Author responses for "Neutral equilibrium and forcing feedbacks in marine ice sheet modelling"

Rupert Gladstone, Yuwei Xia and John Moore

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1   Author notes

The authors would like to thank the reviewers for their constructive comments and suggestions. We have copied the text from the reviewers below. *Authors responses are given in blue.* *Page and line numbers of additions/modifications in the revised manuscript are given in blue italics.* We provide in addition both the modified manuscript and a document highlighting the changes between our original and revised manuscripts.

2   Response to RC1

2.1   General comments

The manuscript discusses the existence of multiple steady state (and poor numerical convergence) in a marine ice sheet grounded on a prograde bedrock slope as a result of the discretisation of the total basal friction, which the authors refer to the friction force feedback. A flowline based on the finite elements method is constructed to solve the full-Stokes equations (in this case, the domain is 2-D with horizontal e vertical axes). Idealized numerical experiments varying the accumulation rate (surface mass balance) are performed to explore the movement of the grounding line in a coarse mesh (the authors purposely defined the model in a coarse resolution, 1 km, to study the behavior of the grounding line). The numerical experiments consist of an advance phase followed by a retreat phase. Two perturbation experiments are also carried out to investigate further the model reversibility. The grounding line positions as function of simulation time (for all experiments) are shown in graphics. These results are used as arguments in the discussion of the existence of a region multiple steady states, and in the problem of the reversibility. In special, a perturbation experiment (P2) that shows reversibility, even not being in a steady state, is used in the discussion of implications for experiment design. The discussion also counterposes the difference of "neutral equilibrium" and "multiple steady states" (a didactic figure is shown), and a possible reason for the existence of the last one (multiple steady states) and not the first is also listed: the discretisation of the total basal friction (named by the authors as friction force feedback). The result of a small perturbation experiment (PS) where the perturbation force is not sufficient to "move" the grounding line is used as argument of this possible reason.

Overall the manuscript is well written, and the figures are well visible. The experiments are described and constructed to sustain the authors arguments. The discussion of implications for experiment design to evaluate ice sheet models is relevant. I recommend the publication of the work.

Thanks to the reviewer for positive review, and for the constructive comments. These have improved the clarity of the manuscript.

2.2   Specific comments

- The term "false positive" is used in abstract to refer to the case that appears to achieve convergence when in fact (...) is not true. This case is the experiment named P2. The term "false positive" could be

written allong the text (Sections 2.2, 3.1, 5, 6) such that the reader can link and follow the discussion started in the abstract.

We have now made use of this term in sections 3.1 and 6 (note that section 2.2 is purely descriptive where as the term "false positive" is an interpretation, and that section 5 is mainly about the friction feedback).

Page 6, line 31; page 11, line 2

- A suggestion of two additional papers as reference of special treatment of grid cell or element containing the grounding line (page 1, line 24): Seroussi et al. (2014) and Feldmann et al. (2014).

yes, these are also relevant, and we've added them now.

Page 2, lines 11-12; page 10, lines 20-21; page 2, line 2

- The term "convergence" is treated as a numerical convergence along all the manuscript. I ask to the authors to insert few words in the beginning of the manuscript (in introduction and maybe in abstract) explaining that the term "convergence" means (will be used as) "numerical convergence".

We have clarified our use of the term convergence where it first occurs in the introduction

Page 1, lines 20-22

- Sections 5 and 6 are not addressed in the end of the Introduction (page 2, lines 9 to 11).

We've added a line at the end of the inctroduction to refer to section 5. Section 6 (conclusions) needs no introducing.

Page 2, lines 17-18.

- The lateral drag (an approach to model the buttressing) is parameterised according to channel width. There is no description of how it is done, but it seems that is similar to the reference cited (Gagliardini et al., 2010). An issue that arises is how this lateral drag (buttressing) impact the size of the region of multiple steady state? Did the authors perform any experiments with no lateral drag? For example, experiments P1 and P2 starting since t=0 ka (advance and retreat phases) with no lateral drag?

This is a mistake in our description. In fact we did some initial simulations in which there was lateral drag, but all the simulations shown have zero lateral drag. We've removed the mention of lateral drag.

Informally I can say that the presence of lateral drag can reduce the relative impact of the step change in resistive forces at the grounding line, reducing the relative importance of the friction feedback, and reducing the size of the region of steady states. However, the lateral drag needs to be very high in order to make a difference when a Weertman sliding relation is used. This argument is already made in the Gladstone et al. 2012 Annals paper.

- It is written (page 2, lines 14 to 15) that the model setup is similar to that original MISMIP. But the bedrock used in the manuscript (Equation 1) is different of the original MISMIP bedrock. The authors could add some words explaining that the bedrock used in the manuscript is inspired or it is a modification of the original benchmark. Is there any reason for that modification?

We don't feel that the values of parameters chosen relative to previous experiments are of much importance. The relevant thing here is that the inputs we use demonstrate the features (irreversibility, region of steady states, advance and retreat) of relevance. We've extended the sentence about MISMIP to summarise the similarities, but we don't feel a discussion of the differences adds value to the paper.

Page 2, lines 22-23

- What is the length of the domain in the x-direction (maximum x, ice front position)?

We should have stated this. It is 600km. We've added this information just below the bedrock equation.

Page 3, line 3

- What is the time step used in the experiments? It could be insert in flowline description (Section 2).

We should have stated this. It is 0.2 years. We've added this information in section 2.

Page 3, lines 13-14

- I think it is important to address in the Flowline description (Section 2) that the grounding line position is defined only on the vertices of the elements (I hope I understood correctly the approach that Elmer/Ice does solving the contact problem). Maybe it could be inserted after the description of contact problem (page 3, line 19).

Yes, this is correct. We've clarified it and added a line about represnting friction at the grounding line.

Page 2, lines 27-30

- The experiments (advance/retreat phases) are well written, but a table or a graphic resuming all of them (showing the variation of the external forcing as function of the time, for example) should be also inserted such that the reader will follow the results according. This would enhance the understanding of the results, mainly for P1, P2 and PS (I spent a time following and interpreting the results, the grounding line evolution, mainly for the perturbation experiments).

It is a bit awkward to make the table because the advance retreat experiments are almost identical, and having a row each for this is just a waste of space. We've implemented a name convention for these to enable making the table, and we've added the table. We've modified the advance retreat section to introduce the new naming.

Page 3, Table 1; Page 3, lines 22-24; Page 4, lines 3-4

Note that we also made a minor mistake describing the small perturbation experiment, implying that it had the same retreat forcing as P1 and P2. We've corrected this.

Page 4, lines 9-11

- Page 5, line 19. In the phrase: The region containing steady state grounding line positions in the current study spans from x = 143 km to x = 176 km. This interval refers at the end of the retreat phase, right? So, maybe an additional note could be inserted: The region containing steady state grounding line positions (at the end of the retreat phase) in the current study spans from x = 143 km to x = 176 km.

We've added this as suggested.

Page 6, line 7

- Page 5, line 21. In the phrase: We tested this hypothesis near the seaward end of the region by implementing small increments in a between advance simulations. Are these increments the simulations with a=1.4 to 2.0 ma-1? If yes, a note could be added, for example: we tested this hypothesis near the seaward end of the region by implementing small increments in a between advance simulations (a=1.4 to 2.0 ma-1). If no, it is necessary to write more details about what was done.

Yes. We've clarified this using the new experiment naming.

Page 6, line 10

- Page 5, line 23. Same as above. A note could be added, for example: Specifically we obtained a final grounding line position on every node from x = 174 km to x = 180 km for the advance simulations and from x = 174 km to x = 176 km for the retreat simulations (see Figure 2, simulations with a=1.4 to 2.0 ma-1).

We've added a reference to the figure.

Page 6, line 12

- It is important to note that between x = 174 km to x = 180 km there are 7 mesh nodes; in x = 174 km to x = 176 km, there are 3 mesh nodes.

We're not convinced this is important to point out. In any case it is self-evident, given that the mesh resolution is 1km. We have not altered the text.

- Page 5, line 25. even for simulations showing no grounding line movement -¿ even for simulations showing no grounding line movement (i.e., a=0.5, 0.7, 1.0, 1.4, 1.5, 1.6 ma-1)

Again, we've followed this suggestion using the new experiment naming.

Page 6, line 14

- Page 6, line 5. In the phrase: P2 shows full reversibility and P1 does not. Does this "full reversibility" refer only for the grounding line position or also refer to the ice volume? In Figure 3, the variation of the ice volume for P2 is not shown. So, maybe it is also relevant to include (in Figure 3) the variations of P2 in terms of ice volume and basal friction force.

The ice volume and friction are mostly shown to support later arguments. Showing both P1 and P2 for these plots is not very useful because the differences are at the far left couple of mm of the plot, and are obscured by the rapid rates of change. We've clarified that reversibility does refer to all aspects of the model state.

