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

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Overview:

This paper shows that grounding line migration can be simulated with coarse resolution marine ice sheet models that implement sliding laws that force the basal shear strength to smoothly decrease to zero and force basal melt to smoothly decrease to zero near the grounding line. This study builds upon–and sometimes challenges–the growing body of literature that examines numerical accuracy and convergence in ice sheet models. The premise of this study is that the grounding line position (and migration) determined in numerical marine ice sheet models can depend greatly on model resolution. This raises the concern that grounding line position ultimately depends (unphysically)

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on grid resolution. This is an important topic in glaciology and ice sheet modeling that is appropriate for The Cryosphere. Overall, despite some lengthy questions about aspects of the study, suggestions for some additional work and stylistic/figure improvement, I think this study is an important contribution to this growing field. Below I outline some points that I found confusing along with some suggestions for improvement.

Concerns

1. What is meant by "grid scale dependence"?

Models usually display "grid scale dependence" for one of two reasons. The first reason is that the resolution of the model fails to appropriately resolve the fundamental scale in the physics. This leads to a numerical model that fails to accurately represent the underlying physics and, not surprisingly, inaccurate numerical solutions. From a physicists perspective, there is little of physical interest in this situation and the poor accuracy can be cured by merely increasing the model resolution until the fundamental scale is appropriately resolved. From the perspective of a numerical modeler however, resolution may be severely limited by computational resources and increasing the numerical resolution sufficiently may be challenging, require clever tricks and/or much bigger computers. Sadly, most climate models live in this regime and this often necessitates clever parameterizations for sub-grid scale processes. A second reason a model may display "grid scale dependence" occurs when the model results actually fail to converge (or even diverge) as resolution increases. This leads to a true grid scale dependence where no amount of grid refinement can cure the divergence. This is, of course, unphysical, and usually caused by a breakdown in the model physics. This can point to a problem in the equations solved (e.g., ill posedness) or can result from a model that has been applied to a length scale where the model no longer applies. For example, the stress field near sharp cracks in elastic mechanics formally diverges. This is unphysical and actually leads to poor numerical convergence in numerical models that don't explicitly account for the crack tip singularity. The singularity in the stress field is caused by a break down in the elastic assumption and can be cured by modifying the physics to include a plastic process zone near the crack tip. This example is relevant because in the elastic crack case numerical models simulations will exhibit grid-scale dependencies. This dependency can be cured by either incorporating the singularity explicitly in the numerical scheme (appropriate for length scales larger than the process zone) or by physically curing the singularity by modeling the process zone (appropriate for length scales on the order of the process zone or smaller).

The reason for this long digression in my review is that it is unclear to me which type of grid scale dependence the authors are invoking. Previous work (see, e.g., the work by Schoof on grounding zones) suggests that the problem with resolving grounding zones is "merely" a numerical resolution issue and not a physical issue. Some of the statements by the authors instead seem to indicate that they think the sliding law and basal melt parameterizations themselves are to blame. I think readers would appreciate a more precise discussion of the type of grid-scale dependence and its connection to physics. My understanding is that the authors are primarily discussing the first type of grid-scale dependence, but the relationship between the grid-scale dependence and incorrect model physics would be improved.

2. Physical appropriateness of the sliding laws: The previous point leads me to a more fundamental question: the authors introduce 4 sliding laws. The first is the standard "Weertman" sliding law often used in ice sheet models. The remaining sliding laws represent various heuristic generalizations of Weertman sliding to include effective pressure. I have no objection to picking a subset of the infinitely large number of possible sliding laws. However, what is missing is a discussion of why the authors think the various generalizations of Weertman sliding are more appropriate for more realistic ice sheet models. This is especially true when considering the simple assumption employed by the authors that effective pressure is a linear function of water depth **all the way to the ice divide**. There are adequate measurements at this point to show that effective pressure is not simply related to bed topography. As the authors clearly state, subglacial hydrology is beyond the scope of this study. Nonetheless, it is unclear to

