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Interactive comment on "A comparison of two Stokes ice sheet models applied to the Marine Ice Sheet Model Intercomparison Project for plan view models (MISMIP3d)" *by* Tong Zhang et al.

Anonymous Referee #1

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1 General statement

The manuscript "A comparison of two Stokes ice sheet models applied to the Marine Ice Sheet Model Intercomparison Project for plan view models (MISMIP3d)" by T. Zhang compares the results of two finite element full-Stokes models on a widely used benchmark designed to assess the accuracy of models to represent grounding line evolution. It describes the differences in the numerical implementation of the two models: type of elements, friction applied around the grounding line, choice of floating and grounded areas. The manuscript concludes on the importance of including more than one full-Stokes model in intercomparison projects to provide a measure of their

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Discussion paper

uncertainty. The paper is clear, well-written, and the figures usually appropriate. However, the conclusions presented do not accurately reflects the results presented in this manuscript.

I have several major concerns with the manuscript. First, I am questioning the novelty of this paper. The full-Stokes treatment of grounding lines has been investigated for almost 10 years, and a recent paper by Gagliardini et al. (2016) discusses the problem of numerical convergence due to the treatment of basal friction. The only new element introduced here is the development of grounding lines within FELIX-S, which I believe would be a better fit for Geoscientific Model Development.

Second, I found the abstract in particular to be misleading, and to put emphasis on the "wrong" solutions to real questions. As mentioned by the authors, "as grid resolution increases the grounding line positions for FELIX-S and Elmer/Ice appear to converge", which suggests that full-Stokes models (just like any numerical model) should do a convergence study to assess the impact of grid resolution on their results. The authors on the opposite suggest that "future model intercomparisons using full-Stokes models as a metric should include more than one model, to provide both additional confidence in the results from full-Stokes models and a measure of their uncertainty". Comparing two different full-Stokes models will not allow quantifying the error in any of these models. The only way to assess the error of each model is to do a convergence study, with varying grid resolution, to see if the results are converging, and if so, to quantify the error associated to the discretization. To my knowledge, this has never been properly done in three-dimensional models, and needs to be done instead of writing numerous papers on variations along the same subject (comparison of friction treatment around the grounding line, comparison of several full-Stokes models, ...).

Another point that I think should be emphasized in this manuscript is that not only is the resolution in the along-flow direction important, as noted in many previous studies (Durand et al., 2009; Pattyn et al., 2012; Pattyn et al., 2013), but also the across flow resolution matters for grounding line advance and retreat. So instead of just mentioning the difference between the two models is reduced for increased “grid resolution” (currently what is said in both the abstract and the conclusions), the authors should clearly state that across flow resolution is also an important factor, which has not been really considered in the previous studies.

Finally, the results presented in this paper do not present major new results, as the conclusions are somewhat similar those of Gagliardini et al. (2016), which already discuss the impact of different treatment of basal friction around the grounding line. As errors in full-Stokes models start to be better estimated, and lower order approximations models also improved grounding line treatment during the past few years (Feldmann et al., 2014; Seroussi et al., 2014), a more interesting study would be to reassess the difference between full-Stokes and lower order approximations based on all these new data.

2 Specific comments

The convergence or absence or convergence with grid resolution should be rigorously assessed using a convergence study instead of providing statements like “appear to converge” in the abstract.

The description of the friction coefficient in section 3 is not clear. Figure 1 is also confusing, especially the friction coefficient along profile 2. This seems to be a straight line while there is a node in the middle of the profile. The details provided in the discussion should be moved in this section (p.5 l.15-16).

p.8 l.3-7: For which model is there a 5 km difference? This paragraph also mentions that this distance is expected to decrease with increased grid resolution. Why not do these runs and provide an accurate answer? These positions should be the same for a model that has converged with grid resolution. So here again, this seems to suggest that the results presented have not converged. How long is the steady-state run for?

Could this difference be partly explained because the steady-state should be run longer to fully converge?

p.8 l.22-23: "the agreement between FELIX-S and Elmer/Ice increases for all of Elmer/Ice GL implementations". This seems like another sign that the differences in the results are caused by the non-converged aspect of the results presented.

3 Technical comments

p.1 l.2 (and other places in the text): full Stokes is commonly used in the literature, so there is no need to use "full" Stokes.

p.1 l.13: "appear to converge": the convergence should be better studied to assess if there is convergence with grid resolution.

p.2 l.4: "inherent dynamic instabilities associated with marine-based ice sheets". The marine instability should be briefly explained in a couple sentences.

p.4 l.28-30: It should be stated here that errors in full-Stokes models and lower-order approximations should be accurately characterized to assess if the differences are within the error margins or if these models do lead to different results.

p.5 l.2: "Finite Element Methods" → "the Finite Element Method"

p.5 l.30: What about the vertical resolution used in both models?

p.6 l.4: "the prognostic, basal sliding perturbation experiment (P75S and P75R)": is that one or two experiments?

p.6 l.20: What resolution is used for the diagnostic experiment?

p.7 l.21: What is the vertical resolution?

p.6 l.11: Replace "S", "R", ... by the experiment name.

p.8 l.15: Why are the grid different for these experiments? If it is possible to use the same grid for experiments Stnd and P75D, it should also be possible to use a similar grid for the other experiments (especially as they are much shorter than the Stnd experiment).

p.8 l.21: “the GL improves”: do you expect the grounding line to come back to its initial position on such a short time scale?

p.8 l.27-29: “We attribute ...” I attribute it to the coarse grid used in the across flow direction.

p.9 l.13-23: This paragraph should be moved to section 3.

p.9 l.20: “basis functions”: a regular grid is used so the basis functions should have similar properties.

p.10 l.32-33: I don’t agree with the conclusion that “two or more full-Stokes models should first conduct their own intercomparion”. I think that just like any numerical model, they should make sure that their results are not grid dependent and provide an error associated with their results.

p.11 l.4: What does “a more direct comparison between models” mean? The two models are already sharing lots of parameters, even the same grid. This should be detailed and not just mentioned.

p.14 Tab.2: There are no Stnd results (x_{GO}) provided for the 40 and 80 elements in the y direction. So what are the initial states used for the P75S experiments in these cases?

p.15 Fig.1: The friction coefficient used along the profile is not totally clear, especially for Profile 2 (it looks like a straight line, which is surprising).

Fig.4, 5 and 6: Thin (or thick) curves should be used consistently for the 4 panels to represent the same FELIX-S results. Also consider adding letters (a to d) for the panels, as mentioned in the captions.

Fig.7: How can an absolute value be negative (right panel)?

4 References

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