Page 6, lines 29-30

- About the discussion initialized in page 5, line 12 (Section 3.1). The discussion of Schoof (2007) (Section 4.4, page 14, line 105) says that "numerical underresolution may also affect the results of Pattyn et al. (2006) ...". So, what should be the impact of the numerical underresolution on the Pattyn et al. (2006)s results (in terms of size of region of multiple steady state, reversibility)?

Previous work by Gladstone et al (notably JGR 2010, TC 2010, Annals 2012) has demonstrated that the region of steady states in general decreases in size with finer resolution. This is not specific to the Pattyn 2006 paper.

The current study demonstrates that reversibility is not a robust test of whether sufficiently fine resolution has been achieved. Again, this is not specific to the Pattyn 2006 study. We have added a line to make this clearer in the previous paragraph.

Page 6, lines 31-34

The important implications for the Pattyn 2006 study are that their experiment design did not reliably support their result about transition zone length, and we believe we have demonstrated this clearly.

- The phrase (page 7, line 17): "We argue that IDMs exhibit a region containing multiple locally stable equilibria ...". This region exists due to the errors on the numerical modeling, right? I think it should be reinforced in that phrase or in the respective paragraph.

Yes, it is a numerical artefact. This is an important point which appears to have confused the other reviewer. We've modified the first and last sentences of the relevant paragraph to make this clear.

Page 8, lines 25 and 28

- The fact that some IDMs exhibit a region containing multiple locally stable equilibria makes the "reversibility test" fragile, as the authors well pointed out in Section 5, since the results (reversibility) would depend on the initial condition (the region of multiple locally stable equilibria). However, I think it is important to address in the discussion that the existence of these regions should not be admissible, in the sense that further researcher to improve the numerical schemes used in IDMs should be carried out.

Certainly any numerical artefacts should ideally be removed from, or at least quantified in, any published studies using IDMs to make statements about the real world. Hopefully this is obvious to all readers, but we've added a paragraph at the end of the discussion just to make this absolutely clear.

Page 10, lines 9-14

- The phrase (page 8, line 2): "but heavily discretised in the model due to basal friction reaching a peak at the grounding line." Maybe it should be: "but heavily discretised in the model due to the numerical scheme used to solve the contact problem (grounding line "jumps" only on the element nodes)".

The suggested change in wording implies that the problem is specific to the numerical scheme used in this particular model. But this is not the case, the problem is common to nearly all models, and the various parameterisations used are in general only partial solutions. For this reason we prefer the original wording.

- Note that it is expected the basal friction reaches a peak at the grounding line, since it is expected that the basal velocities are higher there (for example, Figure 11 in Schoof (2007)), considering the Weertman model. So, for the flowline-Stokes used in the manuscript, the grounding line represents a "singular point", in the sense that there is an abrupt change in the boundary condition (basal friction) considering the last point grounded (grounding line) and the first floating node (no basal friction). Using another sliding relations, possible this singular point would "vanish", as the authors well written in the paragraph started in line 10, page 8. I recommend the inclusion of these discussions in the manuscript.

This is a valid point, but is not the main focus of the current manuscript. These issues are discussed in more detail in more relevant recent papers (Gladstone et al 2017 in TC and Brondex et al 2017, J. Glac.).

- From my point of view, the friction force feedback represents the variation of the boundary condition, which is solution-dependent: depends on the velocity field (in special, the basal velocities) and the position of the last grounded point (grounding line), which in turn depend on the boundary conditions. Then, some possible sources of discretization errors are, in my opinion (not necessarily in this order of weight, and not just summarized to these): a) the boundary condition (the friction force feedback in this case) should be continuous, but it is applied only on the element nodes; b) near (and at) the grounding line, both the velocity field (in special the basal velocity) and the basal friction have high gradients, what could not be well captured

if there are few elements in there; c) and, the last grounded node (the grounding line) represents a "singular point", what requires a high mesh resolution in its neighborhood (see, as an example, the Figure 10.9, page 189, in Szab and Babuska (1991)). So, if the authors agree with my opinion, and if relevant (it is up to the authors), the observations as above could be also added in the discussion part.

These are valid points, but they all essentially relate to resolution and or grounding line parameterisations, which are the focus of other more relevant studies. In particular Gagliardini 2016 TC considered approaches to handling the singularity in the Elmer/Ice model, but plenty of other studies over the last decade look at this issue and how different approaches have been taken in different models. There are different angles one could take when looking at this issue, and noone yet has a really good solution. We prefer not to modify the text in response to this comment as we want to keep the focus of this paper on the issue of the numerical artefacts and neutral equilibrium in relation to experiment design.

- A last question: if the region of multiple steady state is due to the numerical scheme used (so, depends on the IDM), how this region could be used as metric in model evaluation/comparison, as pointed in the conclusion part (page 9, line 2). (If each IDM has its own region of multiple steady state...)

Well, it is a numerical artefact, and so it should reduce with increasingly finer resolution. Any model can demonstrate this without recourse to another model. And of course it would be possible to compare the size of the region across models at a given resolution. We've added a sentence to the conclusions, though we don't feel it necessary to point out the latter point, as this may be misleading: if model A is "better" than model B at resolution XXX, but model B is typically run at resolutions several orders of magnitude finer than XXX, then such a comparison is not meaningful. Also, such a direct comparison is not informative about the rate of convergence.

Page 11, lines 4-5

2.3   Technical corrections and typos

- The term spin up is used along all the manuscript. Please, check the correct spelling along all the manuscript (spinup, spin-up or spin up?):
a) page 1, line 12
b) page 5, line 31
c) page 6, line 1, line 3, line 12, line 13, line 17
d) page 9, line 5

We now use only "spin-up".

- The term artifact is sometimes written as artefact. Please, check the correct spelling along all the manuscript (artifact or artefact?):
a) page 1, line 3
b) page 5, line 14, line 17
c) page 6, line 19
d) page 8, line 10

Both spellings are ok. We have now used "artefact" everywhere.

- Page 2, line 18. The variable W (channel width) is not used neither defined. Maybe it could be deleted. The channel width should defined in the text.

Removed.

- In Section 2.1, pg. 5, line 4. They are run for 1 ka with a=2.0 ma-1 (...). The value of a refers to the forcing perturbation experiments P1 and P2. However, in the legend of Figure 3 (pg. 12), the value of a is 0.2 ma-1. Maybe it is a typo, but I would like to ask to the authors to check if all forcing values (accumulation ratio) are correct.

We've corrected the figure caption. Thanks!
Page 15, Figure 3 caption

- Check units and space between number and units in all the text:
a) page 2, line 21: -15 C -¿ -15 oC (please use the default degree symbol of the text editor used)

b) page 3, Figure 1: 13ka -¿ 13 ka; a=0.7ma-1 -¿ a=0.7 ma-1; a=1.7ma-1 -¿ a=1.7 ma-1
c) page 3, line 6: 100m -¿ 100 m
d) page 4, Both legends in Figure 2 (accumulation rates legends)
e) page 4, Figure 2: 7ka -¿ 7 ka
f) page 4, line 1: 13ka -¿ 13 ka
g) page 5, line 9: 7ka -¿ 7 ka
h) page 5, line 11: 7ka -¿ 7 ka (maybe here t=7 ka)

We use Latex. We've now tried to make our units and spacing consistent, and will also follow the recommendations of the Copernicus typesetters if the manuscript is accepted.

- A note explaining the Area in Figure 5 (c) is the ice volume per unit width should be inserted in the Figure 5 legend (as was written for Figures 2 and 3).

Good point. We've implemented this change.

Page 17, Figure 5 caption

- page 8, line 26: See -¿ see

Changed as requested.

Page 19, line 5

- page 7, line 13: Schoof (2007) -¿ (Schoof, 2007)

Changed as requested.

Page 8, line 24

3 Response to RC2

3.1 General comments

This paper aims to examine grounding-line behavior in advance and retreat scenarios, particularly examining cases in which the authors models show examples of what could be termed neutral stability (or multiple steady-state grounding-line configurations for the same forcing) of grounding line position, while also demonstrating that GL reversibility, in and of itself, is likely not a sufficient test for demonstrating that a model is sufficiently resolved.

The paper is well-written and clear, although would perhaps benefit from a statement of the goals of the experiments at the beginning. The approach taken is well-described (I think I could re-run these experiments on my own if I wanted to), and the figures are for the most part clear and well-documented (the figure illustrating stability is a useful one). It is a useful addition to the literature, and I support publication after a few fairly minor points are addressed.

Thanks to the reviewer for the constructive comments. We've expanded the final paragraph of the introduction to give a clearer context for our experiments.

