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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.

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Review

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 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 discrepency between

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 extrapolatation from center line profiles across the width of a glacier.

p.896, l.6: Calving rate should not be presented as an observed quantity. It is derived. l.23: Chandler et al., JGR, 2006 also used Monte Carlo methods, in their case to derive basal motion.

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. 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.

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.

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 h_s, depending on how you formulate it. Boundary values then need to be given for that quantity.

p.902, l.21: There is a missing funny looking H in that sentence l.26: Does F really account for divergence? I'm not convinced. But the variation of w does in a way.

p.906, l.14-: How dependent is this result on the parametrization of sliding and the forced limit on alpha (see earlier comment)?

p.906, l.26: Presumably you mean the sign of the velocity change, and not the sign of the velocity itself?

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 discrepencies and leave the reasonableness to the reader.

p.909, l.24: 'begin' -> 'began' 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.

p.910, l.16: I would also add 'lack of data' here. Columbia Glacier is really quite exceptional in that regard.

p.911, l.12: The 2007 ice thickness is largely inferred from a model, not observed.

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.

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