me why a sliding law that incorporates an unrealistic effective pressure is always more accurate or appropriate than a model that ignores effective pressure. In other words, why is it better to assume effective pressure varies like water depth as opposed to the (implicit) assumption in the Weertman sliding law that effective pressure is constant? To be clear, effective pressure proportional to water depth is often invoked because it forces basal shear stress to decay to zero at the grounding line, but there is no clear physical reason why this decay is required to occur over a length scale determined by bed topography as opposed to basal hydrology (or other processes). The fact that one sliding law is more convenient for numerical modelers doesn't necessarily make it the most physically appropriate sliding law. See point 1. Here I think the authors case would be strengthened considerably if they could provide first principles or–I think this is easier–empirical reasoning to suggest one or more of the sliding laws is more plausible than others. For instance, the 4 sliding laws predict very different ice sheet profiles with different divide thickness, slopes and even concavity of the profile. Can we say anything about the reasonableness of the sliding law assumptions based on observed ice sheet profiles (Pine Island?, Thwaites?, Lambert Basin, Siple Coast Ice Streams?). My guess is that one might find different sliding laws appear more appropriate for different portions of the ice sheet? I understand the experiments are necessarily idealized, but the argument that the results presented here are relevant to actual marine ice sheets will be much stronger if the the authors could show that the parameter regime they examine resembles an actual ice sheet.

3. Selection of numerical parameter: Another question along a similar line of reasoning is how did the authors select the numerical "C" coefficients in the sliding laws? Are they chosen so that the average basal shear stress across the ice sheet is the same for all models? So that the basal shear at a particular velocity and effective stress matches? So that the profiles are as similar as possible? So that the ice sheet profiles are qualitatively similar to some section of an ice sheet? I can imagine that it may be possible to take a single sliding law, like the Weertman relation (SR1) and obtain different convergence results depending on the value of the sliding coefficient. (The

boundary layer size is a function of the sliding coefficient and so one could presumably make the problem easier or harder based solely on varying the C value.) Are the results that the authors show a function of the form of the sliding law or the magnitude of basal friction (or both)?

4. More detailed explanation of the results: Getting into the actual experiments, I'm puzzled by the fact that the authors spin up the models and find a steady-state that is independent of resolution, but then find that some of the forcing experiments do yield a dependence on grid resolution. Is this because the models only display grid resolution dependence when buttressing or basal melt is implemented as a forcing and not otherwise? Either way this would appear to indicate that grid sensitivity depends not only on the parameterizations considered (basal melt and sliding), but also on the type of forcing. This suggests that dependence on grid resolution is far more complicated than indicated and depends not only on the model physics, but also on the external factors driving change. I would like to see this commented on and explained in more detail in the manuscript.

5. How generalizable are the results: Similarly, I would urge the authors to dig a bit deeper into the physics behind the results. The authors perform a suite of idealized experiments using a particular geometry, set of parameterizations (sliding and basal melt) and forcing and find some of these combinations are more amenable to simulation in low resolution ice sheet models than others. It is, however, not straightforward to translate these results to other, more physically realistic situations. Would the results hold if instead of water depth dependent effective pressure, a model calculated effective pressure based on a subglacial hydrology model? Here I strongly encourage the authors to dig deeper into their results and guide us in interpreting them. I suspect that many of the results can be anticipated by a analytic consideration of the structure of the transition zone near the grounding line for the different sliding laws. My (perhaps naive) expectation is that models need a grid spacing that is small compared to the length scale of the transition zone between vertical shear and horizontal spreading

dominated flow regimes.

Comments on presentation

The writing in the manuscript is generally clear, but stylistically, the manuscript contains many paragraphs that consist of one or two sentences. The abstract, for example, contains 4 distinct paragraphs! Normally, abstracts are a single paragraph. Traditionally, at least in the US, paragraphs are units that contain coherent ideas. Paragraphs depend on each other, but paragraphs themselves should be coherent. I found it jarring to have indentation that has little relation to the organization of ideas. For example, on Page 4 line 9 contains a paragraph with the single sentence starting with "This parameterisation is similar to that used in the . . ." The next paragraph begins with a definition of a variable. Neither of these are coherent or can stand on their own. This peculiar isolation of sentences into incoherent paragraphs was jarring to my American sensibilities, but this is perhaps a European or Cryosphere Discussion stylistic preference. I leave it at the editors and/or authors discretion as to whether this is stylistically appropriate.