Page 2, lines 14-18

I think the biggest thing missing from this paper is much, if any, discussion of mesh resolution. Its not controversial to state that an insufficiently-resolved ice sheet model will exhibit artifacts in its grounding-line response (even alluded to that in the discussion). It would be very helpful to present some sort of mesh convergence result to demonstrate the regime being operating in for this paper. Resolution is mentioned at the beginning (implying operation in an under-resolved regime), but then dont do anything to place the experiments in context in this sense. Without that sort of discussion, its tempting to label the results here as "odd things that happen when a grounding-line problem is under-resolved"), and attribute the multiple steady-states to hysteresis due to underresolution. It would be very useful if you picked a few of the initial cases (say a= 0.2, 0.7, and 1.7) and show (a) the convergence of the GL and area at steady-state with mesh resolution, and then (b) how the experiments behave in fully- and under-resolved regimes. Otherwise, you essentially seem to be making the point that GL reversibility is not a sufficient test by itself to demonstrate that a model is sufficiently-resolved (which is an important point that the only reliable way to assess whether

one is sufficiently resolved is via a convergence study along the lines of Cornford et al (2016) but its not a point thats being made explicitly in this paper).

It is not true to say that discussion of mesh resolution is missing from this paper. Whenever we mention convergence, which is discussed quite extensively, we mean the convergence of model outputs with respect to increasingly finer resolution. We have added clarification in the introduction that this is what we mean by convergence.

Page 1, lines 20-22

A misunderstanding appears to have occurred here. It is not merely tempting but actually quite correct to label the results here as "odd things that happen when a grounding-line problem is under-resolved", and to attribute the multiple steady-states to hysteresis due to underresolution. Exploring numerical artefacts is a primary aim of this paper, because of their relevance to understanding both the nature of the grounding line problem and implications for designing computer experiments. We think the enhanced final paragraph of the introduction will help to make this clear. We have also enhanced the section on neutral equilibrium, explicitly stating twice that the region of steady states is an artefact.

Page 8, lines 25 and 28

Actually the Cornford 2016 convergence test is not necessarily robust because of the way it is initialised. Using present day geometry and inversion for basal resistance does not provide any guarantee of where the initial grounding line position may lie with respect to a potential region of multiple stable grounding line positions. We have added a couple of paragraphs on the implications of our results for simulations initialised through inversion in section 3.1.1.

Page 7, lines 15-30.

The role of mass balance also isnt mentioned in the results You appear to have chosen a test case in which the additional mass flux onto the grounded ice due to the increased surface area for an advanced grounding line is exactly balanced by the increased flux through the GL due to the increased ice thickness at the GL (hence the apparent multiple stability points). A useful test would be to try this experiment again with (for example) a different bed slope, which would in principle change that relationship.

The chance of the balance described by the reviewer actually occurring is vanishingly small. Of course increased grounded area must lead to increased steady state thickness at the grounding line, but in practice this could only lead to a neutral equilibrium in the underlying system with careful engineering (of, for example, spatially varying basal resistance). In fact Schoof (2007) showed that the relationship between grounding line ice thickness and cross-grounding line flux is strongly non-linear. Multiple steady state grounding line positions have been reported before with different models and with different (but also linear) bed slopes. For example see other Gladstone et al papers (in particular JGR 2010, TC 2017). The multiple steady states are due to numerical artefacts, as we aim to describe in the sections about the friction feedback.

3.2   Specific comments

1. page 1, line 9, 11, 16, etc: The word "convergence" has a particular meaning in numerical modeling describing how a model behaves as the mesh spacing, timestep, etc are refined (or possibly the tendency of, say, a solver, to reduce its residual to a prescribed tolerance). In at least some places, you appear to use "convergence" when you likely mean "steady state". Id suggest a careful check on all of the uses of the word "convergence" to ensure that its being used consistently. Otherwise, there is a tendency for confusion when a single word has multiple meanings and connotations. Id even suggest the use of "convergence with resolution", etc...

We always mean "convergence with resolution" and have clarified this in the introduction. We have double checked all instances of the word and these are all correct.

Page 1, lines 20-22

2. page 2, line 8: Id suggest also citing Seroussi and Morlighem (2018) on discretizing melt forcing near grounding lines here.

We have added a line about the Seroussi paper, which is a useful addition to the literature. We've also extended the discussion to consider the implications of a basal melt forcing feedback.

Page 2, lines 11-12; Page 10, lines 15-27

3. page 2, line 9: I think the choice of flowline modeling and Weertman sliding law are unfortunate here flowline because its perhaps overly simplistic given the current understanding of the effect of buttressing and other effects that are not present in a flowline model; I would have suggested using either a MISMIP3D or MISMIP+ configuration as a testbed. Weertman is unfortunate because as the authors point out, it produces much more of a forcing discontinuity at the grounding line, which is likely amplifying the effects described in this work; something like the Tsai Coulomb-limited sliding law would have been a useful counterpoint. That said, none of these specifically discount the conclusions drawn in this paper, but instead leave important questions unexamined.

In order to demonstrate a specific feature of a system, the greatest clarity and attribution can be achieved by demonstrating the simplest configuration in which said feature is manifest. This, plus computational efficiency, motivates our choice for a flowline domain.

Weertman sliding was chosen for 2 reasons: it is commonly used and it gives strong numerical artefacts, which are, after all, what we wish to focus on. The results shown here do apply, with a lesser magnitude, to other sliding relations, and this is discussed in the paper.

4. page 2: (problem description) how long is your domain in the x-direction?

600km, we've added this information now.

Page 3, line 3

5. Figure 1:

(a) Is there really no vertical shear in the velocity field? Thats surprising, but is the impression I get from the vertically-constant coloring of the velocity field.

There is vertical shear, but the velocity variations are dominated by increase along the flow, which is much greater than the vertical shear. Bear in mind also that the plot is vertically exaggerated. We have now mentioned this in the Figure caption.

Page 4, Figure 1 caption

(b) The use of the intensity-based colormap doesnt work well with the two profile plot as designed, since the semi-transparent colors cant be distinguished from different speeds for the second profile. Id suggest switching to a colormap which isnt intensity-based if you want to present the second profile as a lighter-shaded version of the primary colormap. Another option would be to simply show only one representative velocity magnitude plot, but overlay the outlines (in black) of multiple profiles, which would also allow for more than one alternative profile.

The aim of the profile plot is to give the reader an intuitive view of the model setup. We don't need to show more than the two profiles used in the perturbation experiments. The plot conveys the convex nature of the grounded region and it conveys that the ice velocity increases down stream, and it conveys the magnitude of difference between initial states for hte perturbation experiment. There is no further information that needs to be conveyed by this plot. It is not important to show the details of the velocity profiles separately. We have not changed this plot.

6. Figure 2:

(a) Did the 0.2 m/a run ever actually achieve steady-state? Its not obvious from the area plot.

This simulation had an unchanging grounding line position for the last 6000 years. Also, we added a line to report rates of change of area at the end of the simulations, to demonstrate that steady state has been approached.

Page 5, lines 2-4

(b) It appears that all of the cases for which the advance-phase a is greater than 1.6 m/a all collapse onto the 1.6 profile. Is that the case? Id say this is really odd behavior; do you have any idea why? Are the GLs all getting stuck on the same cell boundary? It seems like the behavior is very different above and below that threshold.

Yes, all GLs on the same node at 176km. This is the upper end of the region of steady state grounding line positions, as discussed in section 3. We believe this is due to discretisation of a link between model state and forcing, which is discussed in sections 4 and 5. We've added a couple of lines to the end of section 3 to clarify the link between this behaviour and our explanation.

Page 6, lines 16-19

7. Section 3 (discussion of multiple steady states) it might make sense to move this section to before what is now section 2.2; if you did that, the initial experiment (section 2.1) would be followed by the discussion of the initial experiment, and then the follow-on perturbation experiments would have a context when theyre introduced. It would also lessen the number of times a reader has to page back and forth between experiment description, results, and discussion.

This is a valid point, but there are arguments both ways. An early draft of the paper was laid out as suggested, but co-authors found it counter-intuitive. The current layout may require some referencing back and forth, but on the plus side it is clear where to find the information needed, on account of having one section to describe the model and experiments. We prefer to keep the current structure.

8. page 5, line 10: A useful test for steady-state would be to compute the time derivative of the area and plot that.

We have now calculated the area change rate and find it to be negligible for our advance and retreat simulations. We report this in Section 3.

Page 5, lines 2-4

9. page 5, line 10: "grouding"-¿ "grounding"

Fixed, thanks.

10. page 7, line 2: suggest changing "a ball on a hill" to "a ball perched on the summit of a hill"

Changed as requested.

Page 8, line 13

11. page 7, line 2: "after begin" -¿ "after being"?

Fixed, thanks.

Page 8, line 14

12. page 7, line 6: suggest changing "A large perturbation" to "A large-enough perturbation"

Changed to "A sufficiently large perturbation"

Page 8, line 17

13. page 8, line 25: This suggests something isnt quite right, since one would expect the friction to vary smoothly throughout advance and retreat if the subgrid-scale friction discretization is done correctly. If thats the case, then one wouldnt expect to see mesh-related artifacts of advance and retreat in the friction field, would you?

