The Cryosphere Discuss., 6, C662–C669, 2012
www.the-cryosphere-discuss.net/6/C662/2012/

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.

W. Colgan et al.

william.colgan@colorado.edu

Received and published: 30 May 2012

Original reviewer comments in italics
Author comments denoted "**AC**"

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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.

AC: We feel that imposing empirical and statistical parameterizations in a non-deterministic fashion (i.e. over a wide parameter space), and reasonably reproducing observed and inferred historical data, is a novel aspect of this paper. We acknowledge that the basal sliding parameterization is far from ideal, but in the manuscript we clearly state that it is "an empirical, and thus site-specific, parameterization." In a revised version of the manuscript we will clarify that the upstream limit of basal sliding evolution at km 50, and hence the lower limit of the alpha scaling length, is derived from an empirical criterion used to assess the terminus stability of tidewater glaciers (Pfeffer, 2007). This stability criterion suggests that tidewater terminus geometry may be regarded as stable when $H/Hw \geq 1.5$ (the ratio of ice thickness to water depth), and unstable (i.e. prone to retreat) when $H/Hw < 1.5$ (Pfeffer, 2007). As described in our response to reviewer 1, we use inferred bedrock elevation and observed 2007 ice surface elevation (McNabb et al., submitted) to calculate the H/Hw profile along the main flowline of Columbia Glacier, and find that the terminus will cross this stability threshold, from unstable to stable states, around km 50 (See

TCD
6, C662–C669, 2012

Figure 1 of this comment). The manuscript presently states that "We prescribe α as a function of terminus position (*xterm*), which allows α to decrease as the terminus retreats upstream." We will clarify that the minimum bound of alpha is derived from extrapolating the observed relation between alpha and terminus position to km 50. We also note that there is not a single minimum α values imposed across all simulations, but rather that α ranges between 5.0 and 5.5 km over the ensemble of simulations. Figure 3 illustrates the variability in basal sliding velocity allowed by this range. We are receptive to performing a more explicit sensitivity study of the lower bound of alpha at the request of the Editor. Such a sensitivity study, for example, may allow alpha to evolve (i.e. decrease) until the terminus reaches km 40, at which point the inferred bedrock elevation rises above sea level. We note, however, any upstream limit for the evolution of basal sliding other than km 50 is not supported by the above empirical stability criterion, and would thus be rather arbitrary.

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.

AC: We intend to remove the majority of the final paragraph of Section 4.2. Our revised Section 4.2 will focus only projecting the future sea level rise contribution of Columbia Glacier. This will remove all speculation that anticipated changes of Columbia Glacier may be transferred to other glaciers. We will maintain the discussion of statistically-based projections of sea level rise.

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?

AC: In a revised version of our manuscript, we will clarify that: "The spatially variable shape factor (F) is prescribed as a function of glacier width, assuming a parabolic cross-channel geometry, following Paterson (1994, p. 269)." We will also clarify that: "As this shape factor appears in the equation describing ice discharge, F directly scales ice discharge." We note that w (glacier width), not F, appears in the equation describing ice discharge (Equation 8).

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?

AC: It is difficult to change the first-type boundary condition (prescribed terminus height) during a transient run. For example, by increasing the prescribed terminus height part way through a simulation, all ice with a surface elevation between the old and new terminus heights will be immediately calved, leading to short-lived, artificial, increase in terminus retreat before coming into equilibrium with the new prescribed terminus height. In a revised manuscript, we will perform an explicit sensitivity study on prescribed terminus height by performing an ensemble of simulations in which terminus height is prescribed as 100 m (instead of 80 m) for the duration of the run. We will then discuss any significant differences between the 80 and 100 m boundary conditions.

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?

AC: It is difficult to quantitatively assess the mass balance of the West Tributary, as we only model the main flowline. When calculating the total anticipated sea level rise contribution from the model domain (Equation 10), we note that: "This estimate excludes all ice covered areas outside the main flowline model domain (i.e. adjacent small glaciers, cirques and tributaries)." In a revised version of our manuscript, however, we will speculate on the dynamic implications of the main flowline terminus retreating upstream of the km 51 confluence with the West Tributary (c. 2005).

page 10, ll -9: The Parallel Ice Sheet Model is initially described in Bueler and Brown (JGR, 2009), this citation should be included here.
AC: We have replaced the Winkelmann et al. (2011) reference with Bueler and Brown (2009).

Table 1: Please cite within the text. It would also be helpful if the values for the constants were provided in the table.
AC: We submitted the notation table as an appendix, and will ensure it appears as such (i.e. not a in-line table) in the revised manuscript. We will include the values of all constants.

Fig. 3: It is not clear to me why α is constant once the glacier terminus has reached the overdeepening. The relation between α and xterm shown in the inlay rather suggests that α decreases further as the glacier retreats. How does the choice of the lower bound for α impact the new equilibrium state? Would Columbia Glacier retreat further if a different lower bound were applied?
AC: We believe we have addressed this comment above. To clarify, however, we note that alpha continues to evolve until the terminus retreats out of the overdeepening (i.e. achieves a geometry in which water depth is small in comparison to ice thickness), not once "the terminus has reached the overdeepening" (where water depth is large in comparison to ice thickness).

