suggest moving the equation for the eff. visc. up to after where it is first defined (line 11).

Parameterising the grounding line

p.1069

line 8-9 – “the GLOs all generalize to the case of multiple grounding lines without

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modification.” Line 10 – “according to *the* choice of ...” Line 11 – “...and one of four corrections to the geometric forcing term, giving 24 ..” Line 15-20 – confusing description of dim. coord. (see discussion in specific comments)

Parameterizing the thickness profile

p.1070

line 5 – “...demonstrates how closely *these 6* (?) thickness profiles ...”

linear extrapolation

p.1071

line 15 – It’s a bit unclear what “this choice” refers to. Line 16-17 – “...make use of the thickness and thickness gradients landward and seaward of the grounding line.” In general, the discussion of cubic / higher-order interpolation is a bit confusing here, since it is discussed later in it’s own section. Perhaps that discussion should be left out of this section entirely and moved to the cubic interp. section?

Cubic interpolation

As noted above, suggest moving initial discussion of cubic interp from the linear extrapolation section to here.

Harmonic mean-based parameterisation

p.1074

line 10-13 – the description says that the “harmonic mean is a special case of an equation used in numerical heat transfer problems ...”. This is a bit misleading, since “harmonic mean” is a general mathematical description of one of several kinds of averages (e.g. geometric mean, arithmetic mean). Suggest that this description be corrected or broadened a bit.

Gravitational driving stress

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p.1077

line 6 – “... errors due to the numerical integration start to become ...” lines 10-12 – I’m not sure I follow this discussion.

Experiments

p.1079

line 4 – “... (MISMIP) of Schoof et al. (2009)” - misplaced “(“ line 22 – “... must advance *by* more than ...” line 24 – “*Both* advance and retreat simulations are used ...”

Assessing performance

p.1080

lines 2-4 – “...demonstrated that when using the linear thickness GLP ... the steady state g.l. position approaches the analytical solution ...” lines 8-9 – “The values of *these* error metrics ...” line 17 – “*Here this is referred to as* “Retreat minus Advance” (RMA) *and* is defined as ...”

p.1081

line 4 – “Here we have made the *assumption* ...” (?) line 13 – “...by a factor of 2 ...”

Results

Awkward 1st sentence here. Lines 24-25 – “In section 5.2 ...across GLPs *and* we quantify ...”

Time evolution

p.1082

line 3 – “*The* grey bars ...” line 19 – “...better understood *by* considering ...” lines 20-23 – clarify what “unstable in retreat” and “completing successfully” mean here?

p.1083

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line 11 – “*Here*, the relevant state of the system is the ice thickness profile . . .”

p.1084

lines 8-17 – Why is it that the “jumps” in g.l. motion associate w/ the two parameterizations, both at the same grid resolution, occur at what looks to be different spatial locations? Is that an artifact of the plot, or is the lesser of the two GLPs getting hung up “at” a grid cell whereas the better of the two is not? Perhaps this is just a plotting issue or I’m not paying close enough attention to the axes . . .

line 24 – “. . . rather than *being* indicative of faster . . .”

non-linear drag law

p.1085

line – 27 – “. . .by approximately *a* factor of two . . .”

Discussion

Line 21 – “. . .and the simplest GLP or changes (doubling) of grid resolution.”

p.1087

lines 15-20 – The “stick-slip” behavior of the g.l. w.r.t. the fixed grid seems to be the fundamental problem here and I’m still not sure we understand why this occurs. If the authors DO understand why it happens, they should clarify it here, because it seems like a correction of that behavior would solve the problem.

Line 21 – “this interpretation” – make sure it is clear what we are still talking about here.

p.1088

lines 15-22 – from the discussion, it is not clear whether or not Pollard et al. still get the stp like behavior w.r.t. g.l. motion. If so, are their steps just “correct”, w.r.t. ice flux such that they get the right g.l. migration?

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Conclusions

p.1088

line 26 – “. . .centred on interpolating ice thickness, driving stress, and basal traction over the grid cell . . .”

line 27 – “an order of magnitude” . . . w.r.t. what ?

Appendix A

p.1090

lines 7-10 – Why is a linear forcing used here vs. a “step” function forcing, as in MISMIP? Perhaps not relevant, but please note if there is some significant reason for choosing one over the other.

Interactive comment on The Cryosphere Discuss., 4, 1063, 2010.

TCD

4, C670–C678, 2010

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