Indeed, the problem here is that noone has yet come up with the perfect grounding ine parameterisation, and perhaps such a thing is simply not possible. People have certainly come up with "correct" implementations of an interpolation based on the floatation condition, but it doesn't fully capture the forcing feedback, perhaps partly because the ice geometry at sub grid scale is not captured. If you can come up with a grounding line parameterisation in which mesh artefacts are absent then you'll be famous (as Steve Price once said to me)!

14. Figure 3: Why isnt experiment P2 shown in 3(b) and 3(c)?

It is barely distinguishable from P1, which we felt was more confusing than including it. In fact the aim of subplots (b) and (c) is to start discussion on the forcing feedback. Subplot (a) is already sufficient to demonstrate the weakness of perturbation experiments as a demonstration of convergence.

15. Figure 5: What happens if you allow the system to reach steady-state after the perturbation is applied (rather than discontinuing the forcing after 1000 years? Does the GL finally advance, in that case?

It is actually fairly close to steady state after 1ka. But it is not essential to reach steady state to demonstrate that this is not a neutral equilibrium, which is the main purpose of PS.

Neutral equilibrium and forcing feedbacks in marine ice sheet modelling

Rupert Gladstone[1], Yuwei Xia[2], and John Moore[1,2]

[1]Arctic Centre, University of Lapland, Rovaniemi, 96101, Finland
[2]College of Global Change and Earth System Science, Beijing Normal University, 100082, China

Correspondence to: Rupert Gladstone (rupertgladstone1972@gmail.com)

Abstract.

Poor convergence with resolution of ice sheet models when simulating grounding line migration has been known about for over a decade. However, some of the associated numerical ~~artifacts~~ artefacts remain absent from the published literature.

In the current study we apply a Stokes-flow finite element marine ice sheet model to idealised grounding line evolution experiments. We show that with insufficiently fine model resolution, a region containing multiple steady state grounding line positions exists, with one steady state per node of the model mesh. This has important implications for the design of perturbation experiments used to test convergence of grounding line behaviour with resolution. Specifically, the design of perturbation experiments can be under-constrained, potentially leading to a "false positive" result. In this context a false positive is an experiment that appears to achieve convergence when in fact the model configuration is not close to its converged state. We demonstrate a false positive: an apparently successful perturbation experiment (i.e. reversibility is shown) for a model configuration that is not close to a converged solution. If perturbation experiments are to be used in the future, experiment design should be modified to provide additional constraints to the initialisation/~~spin up~~ spin-up requirements.

This region of multiple locally stable steady state grounding line positions has previously been mistakenly described as neutral equilibrium. This distinction has important implications for understanding the impacts of discretizing a forcing feedback involving grounding line position and basal friction. This forcing feedback can not, in general, exist in a region of neutral equilibrium, and could be the main cause of poor convergence in grounding line modelling.

1 Introduction

Strongly resolution dependent behaviour when implementing grounding line movement (sometimes referred to as grounding line migration) in a marine ice sheet model was identified by Vieli and Payne (2005) and was further characterised as a convergence problem by subsequent studies (Durand et al., 2009; Goldberg et al., 2009; Gladstone et al., 2010a, b, 2012). In the current study "convergence" refers to the approach of model outputs to a consistent state as resolution is made progressively finer. Some models incorporating a moving grid that explicitly tracks grounding line position do not appear to exhibit this poor convergence (Vieli and Payne, 2005). Various forms of mesh refinement help to address the problem, though very high resolution is still

needed (Goldberg et al., 2009; Cornford et al., 2013), and special treatments of the grid cell or element containing the grounding line can also improve convergence ~~(Pollard and DeConto, 2009; Gladstone et al., 2010b; Gagliardini et al., 2016)~~(Pollard and DeConto, 20

This problem has also been decribed as neutral equilibrium (Durand et al., 2009; Pattyn et al., 2006) in modelling studies. This terminology may follow from earlier studies in which it was proposed that real marine ice sheet systems may exhibit neutral equilibrium (Hindmarsh, 2006). Although these theories are no longer accepted (Schoof, 2007), unconverged model behaviour at coarse resolution is still sometimes referred to as neutral equilibrium (Durand et al., 2009).

Most of the studies cited above use a Weertman sliding relation (Weertman, 1957). More recent studies (Leguy et al., 2014; Tsai et al., 2015; Gladstone et al., 2017) suggest that the convergence issues may be to some extent mitigated by use of sliding relations incorporating a dependence on effective pressure at the bed. However, irrespective of sliding law, similar convergence issues may arise due to a step change in basal melting at the grounding line (Gladstone et al., 2017). The numerical implementation of basal melting at the element or grid cell scale may also have a large impact on convergence (Seroussi and Morlighem, 2018).

In the current study we employ a flowline Stokes-flow model with Weertman sliding and no basal melting (Section 2) to further characterise the nature of this grounding line convergence issue (Section 3). We choose a setup in which the problem is under-resolved, i.e. the model outputs are not close to a converged solution. This is chosen in order to demonstrate the nature of the numerical artefacts arising at coarse resolution. We explore implications for design of computer experiments (Section 3.1) and for the issue of neutral equilibrium (Section 4). This leads to a discussion on discretisation of a forcing feedback involving basal friction and model state (Section 5).

2   Flowline modelling

Our aim is to provide a model configuration in which convergence with resolution is not achieved (i.e. our resolution is too coarse for self-consistent model behaviour), and explore the nature of the grounding line problems. Our setup is similar to that of the original Marine Ice Sheet Model Intercomparison Project (Pattyn et al., 2012), with a linear bed, Weertman sliding, and spatially uniform surface accumulation.

We use the ice dynamic model (IDM) Elmer/Ice (Gagliardini et al., 2013). The Stokes equations for a viscous fluid with non-linear rheology are solved using the finite element method over a two-dimensional flowline domain (one vertical and one horizontal dimension) ~~in which lateral drag is parameterised (Gagliardini et al., 2010) according to channel width, W, and a~~ with grounding line capability.

A contact problem is solved to determine the evolving grounding line position (Favier et al., 2012)~~.~~, which is constrained to be located at a node. Basal resistance, or friction, in the vicinity of the grounding line is determined using the discontinuous approach (DI in Gagliardini et al. (2016)). This imposes full friction for all grounded elements and free-slip for all floating elements, with the element containing both grounded and floating nodes considered to be floating.

The rheology follows Glen's law (Glen, 1952; Paterson, 1994) with viscosity calculated using a temperature dependent Arrhenius law (Gagliardini et al., 2013; Paterson, 1994). A constant uniform temperature of -15 C is used in all simulations.

Table 1. Summary of experiments.

Experiment	Initial condition	Advance phase	Retreat phase
ARXX	Uniform 100m slab	Run length= 7 ka, $a =$XX ma^{-1}	Run length= 6 ka, $a = 0.2$ ma^{-1}
P1	AR0.7 final state	Run length= 1 ka, $a = 2.0$ ma^{-1}	Run length= 4 ka, $a = 0.2$ ma^{-1}
P2	AR1.7 final state	Run length= 1 ka, $a = 2.0$ ma^{-1}	Run length= 4 ka, $a = 0.2$ ma^{-1}
SP	AR0.7 final state	Run length= 1 ka, $a = 0.71$ ma^{-1}	Run length= 4 ka, $a = 0.7$ ma^{-1}

The linear down sloping bedrock, b, is given in m relative to sea level by

$$b = 500 - 0.005 \times x, \tag{1}$$

where x is distance from the inland boundary. The domain length is 600 km.

The horizontal component of the velocity is set to zero at the inland boundary, and an ocean pressure condition applied at the ice front and under the floating ice shelf. We take ice density to be 910 kgm^{-3} and water density to be 1000 kgm^{-3}.

A spatially uniform net surface accumulation flux, a, is used, and this value is varied between simulations.

The basal friction or shear stress, τ_b, acts opposite to the direction of flow and has magnitude (Weertman, 1957)

$$\tau_b = C u_b^{\frac{1}{3}} \tag{2}$$

where u_b is the sliding velocity and C is a friction coefficient. C is set to 0.02417 MPam$^{-\frac{1}{3}}$a$^{\frac{1}{3}}$ for all simulations in the current study.

The simulations carried out for the current study are described below. They comprise grounding line advance simulations followed by grounding line retreat simulations (Section 2.1). In some cases further perturbation experiments are then carried out (Section 2.2). These simulations were all carried out with a horizontally uniform element size of 1 km and a timestep size of 0.2 a. Typical steady state profiles for this model setup are shown in Figure 1.

