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Interactive comment on "The sensitivity of flowline models of tidewater glaciers to parameter uncertainty" *by* E. M. Enderlin et al.

Anonymous Referee #2

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In "the sensitivity of flowline models of tidewater glaciers to parameter uncertainty" Enderlin and co-authors present the impact of slightly modifying the parameters describing the viscosity and basal friction law on the prognostic evolution of a tidewater glacier flowline model. The work is well presented and deserve publication although the results are not surprising and qualitatively obvious (close to a tipping point, the evolution of a system is extremely sensitive to the description of its initial state). Recommendation is also pretty straightforward, establishing modeling projections without sensitivity study is somehow meaningless (but I agree that it is not always done).

I have two main suggestions.

- Rate and enhancement factors are by definition tied together as they affect similarly the effective viscosity (eq. 1 of the paper). I do not clearly understand the motivation

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to share the sensitivity study between this two parameters and not directly discussing the sensitivity of the model to the estimation of ice viscosity.

- I strongly believe that the authors can got one step forward into their conclusion and recommendations. They demonstrate that the description of an outlet glacier at a given time is not enough to properly model its evolution, particularly when resting on an over-deepened bedrock profile. But if you have access to data at different times, you may strongly reduce the uncertainties in the projections of the model. This could be illustrated using figure 5 en 6. In other words, ice sheet models will have to use hindcasting (together with sensitivity tests) to improve the robustness of their projections. I would suggest to discuss that point (and refer to Aschwanden et al. : Hindcasting to measure ice sheet model sensitivity to initial states, The Cryosphere, 7, 1083-1093, doi:10.5194/tc-7-1083-2013, 2013).

Minor comments.

- p 2570, line 2 "Large scale ice-sheet models...., unable to incorporate dynamic calving front variations". I believe this is not correct. It may be true for the 3 referred models but other large scale models have dynamic calving front (e.g. Levermann et al.: Kinematic first-order calving law implies potential for abrupt ice-shelf retreat, The Cryosphere, 6, 273-286, doi:10.5194/tc-6-273-2012, 2012). I think it should be rephrased.

- p 2571, line 22-25. "Although an increase in lateral ice flow convergence can limit this positive feedback and stabilize the grounding line on a reverse bed slope for ice streams (Gudmundsson et al., 2012), this stabilizing mechanism may be absent for outlet glaciers that are confined by bedrock walls along their lateral margins". This justification looks a bit awkward to me, as a stabilization mechanism may be absent or not, I do not think that someone made any demonstration on that point. Gudmundsson and co-authors only show that marine ice sheet instability is not systematic on a reverse slope. But it can occur depending on the geometry. This sentence should be rephrased.

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