Referee Comment on 'The predictive power of ice sheet models and the regional sensitivity of ice loss to basal sliding parameterisations: A case study of Pine Island and Thwaites Glaciers, West Antarctica'

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Summary

In this manuscript, the authors present the results of sensitivity study in which they vary the choice of sliding parameterization along with the numerical values of the associated parameters and evaluate the different solutions that result for a pair of important glaciers in the Amundsen Sea embayment under several idealized geometric perturbations. They find that there are broad similarities in the trajectories of models using these different parameterizations, with the overall sign remaining the same for all cases, but with some qualitative differences as well as some regional peculiarities.

I found the paper to address an important question in ice sheet modelling and to be reasonably easy to follow. The results are interesting and provide some degree of comfort that, even though as modellers we are generally ignorant of the appropriate way to model sliding, there is some hope in West Antarctica that the models are still producing useful results. I have a handful of specific comments and concerns - some more serious than others - which I have included below.

Comments

L21 Define effective pressure here rather than on line 33.

L58 The initialism 'ASE' is first defined on line 88, below where it appears here.

L65 It doesn't really make sense to say that a higher m produces higher velocities unless it's qualified by a description of what's going on with the friction coefficient, which is, in this work and others, treated as an unconstrained spatially-varying free parameter. Can this notion of velocity increasing with m be made a bit more rigorous?

L97–99 There are a few too many commas in this sentence.

Eqs. 1–3 These equations are defined again later, albeit in slightly different form, which is a bit confusing. I understand why they are here for exposition, but maybe they could either be described qualitatively, or not numbered, or something else to make clear that they are not necessarily the equations actually implemented.

L103 While most readers of this paper will likely already understand the assumptions of the SSA, for completeness they should be briefly mentioned here.

Eq. 4 Probably not necessary to include this, unless you are going to state the full set of governing equations. I think it's okay just having it in the Ua reference.

L108 What flux and stress conditions are imposed at the inland boundary?

L109 It is not very clearly described how calving is treated in this model. Is the calving front held fixed or evolved? How?

L112 We're not given any context into how these densities are used (and are not mentioned again in the text). These should either be omitted or the full model needs to be described.

Eqs. 6–10 It's not clear to me what the practical difference is between C and μ. While they may have different physical interpretations, both would appear to be friction coefficients. However, in this paper, one is treated as a spatially varying field to be inferred from velocity observations while the other is a scalar parameter. It would be helpful to try to clarify this in the text. This also implies that different sliding laws have substantially different degrees of flexibility and, as a result presumably exhibit different degrees of fidelity to observed surface velocities. It would be worthwhile to comment on this issue as well: are the differences in evolution really the result of the different sliding laws, or is it because some laws match observed velocities better than others?

Eq. 7–10 I think that the redefining of β^2 is a confusing choice of notation, given that β^2 usually means what you call C. Furthermore, this substitution makes the dependence of the various laws on the sliding exponent m much less transparent. I suggest writing the laws in a way that makes this dependence more plain (perhaps by just substituting the given expression for β^2 back into the equations).

L130–131 As written, the Budd law only becomes similar to the Coulomb law for high m when $q = m$, which these experiments don't explore.

Eq. 12 The choices made for $p_{k,prior}$ need to be shown here. Is it a spatially varying field or is it a scalar? how is it selected? Furthermore, how well the resulting velocity solutions correspond to observations (and to one another between sliding laws) needs to be quantified here.

L143 Total area of what? Perhaps introduce a symbol (Ω is frequently used) to represent the model domain.

L148 I don't know what it means for the system to 'settle', nor is it clear what it means for a strongly non-linear system as this to 'evolve steadily'. The manuscript needs to include a more detailed description of the criteria used for assessing whether the so-called 'transients' associated with data-model incompatibility in time-dependent simulations have reached a sufficiently small degree of influence so as to render the resulting simulations plausible.

L152–157 I would suggest using the 'description' LaTeX environment (or equivalent) here rather than 'itemize'; the two different hyphens look weird together.

L165 Median might be a better choice than mean here, since the sample contains some outliers (e.g. Budd $q = 1, m = 7$).

L170–174 It would be better to be specific about what change was made to convince the solver to converge so that a reader can properly assess the impacts that such a change would have when compared to the other simulations.

L186 and elsewhere Perhaps two significant digits is sufficient when reporting percentage changes.

Fig. 5 This may be a comment on Ua more than the present manuscript, but Fig. 5 shows clear indication of numerical artifacts in the thickness field (presumably the result of an unstable continuity discretization). If this is indeed the case, this needs to be mentioned in the text and the potential implications mentioned (which might be significant: after all, the flux across the grounding line depends quite sensitively on the thickness there).

L245–246 It is not immediately clear why the oceanic pressure condition shouldn't apply away from the grounding line. In any case, a reference for this claim is needed.

L254 typo: 'deivation'

L316–324 It might be worth mentioning that this effect of faster transport of upstream ice to the grounding line is probably a strictly transient phenomenon.

L358–362 The conclusion that the sliding law choice doesn't matter much over centennial time scales is reasonably well supported by the results of this paper, but this might not be true in non-WAIS scenarios; it's worth remembering that this is a system that is already undergoing a strong and non-linear transient response and may be much less sensitive to the types of choices explored here than other places. I think that the language of this section needs to be tempered a bit to reflect this.