2.1 Advance/retreat experiments

The advance simulations are spun-up from a uniform slab of 100m thickness. They comprise 7 7 ka of evolution with a different net accumulation forcing for each simulation. Values range from 0.2 ma^{-1} to 2.0 ma^{-1} (the full set of values used is given in the Figure 2 legend).

The advance simulations are followed immediately by "retreat" simulations, which in some (but not all) cases exhibit grounding line retreat. These simulations continue from the final states of the advance simulations. They all use a net accumulation forcing of $a = 0.2$ ma^{-1} and are run for a further 6 6 ka.

Each pair of advance and retreat simulations constitutes an ARXX experiment, where XX indicates the accumulation forcing used for the advance phase (e.g. AR0.7 refers to the advance simulation with $a = 0.7$ ma^{-1} and the corresponding retreat phase). Experiments are summarised in Table 1.
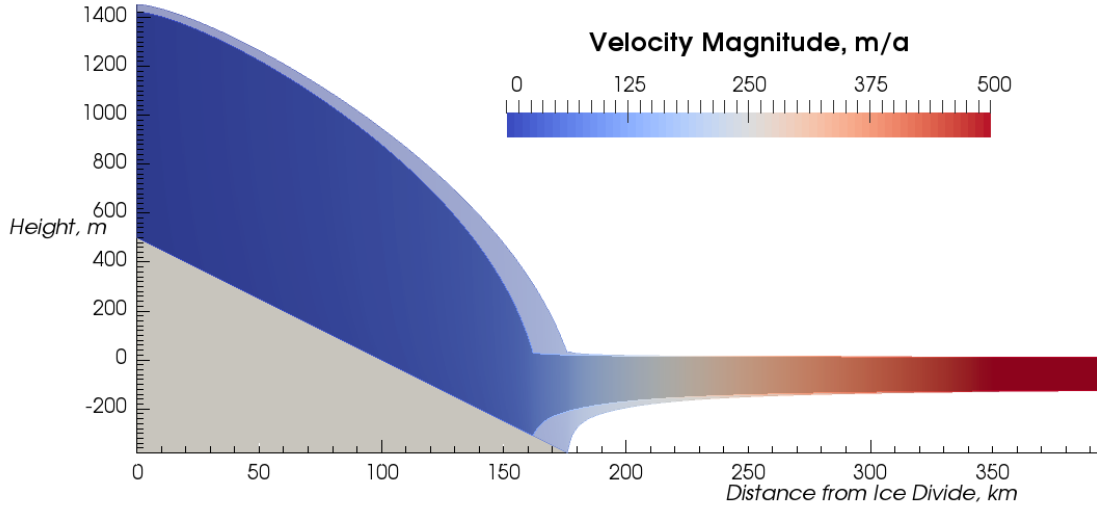
Figure 1. Ice sheet profiles after ~~13ka~~ 13 ka at the end of the advance/retreat simulations described in Section 2.1. The simulations with an advance forcing of ~~$a = 0.7\text{ma}^{-1}$~~ $a = 0.7\,\text{ma}^{-1}$ (solid colour) and ~~$a = 1.7\text{ma}^{-1}$~~ $a = 1.7\,\text{ma}^{-1}$ (semi-transparent) are shown. These states provide the initial state for perturbations experiments P1 and P2 respectively (Section 2.2). Vertical exaggeration is 100 times.

2.2 Perturbation experiments

Starting from the final states of two of the advance/retreat simulations, (i.e. after ~~13ka~~ 13 ka total simulation time) we carried out two perturbation simulations. P1 starts from the final state of ~~the simulation that~~ experiment AR0.7 (which used an advance forcing of $a = 0.7\,\text{ma}^{-1}$), and P2 starts from the final state of ~~the simulation that used an advance forcing of $a = 1.7$. These~~

5 ~~advance/retreat simulations~~ AR1.7. AR0.7 and AR1.7 are shown with a dotted line in Figure 2 and their final states (which form the initial states for P1 and P2) are shown in Figure 1. Note that although P1 and P2 start from approximate steady states, and in both cases the steady state was approached with $a = 0.2\,\text{ma}^{-1}$, these steady states are distinct. The forcing for the perturbation experiments P1 and P2 is identical apart from initial state. They are run for ~~1~~ 1 ka with $a = 2.0\,\text{ma}^{-1}$ followed by ~~4~~ 4 ka with $a = 0.2\,\text{ma}^{-1}$. The perturbation experiments P1 and P2 are shown in Figure 3.

10   We also carried out a small perturbation experiment, PS. PS starts from the same state as P1. It is identical to P1 except that the magnitude of the perturbed forcing is $a = 0.71\,\text{ma}^{-1}$ ~~.~~ and the retreat phase has $a = 0.7\,\text{ma}^{-1}$. Experiments are summarised in Table 1.

4
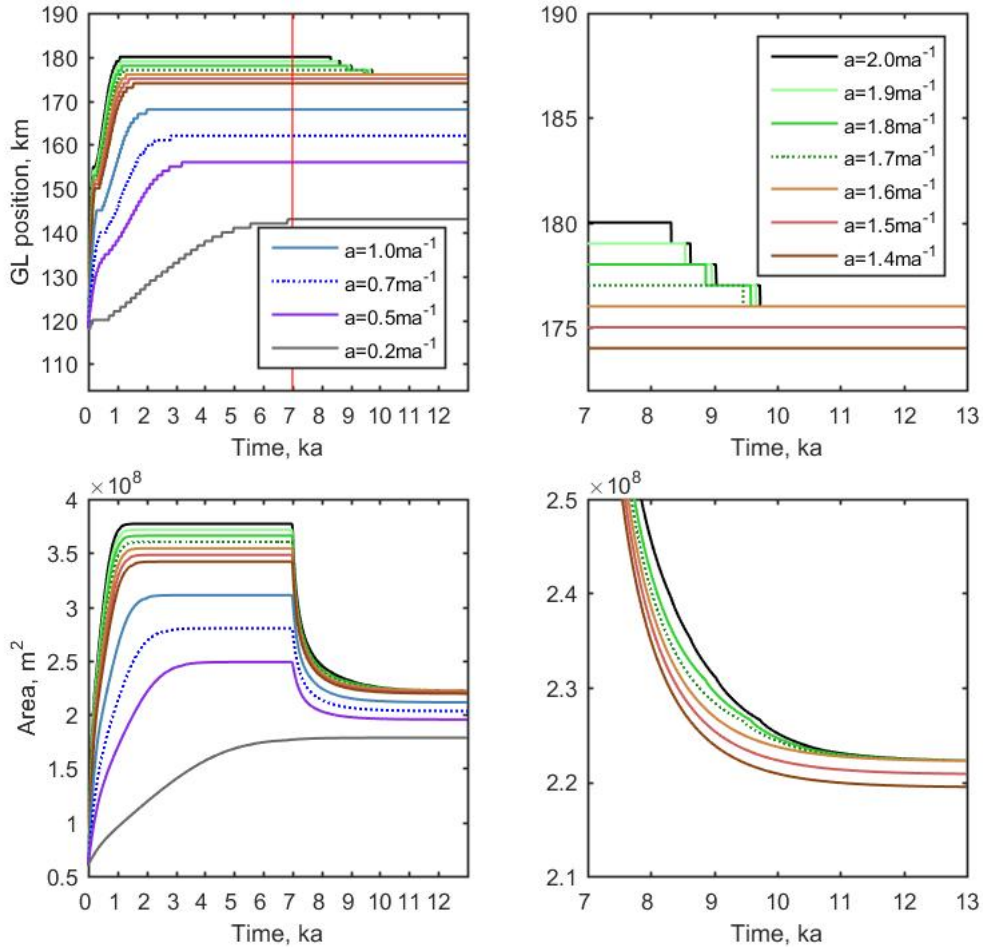
Figure 2. Advance/retreat simulations (described in Section 2.1). Evolution of (top panel) grounding line position for all simulations and (lower panel) ice area. Area in this case is the flowline equivalent of ice volume for a 3D ice sheet, and can also be interpreted as volume per unit width of the glacier. The right hand subplots show detail of a subset of the retreat simulations. The red vertical line indicates the forcing change at ~~7ka~~ 7 ka when the simulations switch from advance to retreat. The legend shows the accumulation rates prescribed during the advance phase, while for the retreat accumulation was ~~0.2~~ 0.2 ma^{-1}. The dotted lines are the cases also shown in Figure 1 and provide the initial state for the P1 and P2 experiments (described in Section 2.2).

