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

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

This paper applies Monte Carlo simulations to effect an extensive parameter search for the past behavior of Columbia Glacier. It then filters the successful models and runs the successful ones prognostically to predict the remaining retreat of Columbia Glacier. The method is interesting, and a new application to glaciology. The paper should therefore be published in TC. There are a number of points though that

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should be addressed in a revision. I list them in order that they occur:

p.895, l.7-8: This is not quite an accurate description of the discrepancy be-tween Arendt et al and Berthier et al. Arendt et al did NOT use an extrapolation from Columbia to less dynamically active areas. Berthier concludes that the issue is the extrapolation from center line profiles across the width of a glacier.

AC: We will clarify this description by amending the passage to read: "This latter contribution rate, however, is considered an overestimate, due to the extrapolation of glacier centerline altimetry data across the width of the glacier. Dynamic thinning reaches a maximum along a glacier centerline, and reaches a minimum at the lateral margins of a glacier (Berthier et al., 2010)."

p.896, l.6: Calving rate should not be presented as an observed quantity. It is derived.

AC: We will clarify that iceberg calving rate is an inferred quantity by saying: "While not strictly an observed quantity, time-series of iceberg calving rate have also been inferred for Columbia Glacier (Krimmel, 2001; Rasmussen et al., 2011)."

l.23: Chandler et al., JGR, 2006 also used Monte Carlo methods, in their case to derive basal motion.

AC: We will include this Chandler et al. (2006) reference in our introduction of Monte Carlo applications in glaciology.

p.898, eqn(4): You should reference which equation you're using in VanderVeen (1987). The closest I found was his eqn (21), but that has additional terms that you are leaving out. You should explain that.

AC: This is correct we use the Van der Veen (1987) Equation 21 formulation, and assume the "*D*" terms are negligible. We will clarify this by revising the description to: "Depth-averaged longitudinal coupling stress is calculated according to Equation 21 of

Van der Veen (1987). This formulation derives longitudinal coupling stress by solving a cubic equation describing equilibrium forces independently at each node, based on ice geometry and prescribed basal sliding velocity. Following the suggestion by van der Veen (1987), we assume that the longitudinal gradients of the depth-averaged longitudinal deviatoric stress are small, so that the "*D*" term in his Equation 11 is neglected, producing a simpler form of his Equation 21, which is our Equation 4. As noted by Van der Veen (1987), this formulation is similar the Alley and Whillans (1984) approximation for depth-averaged longitudinal coupling stress."

l.12: Paterson does not define F in the same way, so his shape factors are not directly applicable. Also, what assumption are you making about the cross sectional shape, and how does F change as the glacier gets thinner? There are some non-trivial choices. For example, you need to make sure you're not messing up mass continuity.
AC: We contend that our *F* parameterization is functionally equivalent to that of Paterson (1994). We note that Paterson (1994) incorporates a first-order *F* in his equation for driving stress, and driving stress subsequently appears as a first-order term in the equation for ice discharge (i.e. our Equation 2). Thus, following a Paterson-type approach, ice discharge due to internal deformation is modified by a single first-order *F*. We believe this is functionally equivalent to inserting the first-order *F* into an equation describing ice discharge, rather than an equation describing driving stress, as we have done. We note that inserting *F* into the ice discharge equation allows the ice discharge due to basal sliding to also be modified by cross-sectional valley profile. Thus, adhering to a strict Paterson-type approach would fail to account for the influence of cross-sectional profile on ice discharge due to basal sliding, which is a significant source of ice discharge at Columbia Glacier. We will modify our description of the *F* parameter to read: "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). A notable variation on the implementation of Paterson (1994), who incorporates *F* in the calculation of driving stress (Equation 62 in p. 268),

is that we instead incorporate F and w in the calculation of ice discharge (Equation 2). These implementations result in functionally equivalent expressions for ice discharge due to internal deformation, as F is first-order in the equation describing τ (Paterson, 1994, p. 268) and τ is first-order when calculating Q due to internal deformation. Our formulation, however, also allows valley shape to influence Q due to basal sliding, which exceeds Q due to internal deformation throughout much of the Columbia Glacier ablation zone. Imposing F on driving stress, and thus only on Q due to internal deformation, would result in a significant overestimation of Q due to basal sliding, as basal sliding would implicitly be assumed to be acting on a perfectly rectangular cross-sectional valley profile. Incorporating w in the equation describing ice discharge ensure mass continuity." In regards to the transience of F, we note in the discussion section that we assume F is only a function of prescribed glacier width, $F(w(x))$, rather than transient glacier thickness, $F(H(t))$, and acknowledge that this is a limitation of the model.

p.899, l.9: I would like to see a discussion of the influence of the assumption that alpha reaches a minimum at km 50. Does that not automatically lead to a more passive upstream area? It seems to me like it disables activation of upstream ice a-priori. If not, then elaborate on that in the Discussion.

