Response to reviewer comments for "The sensitivity of flowline models of tidewater glaciers to parameter uncertainty" by E. M. Enderlin et al. (2013).

Reviewer comments in bold. Response to comments in normal font.

Review #1

Major Comments:

1. In a depth-integrated model, however, the effect of the enhancement factor and the rate factor are essentially the same as they are multiplicative in the equation for viscosity. If the authors prefer to stick to the three parameters, I suggest to at least discuss this more carefully. What roles play other parameters like choice of sliding law, parameterization of effective pressure, and surface mass balance, to name a few?
We have expanded our model description to clarify that other parameterizations, such as that used to describe the effective pressure, will also influence simulated ice flow. We have also clarified why the rate and enhancement factors are treated separately: "While the rate and enhancement factors are similar in their effect, we treat them separately here because they are used to parameterize independent properties of the glacier ice (i.e., temperature and anisotropy, respectively). Further, although the rate factor can typically be constrained by measured or modeled ice temperatures, the anisotropy of the ice is often unknown. As such, in simulations of real glacier systems, the enhancement factor is tuned independently in order to reproduce measured strain rates (Cuffey and Paterson, 2010, p. 71)."

Technical Comments:

p. 2568, l. 2: "predict future" sounds like a tautology to me. I suggest to remove "future".
Revised so that "future" has been removed from all sentences regarding prognostic models.

p. 2568, l. 3-4: The sentence implies that a physically-based calving criterion only exist for flowline models. Rephrase.
Reworded: "Depth-integrated (1-D) flowline models have been widely used to simulate fast-flowing tidewater glaciers and predict change because the continuous grounding line tracking, high horizontal resolution, and physically-based calving criterion that are essential to realistic modeling of tidewater glaciers can easily be incorporated into the models while maintaining high computational efficiency."

p. 2573, l. 23: Change "warmer" to "higher". Values cannot be warmer or colder only higher or lower.
Changed.

p. 2574, l. 2: Side note: In cold-ice models, the rate factor also accounts for additional softening in temperate ice.
Noted.

p. 2577, l.3: Remove "dynamic"
Changed.

p. 2579, l. 14, 22, 25: Change "warmer" to "higher"; "colder" to "lower".
Changed.

p. 2581, l. 10; p. 2582, l. 6 and 24: Same suggestion as p. 2568, l. 2.
Changed.

Review #2

Major Comments:

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

We have expanded the model description section to clarify our use of two separate parameters (rate and enhancement factors) to describe the effective viscosity. The sensitivity to differences in viscosity is briefly described in this section and in more detail in the results of our sensitivity analyses. Please see the response to the major comment from reviewer #1 for the expanded rationale for treating the rate and enhancement factor separately.

2. I strongly believe that the authors can go 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 and 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).

The use of hindcasting to improve the robustness of prognostic models has been added to section 5 and the suggested reference has been incorporated into the text: "Confidence in prognostic models can be improved, however, by comparing the modeled transient behavior with precise measurements of the initial and transient glacier configurations (i.e., hindcasting) (Aschwanden et al., 2013). Using a hindcasting approach, simulations that fail to reproduce the observed temporal evolution of the glacier are rejected, restricting the range of parameters used in prognostic simulations. The benefit of hindcasting is clearly illustrated in Fig. 6: if similar transient results were obtained for a real glacier system with annual front position and speed timeseries, these data could be used to assess the validity of the different model simulations, which would likely improve the accuracy of prognostic simulations."

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.

Reworded: "Although several large-scale ice sheet models include longitudinal stress gradients and continuous grounding line tracking (Favier et al., 2012; Gudmundsson et al., 2012; Cornford et al., 2013), most large-scale models are currently unable to incorporate physically based calving front variations necessary for simulating rapid changes in tidewater glacier dynamics."

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.
Reworded: "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 is absent for outlet glaciers confined by bedrock walls along their lateral margins (i.e., topographically confined), making them susceptible to unstable retreat across reverse bed slopes, as has been observed for numerous outlets in Greenland (e.g. Joughin et al., 2010) and Antarctica (Hulbe et al., 2008; Rignot, 2008)."