3   Multiple steady states

Figure 2 summarises the evolution over time of the advance/retreat simulations. ~~Given that 7ka years is sufficient to approach steady state (we did not impose a~~ Although a formal test for steady state ~~but informally note that no grouding line change occurred during the last few ka years in nearly~~ was not imposed, we calculated the rate of change of area and found it to be of the

5

order of $10^{-8}\,\mathrm{m^2 a^{-1}}$ or smaller at the end of all advance simulations ~~)~~and all retreat simulations (except for AR0.2 for which we calculated the rate only at the end of the retreat simulation as it was not yet at steady state after 7 ka). Given that 7 ka years is sufficient to approach steady state, retreat should occur after ~~7ka~~ 7 ka years for all simulations in which a was initially greater than $0.2\,\mathrm{ma^{-1}}$. This is due to the uniqueness of stable ice sheet configurations on a linear down sloping bed, demonstrated for a "shelfy-stream" approximation by Schoof (2007). However, as also seen with a "shelfy-stream" model (Gladstone et al., 2010a), multiple steady states exist as a model artefact. ~~In common with the advance simulations, 6 years of retreat is not sufficient to achieve an exact steady state in all simulations, but rates of change are low in all simulations after 6 years of retreat.~~

The multiple steady states that exist after ~~13~~ 13 ka are almost certainly numerical ~~artifacts~~artefacts, with the underlying system having only one viable steady state. Similar studies have shown that the size of this region decreases with finer resolution (Gladstone et al., 2010a). The region containing steady state grounding line positions (at the end of the retreat phase) in the current study spans from $x = 143\,\mathrm{km}$ to $x = 176\,\mathrm{km}$. We propose that the model is capable of exhibiting as many viable steady state grounding line positions as there are mesh nodes within this region. We tested this hypothesis near the seaward end of the region by implementing small increments in a between advance simulations (experiments AR1.4 up to AR2.0). Specifically we obtained a final grounding line position on every node from $x = 174\,\mathrm{km}$ to $x = 180\,\mathrm{km}$ for the advance simulations and from $x = 174\,\mathrm{km}$ to $x = 176\,\mathrm{km}$ for the retreat simulations (Figure 2, upper right panel).

The volume evolution plots indicate a reduction in volume for all retreat simulations (except the simulation which advanced under $a = 0.2\,\mathrm{ma^{-1}}$ forcing), even for simulations showing no grounding line movement (experiments AR0.2 up to AR1.6). Simulations ending the retreat phase with the same grounding line position (~~several simulations end at 176~~experiments AR1.6 up to AR2.0 end at 176 km) have the same final volume. Simulations with a more landward final grounding line position have a lower final volume.

Thus, for our setup, 176 km marks the seaward end of the region of steady state grounding line positions under a forcing of $a = 0.2\,\mathrm{ma^{-1}}$. Our explanation for this numerical artefact involves discretisation of a feedback between model state (especially grounding line position) and total basal resistance, which we discuss in Section 5.

3.1 Implications for experiment design

Here we consider perturbation experiments P1 and P2, both of which adhere to a typical perturbation design and both of which experience identical forcing during the experiment. Perturbation experiments are common in IDM studies and intercomparison projects (e.g. Pattyn et al. (2006, 2012, 2013); Favier et al. (2012)). The premise is that an initial ~~spinup~~ spin-up procedure results in an IDM in steady state. A forcing perturbation is applied, causing change, and then removed. The analysis then considers whether or not reversibility has been demonstrated (i.e. whether the IDM state returned to its post ~~spinup~~ spin-up state after the forcing perturbation was reset). However, the existence of multiple steady state grounding line positions means that the requirement to start in steady state is not sufficient to constrain the initial (~~post-spinup~~post-spin-up) state.

The outputs of the perturbation experiments are shown in Figure 3. Although both experiments adhere to typical perturbation experiment design, and are both subject to the same perturbation, P2 shows full reversibility ~~and~~ (in all aspects of model state,

including grounding line position, ice volume and total friction) and P1 does not. The outcomes in terms of reversibility are opposites, resulting directly from the choice of initial state. Here, P2 is a false positive, because considered in isolation it would appear to indicate a converged result, but convergence has not been achieved. In general a converged experiment (i.e. with sufficiently fine resolution to achieve a self consistent result) will always demonstrate reversibility on a linear bed

5   (Schoof, 2007), but our results demonstrate that reversibility is not in itself a sufficient criterion to establish convergence.

We now consider an example of this vulnerability in design of perturbation experiments from the published literature. Pattyn et al. (2006) investigated the role of transition zones in grounding line modelling. The transition zone is a region immediately upstream of the grounding line over which the stress state changes from a grounded regime (in which high basal shear stress approximately balances gravitational driving stress) to a floating regime (where basal shear stress is zero and longitudinal stress

10   in the ice balances a low gravitational driving stress).

Pattyn et al. (2006) used a ~~spinup~~ spin-up procedure that resulted, for most of their simulations, in retreat of the grounding line as steady state was approached. This suggests (but does not prove) that the end of the ~~spinup~~ spin-up resulted in a steady state grounding line position located at the seaward end of the region of multiple steady states, analogous to our experiment P2 (Figure 3). These simulations did demonstrate reversibility. However, their simulation with a short prescribed grounding

15   line transition zone involved no movement of the grounding line as steady state was approached. This suggests (but again does not prove) that the end of the ~~spinup~~ spin-up resulted in a steady state grounding line position somewhere within the region of multiple steady states, analogous to our experiment P1 (Figure 3), see Figure 4 of Pattyn et al. (2006). This simulation did not demonstrate reversibility. Thus the result of Pattyn et al. (2006) that a longer transition zone results in better reversibility may be an artefact of their experiment design rather than a robust result.

20   **3.1.1   Initialisation through inversion**

A common method for initialisation of IDMs is to infer basal properties and ice viscosity (or temperature) through inverse techniques, steady state temperature simulations, and surface relaxation (Morlighem et al., 2010; Gillet-Chaulet et al., 2012; Gladstone et al. These methods can lead to an initial state that is close to steady state, but without any information as to whether convergence is achieved, and hence whether multiple steady states may exist. If a transient simulation intialised in such a way leads to little

25   or no change in terms of grounding line position, it cannot be concluded that the real system being modelled is close to steady state, because the possiblity remains that the modelled steady state is a result of under-resolution.

If a convergence study is carried out for the system undergoing retreat, for example if high basal melt forcing is applied (Favier et al., 2014), this does not prove that the model will also exhibit converged beahviour in advance, nor even does it prove that regions of stationary grounding line within the domain are indicative of a converged result. Conversely, convergence of

30   advance behaviour does not prove convergence of retreat behaviour. The current study does not fully explore the implications of multiple steady grounding line positions in 3D real world cases, which is espeically complicated in that some regions may exhibit an advancing grounding line while others exhibit a retreating grounding line. Based on current simulations the authors recommend that both advance and retreat convergence tests be carried out in order to have full confidence in modelled behaviour.

4 Neutral equilibrium

An equilibrium state (or steady state) of a system is a state that does not change unless the forcing changes. In the context of IDM grounding line simulations, this means that neither the forcing applied to the domain nor the ice sheet configuration are changing over time. Such steady states are typically obtained through long simulations in which forcing is kept constant and the state of the simulated ice sheet gradually stops evolving as equilibrium is approached.

It is important to clarify different types of equilibria for the following discussion. Consider the example of a ball at rest (i.e. in equilibrium) under gravitation on a solid surface. The ball is then subjected to a perturbation: it is moved along the surface then left only under gravitation. Different types of equilibrium may be illustrated by considering the behaviour of the ball after the perturbation has been removed.

An equilibrium state where the perturbation results in the system tending to return to the original state is a stable equilibrium (e.g. Figure 4a - the ball rolls back down to the original position).

An equilibrium state where the perturbation results in the system tending to move further from the original state is an unstable equilibrium (not shown, but consider a ball ~~on~~ perched on the summit of a hill - it will continue rolling away from the summit after ~~begin~~ being given a small push in any direction).

An equilibrium state where the perturbation results in the system remaining in the new state is termed neutral equilibrium (e.g. Figure 4b - the ball may be at rest anywhere on the flat region).

Figure 4c illustrates a system with multiple locally stable equilibria within a confined region. A sufficiently large perturbation will result in the ball finding a new equilibrium position, but a small perturbation will result in a return to the original position.

These types of behaviour for a ball under gravity have analogies for a marine ice sheet system. Schoof (2007) demonstrated the existence of a single stable equilibrium for a marine ice sheet on a downward (in the ice flow direction) sloping bed. Schoof (2007) also demonstrated the existence of an unstable equilibrium on an upward sloping bed, though this may not always be the case in the presence of high lateral drag (Katz and Worster, 2010; Gudmundsson et al., 2012). As mentioned in Section 1, neutral equilibrium in real world marine ice sheet systems is no longer considered plausible, but multiple stable equilibria could exist as a function of bedrock geometry ~~Schoof (2007)~~(Schoof, 2007).

IDM studies using different models have demonstrated that multiple steady states can, as a numerical artefact, exist in models where the system being modelled should exhibit a single stable equilibrium (Durand et al., 2009; Gladstone et al., 2010a). This has been referred to as neutral equilibrium (Durand et al., 2009), and here we consider the distinction between a region of neutral equilibrium and a region of multiple locally stable steady states. We argue that IDMs exhibit~~a~~, as a numerical artefact, a region containing multiple locally stable equilibria (similar to Figure 4c) and not a region of neutral equilibrium.