AC: We will revise this description of the imposed α minimum to read: "We assume that α reaches a minimum of 5.25 ± 0.25 km when the terminus position retreats to km 50, the approximate upstream limit of the inferred bedrock over-deepening of the main flowline of Columbia Glacier (McNabb et al., submitted). The assumption that km 50 is a stable terminus position is couched in the notion that a stability criterion, comprised of the ratio between ice thickness (H) and water depth (Hw), can distinguish stable and unstable terminus positions of tidewater glaciers. Ample empirical evidence suggests that tidewater terminus geometry may be regarded as stable when $H/Hw \geq 1.5$, and unstable (i.e. prone to retreat) when $H/Hw < 1.5$ (Pfeffer, 2007). 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 (See Figure 1 of this comment). This analysis suggests that *H/Hw* is below the stability criterion threshold downstream of km 50, where water depth is large compared to ice thickness, but exceeds the stability criterion threshold upstream of km 50, where water depth is small compared to ice thickness. Thus, we assume that the basal sliding profile will cease to evolve once the terminus retreats upstream of km 50." We will include the attached figure of the *H/Hw* profile in the vicinity of km 50. We also note that minimum α ranges between 5.0 and 5.5 km over the ensemble of simulations, and thus some variability in minimum α is ensured.

p.901, l.24-: This needs to be cleaned up a bit. Eqn (1) and (2) form an initial boundary value problem. The surface mass balance and the basal velocities are not boundary conditions, they are part of the PDE you're solving (in that sense they are like source terms). The combined equations give you an equation for H or for hs, depending on how you formulate it. Boundary values then need to be given for that quantity.
AC: We will clarify that surface mass balance is a source/sink term in the model and that the surface (top) boundary condition of the ice flow model is the assumption that $\tau \to 0$ at the free surface of the glacier (i.e. where $H \to 0$). This assumption is invoked during the formulation of the first-order approximation to the Navier-Stokes equations described by Equation 3. We will merely state that basal sliding is prescribed to the ice flow model, rather than terming the basal sliding a boundary condition.

p.902, l.21: There is a missing funny looking H in that sentence
AC: Yes, the funny looking *H* appears to have been lost in translation between manuscript submission and posting online. We will more carefully inspect future proofs.

l.26: Does F really account for divergence? I'm not convinced. But the variation

of w does in a way.
AC: We will clarify that it is the inclusion of both *F* and *w* in the calculation of *Q* that implicitly account for flow divergence and convergence stemming from changes in flowband width, by implicitly contributing $\partial F/\partial x$ and $\partial w/\partial x$ terms to $\partial Q/\partial x$.

p.906, l.14-: How dependent is this result on the parameterization of sliding and the forced limit on alpha (see earlier comment)?
AC: We believe we have addressed this in the response to the earlier comment.

p.906, l.26: Presumably you mean the sign of the velocity change, and not the sign of the velocity itself?
AC: Yes, we will clarify that it is the "magnitude and sign of velocity changes" that we are alluding to.

p.907: This page contains several "reasonable" and "satisfactory" and qualitative statements of that nature. It is generally a good idea to quantify and then discuss discrepancies and leave the reasonableness to the reader.
AC: We will revise our language to reduce the number of non-neutral adjectives throughout the comparison of the model results with observed or inferred data.

p.909, l.24: 'begin' -> 'began'
AC: We will make this correction.

l.26-: I suggest using 'glacier density' instead of 'ice density' in this context. The density of ice itself is not variable, it is the bulk density of the glacier. Also, it should be stated that continuum mechanics can very well deal with variable density (you just add its rate of change to the mass continuity equation). The problem is that you have to find an appropriate equation of state. So the problem is not continuum mechanics, it is a lack of understanding on how to incorporate the process of crevassing.

