The Cryosphere Discuss.,
doi:10.5194/tc-2016-149-RC1, 2016

Interactive comment on "Marine ice sheet model performance depends on basal sliding physics and sub-shelf melting" *by* Rupert Michael Gladstone et al.

VC Tsai (Referee)

tsai@caltech.edu

Received and published: 8 August 2016

In their manuscript, the authors describe a couple of numerical tests that show how the resolution required of the numerical ice sheet model Elmer/Ice depends on the way in which basal sliding and sub-ice shelf melt rates are parameterized. The results shown seem to agree with previous suggestions that the required resolution should depend on how smoothly the stresses change across the grounding zone, and the current manuscript demonstrates this, making it a useful contribution. However, I had a number of significant concerns about both the numerical results themselves as well as the general applicability of the results to ice sheet modeling. On the numerical side, it seems impossible for the steady-state spin up simulations to achieve a resolution-independent

grounding line whereas the steady-state after forcing is resolution-dependent since both are just steady-state solutions. This strange result brings into question the validity of the results and needs to be explained. On the applicability side, since the parameterizations used are not physically motivated, it is not clear if the results are truly relevant for real ice sheets. The authors should at least discuss this point. In addition to these two major points, the main results could be better illustrated, especially Fig.3 in which more detailed information could easily be provided. Once these points are addressed, the manuscript could be a nice contribution to the literature. More specific comments are below.

The one major concern I have regarding the numerical results is how the steady-state spin up simulations can be resolution independent whereas the forcing steady-states are resolution dependent. My understanding is that the spin up simulations should be just one particular choice of a forcing experiment, where the steady-state solution is achieved by the end of the spin up. If this is true, the same 'forcing' that is used to create the spin up should result in the same steady-state solution as when used in a forcing experiment. Thus, at a given resolution, the solutions should be the same, whether they agree or disagree with solutions at other resolutions. Apparently, this understanding is incorrect since the spin up simulations are claimed to generally have the same steady state but the forcing steady states do not. Since this defies expectation, the authors need to explain this, i.e. why the spin up simulations cannot be thought of as a particular forcing (or in other words, why is the end of a retreat/advance simulation not equivalent to the end of a spin up simulation?). The authors do state on p7L15 that the melting forcing may cause the resolution dependence, but this does not explain Fig3a. It may also be useful if the authors are more precise in their statement on p6L6 that the spin up simulations "do not vary significantly with resolution". How significantly? Are they within a single grid spacing?

The other significant concern I have is regarding how applicable the results are. The authors choose to test 3 sliding laws (SR2-4) that were proposed 30 years ago, where the effective pressure dependence is inserted in an ad hoc manner, despite the existence of at least 2 more recent parameterizations of basal sliding (Schoof 2005 and Tsai et al. 2015) that are more physically motivated. Perhaps it is easier to test the 3 sliding laws used, but if they do not describe the physics properly then they are not relevant to ice sheets. At a minimum, the authors should at least explain why they choose the 3 sliding laws and comment on whether the results can be expected to hold more generally or not. For the melt parameterization, the authors also seem to arbitrarily choose a smoothly decreasing melt rate for no apparent reason. In reality, if anything, at many grounding lines, the melt rate is expected to increase rather than decrease, so the relevance of a smoothly decreasing melt rate is not clear. Are there ice shelves where basal melting decreases towards the grounding line? This should be commented on.

Additional comments:

P2L17: Feldmann et al. 2014 should be cited in this paragraph as well.

P3L1: “The starting point” could be rephrased for clarity.

P3L9: Missing half sentence.

P3L16: Since the C’s in the different equations are actually different, different letters (subscripts?) should be used. Otherwise it is confusing.

P5L8: Sentence wording is confusing. Sounds like a given model’s horizontal resolution is spatially variable, rather than what is intended with 3 different models with different resolution.

P5L21: Should this be 1000km rather than 150km?

P6L12: There are multiple shear stresses. Need to specify which one.

P6L15: Throughout this section, it would be useful if the authors made it easier for the reader to follow which simulation is referred to. For example, the acronyms could

be spelled out once in the main text, the main text could refer to the colors in the figures, and also refer specifically to the figures and figure panels in which the results are shown.

P6L15: Whether or not the simulations (particularly SR2 and SR3) actually achieve resolution independence is difficult to see in Figure 3, partly because it is hard to see the step size. Since the steady-state solutions are the only ones that need to be compared, and contain the useful quantitative information, I would suggest making subplots for each SR# experiment that plot zoomed-in steady-state grounding line location (on y-axis) vs. mesh resolution (on x-axis). It would be preferable if there were more than 3 points, but if that is all that is computationally feasible, that is understandable.

P8L12: Sentence is confusing. In fact, much of this page (L3-L20) is not coherent, and I would suggest the authors reword for clarity and flow.

P8L29: As commented earlier, since the sliding laws used are not physical, it is not clear that the results are easier to interpret than those of the Gagliardini study.

Fig.1: The point of Fig.1 is not entirely clear, and the gray overlay makes it impossible to see what is stated about there being little vertical shear.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-149, 2016.