I was also flummoxed by the bestiary of acronyms. I was confronted with ISM, MIP, MIS all in the first several paragraphs and later hard to deconstruct SR1 and SR2, ALMW, ALMN amongst what seemed like many others. I have strong personal preferences to avoid all unnecessary acronyms and, unless the authors are forced to pay by the word, I urge the authors to use complete words instead of acronyms when ever possible. If this is inconvenient or impossible, then I would suggest a table that readers can easily refer to that provides us with a dictionary of acronyms that we can conveniently look up.

My last comments on presentation concerns the figures, which although physically appealing, were difficult for me to parse. For example, I struggled to understand Figure 1. According to the caption, panels show the steady and final states for the basal melting experiment. However, the top panel doesn't have the gray shaded profile. Why not? Moreover, given that the shaded gray profiles obscure parts of the initial profile, I would prefer to see this figure broken into two figures. The first would show the initial, post spin-up profiles. The second would show the final, post simulation profiles (with velocities). This would allow readers to more easily be able to see the differences in the shapes of the initial and final profiles and contemplate the role that the sliding law in modifying the morphology of the profiles and the transition zone. This figure also needs a scale bar to show horizontal distance (and possibly vertical elevation).

For Figure 2, why are only 2 steady states shown in Figure 2? Why not show all of them (as in Figure 1)?

Figure 3 was difficult for me to parse. I would urge the authors to considering using color schemes that are more appropriate for the color blind and/or line types that don't depend as much on color.

Typographical comments:

page 1 near line 10: even with –> even with

page 3 above line 10: incomplete sentence "These values for p and q are chosen for simplicity, and deviate" ????

From a physical perspective, Equation 2 is problematic. It assumes that the ice sheet has a basal hydrology sentence that is perfectly connected with the ocean all the way to the ice divide.

The constant "C" in Equations (3)-(6) has different units in the suite of basal drag parameterizations. To avoid confusion, I recommend using a different symbol or appending subscripts to more clearly indicate that the numerical value of C (and its units) are not identical in all experiments.

Line 4, line 15 and elsewhere: need a space between the number and unit "100m" –> "100 m"

After section 2.3 "1800km" –> "1800 km"

Accumulation rate: What is the motivation for the accumulation rate defined by Equation (11). This appears to predict linearly increasing accumulation with zero accumulation at the ice divide? Why not use a constant accumulation, which would seem to be more realistic for much of Antarctica?

Spin-up is independent of resolution. This seems to indicate that resolution only matters in some experiment types???

Page 6, line 6 "focusses" –> "focuses"

Page 6, line 7 missing commas –> "The spinup simulations do however vary" –>The spinup simulations do, however, vary"

Page 6, line 21: "But even SR4b still shows significant resolution dependency." –> sentence fragment, consider revising.

Page line 23: "Since" indicates time (e.g., I haven't slept since yesterday). I think the authors want "Because" (Because SR1 and SR4a . . .)

Page 7, line 4 missing space: "20ka" –> "20 ka"

Page 7, line 29, "Any resolution dependence in a model is inevitably non-physical. Ideally model behavior should converge with finer resolution." This line is perplexing and indicates my fundamental misconception. Are the authors arguing that grounding zone position fails to converge with increasing resolution and is, hence, resolution dependent or are they arguing that grounding line position requires finer resolution than they have available. There is no particular guarantee that a numerical model with a priori specified resolution will be an accurate representation of a given set of partial differential equations. This is especially the case when the resolution of the model is more coarse than the fundamental scale of the system.

Page 8, Line 10-15: I don't think it is true that basal melting *requires* subglacial discharge. Because the ice near the grounding line is (usually) located at a depth that is much greater than the melting point of sea water, one expects melt near the

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grounding line of ice shelves with deep grounding lines, even in cold cavity ice shelves. In the traditional ice-pump theory, the cold melt water mixes with sea water and forms a buoyant plume. I'm puzzled by the argument that subglacial discharge is required to initiate the process? Are the authors arguing for massive super cooling near grounding lines? Is this usually observed? Models of submarine melt under ice shelves rarely include grounding discharge and yet predict realistic patterns of basal melt. Why is this if submarine discharge is a crucial component of the process? It also seems like the relevant length scale over which basal melt must go to zero is going to related to the characteristic width of the buoyant plume. Can this be estimated and used to better constrain the parameterization of basal melt?

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