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The Cryosphere
Discussions

# *Interactive comment on* "Monte Carlo ice flow modeling projects a new stable configuration for Columbia Glacier, Alaska, by c. 2020" *by* W. Colgan et al.

**Anonymous Referee #2**

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**General comments**

Colgan and colleagues use a 1-D flowline model to investigate the past and future evolution of Columbia Glacier, Alaska. Monte Carlo simulations are used to cover a wide possible range of input parameters and forcings. The model results suggest that Columbia Glacier will reach a new stable state by circa 2020, after a relatively short time of enhanced iceberg calving of about 40 years. This is an interesting result since it has general implications on the validity of extrapolation methods to estimate future glacier mass loss.



My main concern is that the inherent assumptions made in the model might have a strong impact on the results and the main conclusion presented here: Empirical laws are employed to compute basal sliding, surface mass balance and the calving rate, and the results might change notably if physically-based laws were used instead.
One key point is that the authors assume a fixed lower bound for the basal sliding length scale alpha. It remains unclear how this lower bound is deduced from the relation between alpha and the terminus position presented here, and I think the authors should address this issue and discuss the implications of this assumption on their results more clearly. This being said, I believe that within this framework, both climate and parametric uncertainty are adequately addressed by means of the Monte Carlo method.

Since some of the parameterizations are specific to Columbia Glacier, the transferability to other glaciers and the ice-sheets is difficult to assess, and the general conclusions on sea-level rise should be phrased more carefully. Note that this does not question the authors' main conclusion that statistically-based projections of future sea-level rise contributions need to be exercised with more caution, and I believe that this study impressively illustrates the limitations of such an approach.

**Specific comments**

page 897, Eqn. (2): Please explain in the manuscript how F is derived (even though a reference is given further on). How does the shape factor affect Q and thereby the calving rate, and what role does this play for the results?

page 902, Eqn. (8): Since the main conclusion on Columbia Glacier reaching a new stable state by circa 2020 depends crucially on the parameterization of the calving rate, it is important to explore the implications of the choice of the ice thickness at which calving is prescribed: Is the new stable state reached at a different time if the mean terminus cliff height is varied? Have the authors for instance tried changing it to 100m when the terminus has reached the K-GN gap, as suggested in page 908, ll. 1-6?

page 908, ll. 6-11: Could the authors expand a bit on the impact that the loss of tributary W has on the overall mass balance? Does it produce a visible signal in the timeseries?

page 10, ll -9: The Parallel Ice Sheet Model is initially described in Bueler and Brown (JGR, 2009), this citation should be included here.

Table 1: Please cite within the text. It would also be helpful if the values for the constants were provided in the table.

Fig. 3: It is not clear to me why $\alpha$ is constant once the glacier terminus has reached the overdeepening. The relation between $\alpha$ and $x_{\text{term}}$ shown in the inlay rather suggests that $\alpha$ decreases further as the glacier retreats. How does the choice of the lower bound for $\alpha$ impact the new equilibrium state? Would Columbia Glacier retreat further if a different lower bound were applied?

Fig. 9: I would be curious to see the surface elevation for different stages between 1977 and 2100. This might help interpret the timeseries given in Fig. 11.

Fig. 11, A: It seems that the observed equilibrium line altitude is constant while



the modeled $z_{\text{ela}}$ rises throughout the simulations - how do the authors explain this qualitative difference?

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Interactive comment on The Cryosphere Discuss., 6, 893, 2012.