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.
AC: We can include ice surface elevation timeseries from discrete positions along the main flowline in a revised version of our manuscript.

Fig. 11, A: It seems that the observed equilibrium line altitude is constant while the modeled zela rises throughout the simulations - how do the authors explain this qualitative difference?
AC: In a revised version of our manuscript, we will note that: "the observed equilibrium line altitudes represent period means, while the modeled equilibrium line altitude is transient."

References

Bueler, E., and J. Brown. 2009. Shallow shelf approximation as a "sliding law" in a thermomechanically coupled ice sheet model. *J. Geophys. Res.* 114 (F03008): doi:10.1029/2008JF001179

McNabb, R., R. Hock, S. O'Neel and L. Rasmussen. *submitted*. Using Surface Velocities to Infer Bed Topography: A Case Study at Columbia Glacier, Alaska. *J. Glaciol.*

Paterson, W. 1994. The Physics of Glaciers. Butterworth-Heinemann. ISBN 0-7506-4742-6.

Pfeffer, T. 2007. A simple mechanism for irreversible tidewater glacier retreat. *J. Geophys. Res.* 112 (F03S25): doi:10.1029/2006JF000590.

Winkelmann, R., M. Martin, M. Haseloff, T. Albercht, E. Bueler, C. Khroulev and

A. Levermann. 2011. The Potsdam Parallel Ice Sheet Model (PISM-PIK) - Part 1: Model description. *The Cryosphere*. 5: 715-726.

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

TCD

6, C662–C669, 2012

Interactive Comment

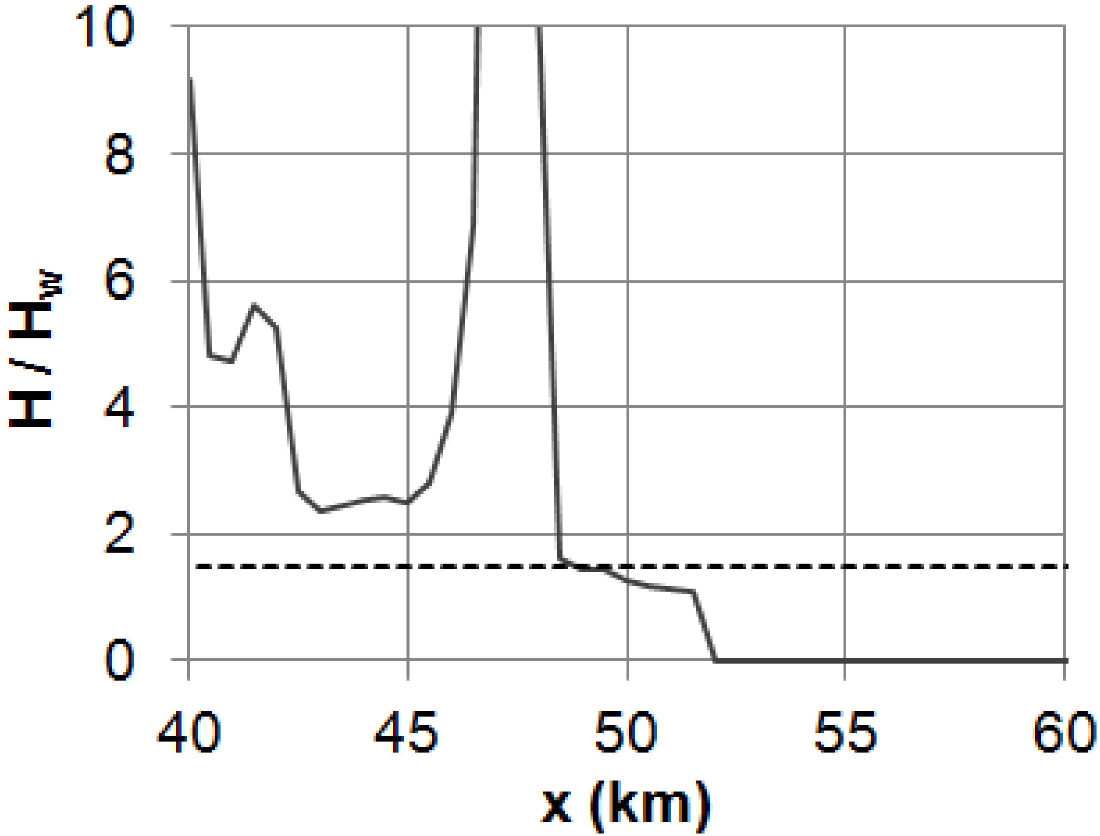

Fig. 1. Ratio between ice thickness (H) and water depth (Hw) along the main flowline of Columbia Glacier.