

Interactive comment on “Neutral equilibrium and forcing feedbacks in marine ice sheet modelling” by Rupert Gladstone et al.

Anonymous Referee #1

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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

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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. Here are some specific comments.

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

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(will be used as) "numerical convergence".

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

- 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?

- 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?

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

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

- 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).

- 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).

- 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'.

- 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.

- 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})'.

- 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.

- 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})'

- 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.

- 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)?
- 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.
- 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.
- 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)".
- 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.

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- 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 Szabó 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.

- 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...)

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

- 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

- 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.

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- In Section 2.1, pg. 5, line 4. 'They are run for 1 ka with $a=2.0 \text{ ma}^{-1}$ (...)'. 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.

- Check units and space between number and units in all the text: a) page 2, line 21: -15 C -> -15 °C (please use the default degree symbol of the text editor used) b) page 3, Figure 1: 13ka -> 13 ka; $a=0.7 \text{ ma}^{-1}$ -> $a=0.7 \text{ ma}^{-1}$; $a=1.7 \text{ ma}^{-1}$ -> $a=1.7 \text{ ma}^{-1}$ 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')

- 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).

- page 8, line 26: See -> see

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

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