Figure 5 shows output from experiment PS, the small perturbation experiment. The perturbation, although not sufficient to cause a change in grounding line position (Figure 5a), is sufficient to cause a shift in model state, as evidenced by the change in total ice volume (Figure 5c). However, the forcing reset results in a return to the original state. This behaviour indicates a locally stable steady state rather than a region of neutral equilibrium. This argument against marine IDMs exhibiting neutral

8

equilibria may appear to be a matter of semantics, but there are important implications toward understanding the nature of the grounding line convergence problem, discussed in Section 5.

5 Forcing feedback

Figure 6 shows in more detail the evolution of total basal friction during the advance phase of perturbation experiments P1 and P2. The spikes in total friction correspond to advance of the grounding line by a single element. These features are characterised by an instananeous increase of total friction followed by a rapid decrease and an ensueing gradual increase. The spikes can be explained as follows: An instantaneous increase in basal friction results from a grounding line advance due to the increased contact area. This increase reduces the sliding velocity, causing the rapid decrease in total friction. This is followed by a more gradual return to the longer term trend.

This is a model discretization of what should be a continuous feedback: incremental grounding line advance should cause incremental increase in total basal friction, causing an incremental slowing and thickening. This positive feedback (which we refer to as the friction force feedback) between grounded extent and total friction is continuous in the underlying system being simulated, but heavily discretised in the model due to basal friction reaching a peak at the grounding line. The modelled flux across the grounding line must be higher than that of the system it attempts to represent in order to compensate for the missing basal friction immediately downstream of the grounding line due to the discretisation. Specifically, the PS experiment (Figure 5) demonstrates that even an increase in modelled volume and total friction force of several tens of percent may not be sufficient to cause a single element of grounding line advance. This understanding could not have been attained if the region of multiple locally stable steady states was viewed as a region of neutral equilibrium, because a neutral equilibrium can have no positive feedback between forcing and state (except in the vanishingly low probability case of an exactly compensating mechanism).

We postulate that this discretisation of a continuous feedback is the main cause of numerical ~~artifacts~~ artefacts and poor convergence with resolution in grounding line modelling. This is consistent with the finding that sliding relations incorporating a strong dependency on effective pressure at the bed show far better convergence with resolution (Gladstone et al., 2017). This is due to the basal friction approaching zero at the grounding line, so that the advance or retreat of the grounding line by a single element will not have a significant impact on total basal friction.

Considering several published sliding relations that feature a dependence on effective pressure, it should be noted that the hybrid sliding relations of Gagliardini et al. (2007) and Tsai et al. (2015) typically feature steep basal friction gradients over a transition zone near the grounding line (Brondex et al., 2017) and so may not exhibit such good convergence as the sliding relation of Budd et al. (1979, 1984). It should also be noted that improved convergence is not a valid reason to choose one sliding relation over another: physical realism should be the deciding factor. A final note is that this issue is not fundamentally specific to the equations solved for ice flow, so while the simulations carried out here use Elmer/Ice to solve the Stokes equations, the same principles should apply in other IDMs that implement some kind of sliding relation.

It might be thought that a special treatment of the grid cell or element containing the grounding line, such that the grounding line position within the cell or element can be represented, would resolve this problem of discretising the friction force feedback. However, the grounding line parameterisations introduced by Gladstone et al. (2010b) (a study featuring numerous different parameterisations implemented in a flowline shelfy-stream model) still show a strong non-linear behaviour correlated to grid cell grounding line advance in the evolution of model state (~~See~~see Gladstone et al. (2010b) Figures 3, 4 and 6). Gladstone et al. (2010b) also find multiple steady states exist, with one steady state grounding line position per grid cell, although the steady state position is not constrained to lie on a grid point. Thus grounding line parameterisations do not necessarily resolve the problem of a discretised (or highly non-linear on a grid cell scale) friction forcing feedback.

The poor convergence of IDMs regarding grounding line movement emphasizes the important, and hopefully obvious, requirement that all IDM studies featuring grounding line movement demonstrate that sufficiently fine resolution has been used to achieve a converged result. The resolution must be such that the region of multiple steady states, which is known to be a numerical artefact, must collapse toward zero (we suggest as a rule of thumb that it be required to be no larger than one grid cell or element, though this is not itself a formal demonstration of convergence). Reversibility experiments can not in general demonstrate that sufficiently fine resolution has been achieved.

5.1 Basal melting

As mentioned above, the forcing feedback involving total basal resistance will be of greatly reduced magnitude in the case of sliding relations in which the basal resistance smoothly transitions to zero approaching the grounding line. Given the currently increasing use of such sliding relations (Gladstone et al., 2017; Brondex et al., 2017; Tsai et al., 2015), it might seem that the impacts on experiment design described here (Section 3.1) will not be widely applicable. However, recent studies have shown that ocean-induced melting at the base of ice shelves can cause convergence problems, both at a sub-grid scale (Seroussi and Morlighem, 2018) and over multiple grid cells or elements (Gladstone et al., 2017). Given that these convergence problems can be partially mitigated by smoothing the basal melt to approach zero approaching the grounding line (in this case approaching from seaward rather than landward), we propose that a forcing feedback, analagous to the friction force feedback, is involved. In this case it is between basal melting and geometry: a retreating grounding line will expose more basal area to melting, increasing thinning in the vicinity of the grounding line and enhancing retreat. Discretisation of this melt forcing feedback also leads to the existence of a region of multiple steady states at coarse resolution (Gladstone et al. (2017), Figure 5), implying that the same restrictions on experiment design will apply.

6 Conclusions

The established poor convergence of many marine ice sheet models regarding grounding line movement is characterised by a region of multiple locally stable states. Our results demonstrate that this is not, as has been previously claimed, a neutral equilibrium.

This region of steady states implies that perturbation experiments, such as are often used in model intercomparison projects, can have a hitherto unrecognised dependence on initial conditions, potentially leading to false positives. Thus the size of the region of multiple locally stable steady states may be a more useful metric for assessing convergence of modelled grounding line movement than advance only simulations, retreat only simulations, or reversibility. Specifically, the region should reduce with finer resolution, and should be vanishingly small at published resolutions. If perturbation experiments are used in future tests of convergence of grounding line behaviour, we advocate that the ~~spinup~~ spin-up method must also be prescribed, as a simple requirement for steady state is not in general sufficient to constrain the experiment design.

This poor convergence is not only a result of inherent difficulties in respresenting a spatial step change in basal drag across the grounding line, but is also due to a temporal forcing feedback involving grounding line movement and basal shear stress. These results apply to marine ice sheet models using a Weertman sliding relation~~, and may be less relevant to~~. The same qualitative features occur with sliding relations incorporating a smoother transition in basal drag across the grounding line, such as through a dependence on height above buoyancy~~.~~, but with a smaller magnitude. A similar convergence issue is raised when imposing basal melt under the floating ice shelf.

Author contributions. Rupert Gladstone designed the experiments and led the manuscript writing. Yuwei Xia carried out the simulations and contributed to the manuscript. John Moore contributed to interpretation of results and writing the manuscript.