AC: We will amend our terminology to "glacier density" throughout the manuscript. We will also clarify that the difficulty in incorporating crevassing into an ice flow model lies not with continuum mechanics, but with the absence of an appropriate equation of state, or even statistical parameterization, that would allow rate of change of glacier density to be incorporated into the mass continuity equation.

p.910, l.16: I would also add 'lack of data' here. Columbia Glacier is really quite exceptional in that regard.
AC: We will include a lack of data as one of the obstacles facing the widespread assessment of the sea level rise due to dynamic contributions.

p.911, l.12: The 2007 ice thickness is largely inferred from a model, not observed.
AC: We will amend our terminology to reflect that the McNabb et al., (submitted) ice thickness dataset is primarily "inferred" rather than "observed" throughout the manuscript.

p.912, last part of Discussion: I find this section rather weak. It is a good idea to discuss the implications for regional sea level rise assessments. But one has to be a bit more cautious. For example, the concept of Johanneson's time scale has been expanded on by Will Harrison in some work. He has integrated the idea that certain ice fields could have negative time scales, i.e. be unstable. This is likely the case for some ice fields in Alaska, where an equilibrium state in 2100 does probably not exist. This comes up again at the end of the paper where you first outline Columbia as special ("biggest contributor to sea level rise in AK") and then suggest to treat other tidewater glaciers in similar fashion.
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 representative of Alaskan Glaciers, and thereby remove the need to invoke response timescale.

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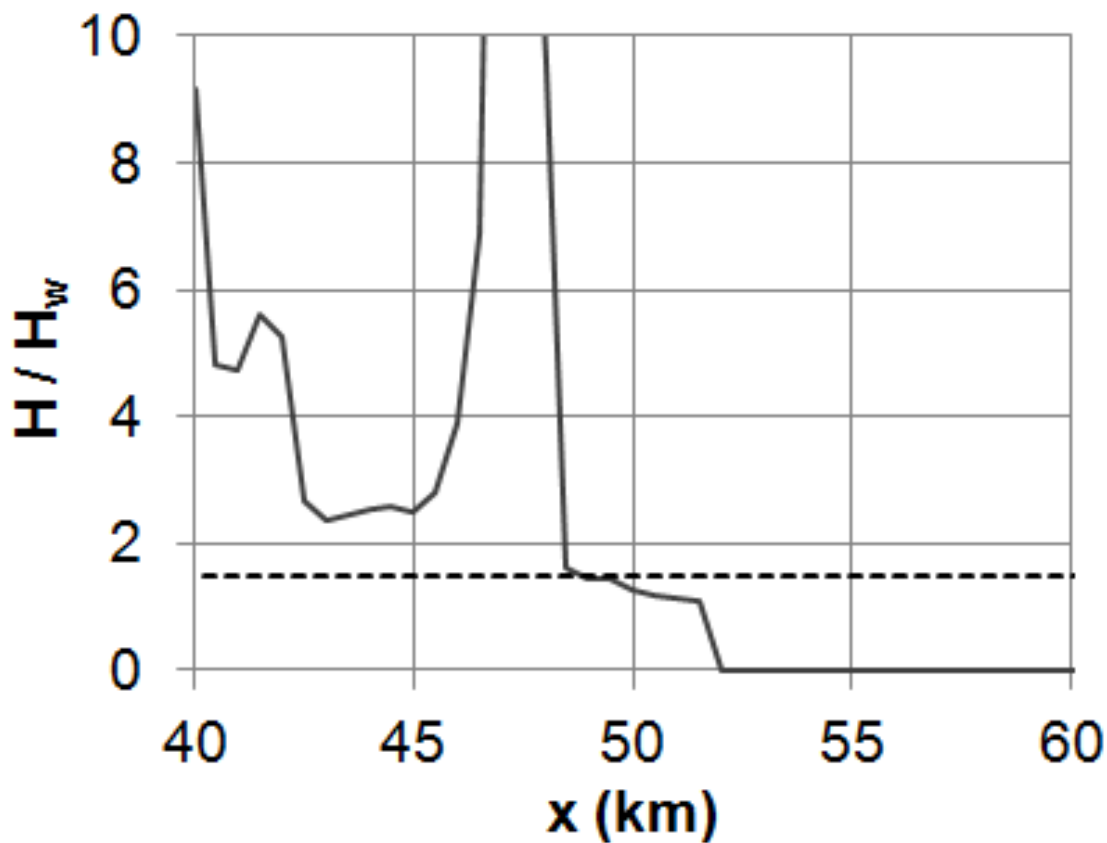
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Fig. 1. Ratio between ice thickness (H) and water depth (Hw) along the main flowline of Columbia Glacier.

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