

Response to Prof. Greve's review

In this manuscript, Cornford et al. report the results from MISMIP+, the third in a series of intercomparison exercises for marine ice sheets. It features a 3D channel geometry that allows buttressing of grounded ice by an ice shelf and a retrograde bed slope in a part of the domain. Several experiments that produce grounding line retreat and re-advance are considered. The study benefits from a variety of contributing models/model variants that cover different treatments of the stress balance and basal sliding as well as different numerical techniques and resolutions. Overall, I found the results very interesting and the presentation adequate. I'd only like to raise some minor issues that should be dealt with as follows:

We thank Prof. Greve for this supportive and thorough review, and acknowledge all of the points made. We reply to each point made (black text) below using blue text.

All multi-panel figures: I'd suggest to label the panels by a, b, etc., and refer to these labels in the captions, rather than using 'top left' etc.

Agreed and done

Line 12, "All ice sheet models are based upon some approximation to Stokes flow": This might be misunderstood as no models use `_full_ Stokes`.

Rephrased to read "All ice sheet models are based upon Stokes flow or, more commonly, one of several approximations to Stokes flow"

Lines 22-24, "community exercises comparing ice stream and shelf models": Seroussi et al. (2019) and Goelzer et al. (2018) are the InitMIP-Antarctica and Greenland papers, respectively. These exercises were about comparing `_ice sheet_ models`, which usually include ice stream and shelf dynamics, but the focus was not on the ice stream and shelf components.

Rephrased: "community exercises comparing ice sheet models where ice stream and ice shelf dynamics are important.."

Line 47: `configuration_s_`

corrected

Caption of Fig. 1: "MIMSIP+" -> "MISMIP+"

corrected

Line 50: Delete comma before "saw".

corrected

Line 59: "about" -> "with respect to" (?)

'About' seems correct to us.

Line 105: "the Weertman (1957)" -> "the Weertman (1957) rule/sliding law"

added "sliding law"

Lines 105-123: Somewhere around here the findings of the study by Gladstone et al. (2017, <https://doi.org/10.5194/tc-11-319-2017>) should also be mentioned: keeping the basal friction continuous across the grounding line is beneficial for simulating marine ice sheets/grounding line dynamics. Weertman-Budd-type sliding (their Eq. (1)) is another alternative to achieve this.

Added a footnote to acknowledge this paper while keeping the flow of the paragraph

5 Line 254: Missing space after "years".

corrected

Lines 273-274: The description of the Úa model could be a bit more comprehensive (along the lines of the other descriptions).

A more comprehensive description was included

10 Figure 3, right panel: If I interpret this correctly, some of the results fall outside the required interval of 450 ± 10 km. Does this have any consequences?

Yes - this is why the Weertman models have a wider spread of retreat rates. This is discussed in the paragraph beginning 'One major division'

Line 294: "lines intersect with"

corrected

15 Line 325, " 450 ± 15 km": In section 2.2, a requirement of 450 ± 10 km was formulated. How does this go together?

Added a footnote: The requirement that $x = 450 \pm 10$ km was relaxed since many participants neglected it.

Lines 327-329: What are "distinctive" vs. "conventional" numerical methods/treatments?

We removed these adjectives

Lines 426-433: I'm not sure whether I got this right. Is the recommendation that subgrid melt schemes should not be used?

20 We do indeed think that such schemes should be avoided, or at least treated with great care.

Lines 439-440: "the distinction ... even between approximate models and full Stokes models, was minor": Fig. 9 (left panel) showed that there was a distinction ("much lower rate of advance"). This seems to be contradictory.

25 This was rephrased and a discussion of the full Stokes model has been added. The picture is not quite as simple as saying the Stokes models are different and better (though that is of course one possibility). Notably, although there are point of disagreement between the Stokes and non-Stokes models, there are also differences of similar magnitude between the two Stokes models (Elmer/Ice FS and ISSM FS). Elmer/Ice FS shows essentially the same rate of retreat in Ice1r as the SSA/HO/LILx models, while ISSM FS shows the same average rate but a much greater initial rate and lower final rate. Elmer/Ice FS shows no re-advance at all, which seems likely to be a numerical issue, while ISSM FS shows re-advance that is slower than average but comparable to its ISSM SSA counterpart.