11

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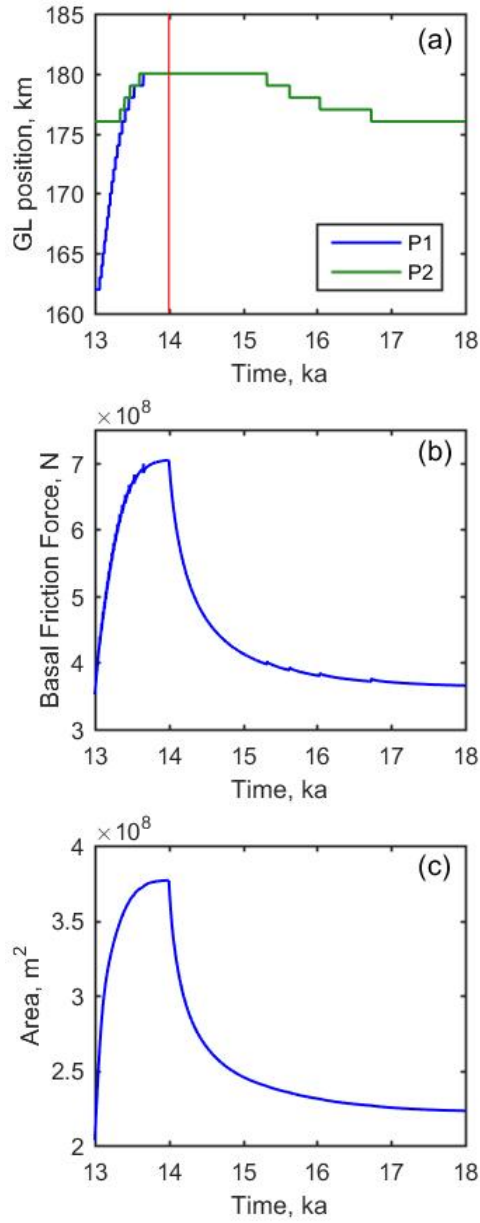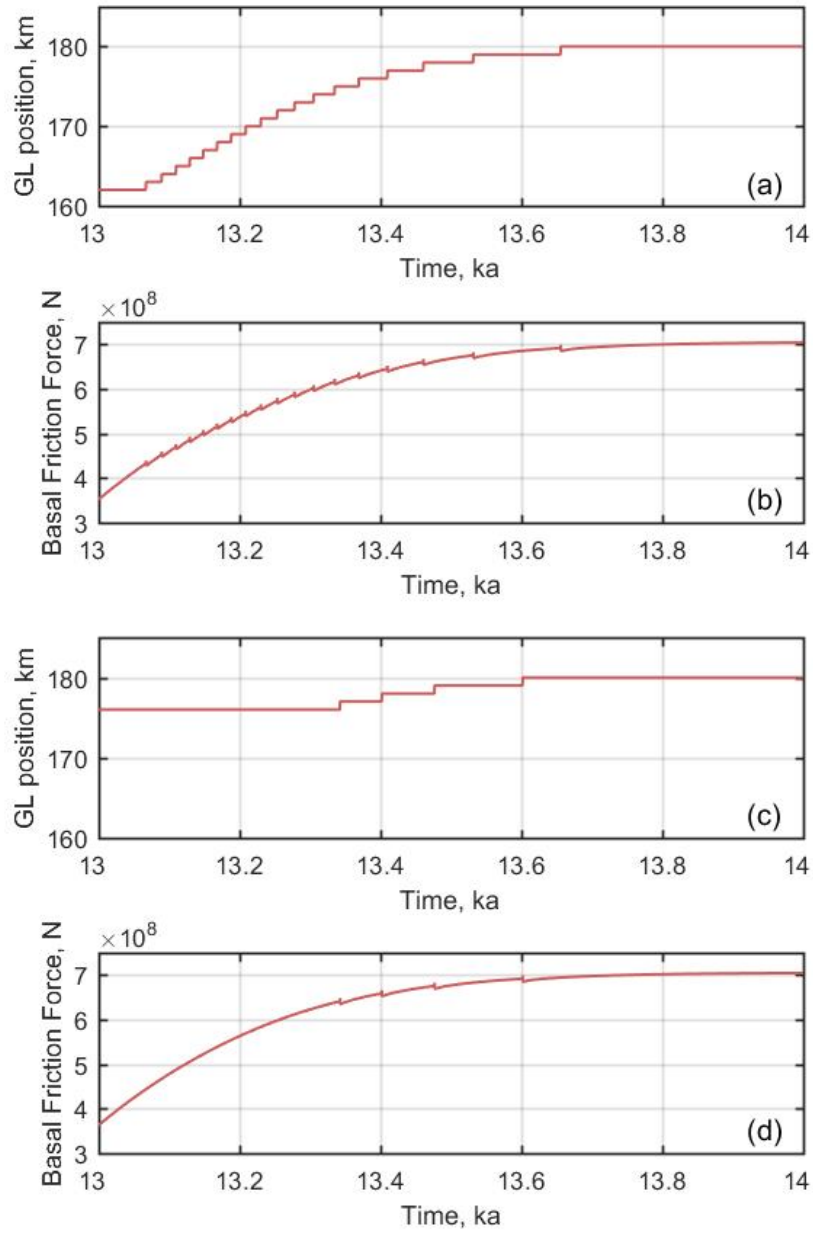
Figure 3. Evolution of (a) grounding line position, (b) total basal friction (this is the basal shear stress integrated over the grounded region) and (c) ice area (the flowline equivalent of volume) for perturbation experiments. Both perturbation experiments P1 and P2 are shown in (a), and the difference in initial states is clearly visible. (b) and (c) show only P1. Both P1 and P2 were run for 1 ka with accumulation rate $a = 0.2$ $a = 2.0$ ma^{-1} followed by 4 ka with $a = 0.2$ ma^{-1}.
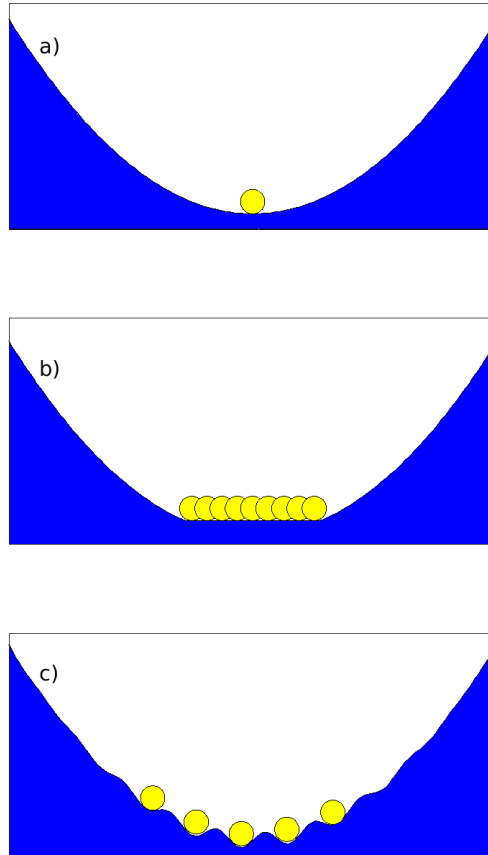
Figure 4. A simple idealised system of a ball under gravitation used to demonstrate types of equilibria: (a) one stable equillibrium is present and the ball will always tend to return to this; (b) a region of neutral equilibrium is present (a region of flat surface), and the ball will remain in equilibrium anywhere on this surface; (c) multiple locally stable equilibria exist, and the ball will tend to roll downhill to the nearest.
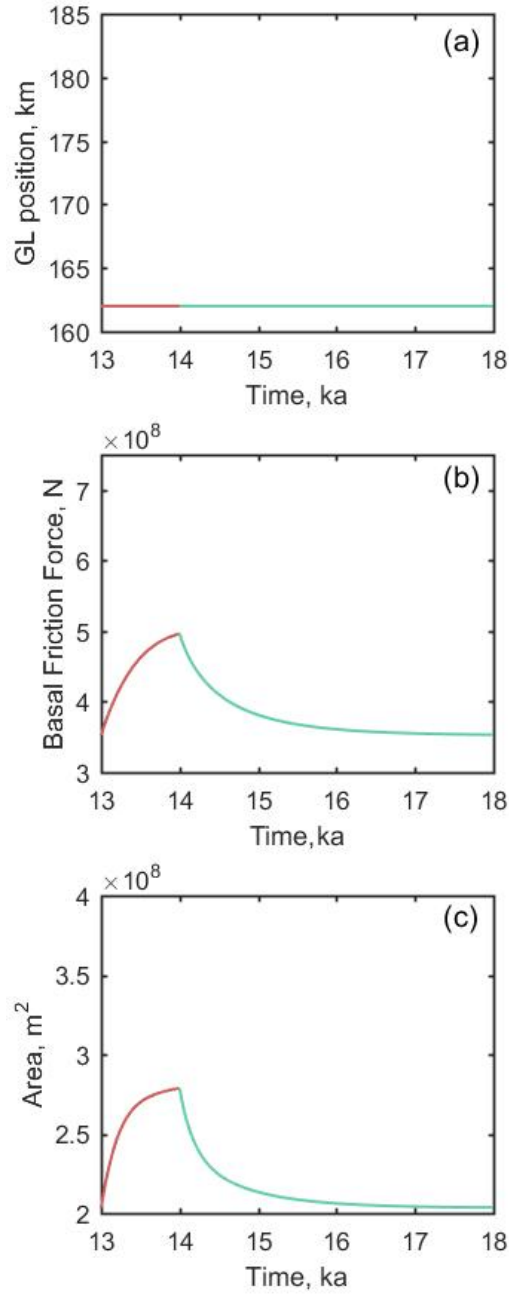
Figure 5. Evolution of (a) grounding line position, (b) total basal friction, and (c) ice area (the flowline equivalent of volume) for the small perturbation experiment PS. PS is identical to P1 except that the magnitude of the perturbed forcing for the first 1 ka is $a = 0.71\,\mathrm{ma}^{-1}$ and for the last 4 ka is $a = 0.7\,\mathrm{ma}^{-1}$.

Figure 6. A closer look at the advance phase of the perturbation experiments. Evolution of (a) grounding line position for P1, (b) total basal friction for P1, (c) grounding line position for P2, (d) total basal friction for P2.