Reviewer response to 'Impact of frontal ablation on the ice thickness estimation of marine-terminating glaciers in Alaska'

July 31, 2019

In 'Impact of frontal ablation on the ice thickness estimation of marine-terminating glaciers in Alaska,' the authors extend the thickness estimate technique of Farinotti by allowing for a non-zero terminus flux, which is to say that the modeled glacier may lose mass not only by surface mass balance processes, but also by calving and terminal melt. The authors show that failing to include this mechanism of mass loss leads to an underestimation of total glacier volume. They explore its sensitivity to a variety of parameter choices. They then apply the method to Columbia Glacier, and then the RGI for coastal AK.

Overall, the paper is improved from its previous versions. I suggest the following technical corrections

- Returning once again to the discussion of a missed factor in the handling of width-averaged fluxes, the paper needs to be specific in how the omission could affect the results, e.g. "neglecting variations of u across the glacier width may lead to an overestimation of fluxes by a factor of xx, however we proceed with this approximation because it allows for easier generalization to alternative bed shapes." The authors have already written something similar in their response, but it does not enter the manuscript.

- I think that Eq. 7 still has a units error. $\int_\Omega \rho \frac{\partial h}{\partial t} \mathrm{d}A$ has units of mass per time, yet this is equated to $q_{calving}$ which has stated units of length per time. Which is it, and is there an equivalent mistake in the code?

- Regarding the definition of depth, I didn't mean to say that you have to include lake-terminating glaciers in the analysis, simply that it is better to include the complete derivation of a method, then simplify it later, which is to say that you can write

$$d = H_f - E_t + z_w,$$

  and then say that you are setting z_w to zero for the rest of the paper. Would it not be valid to use this method if I started measuring water surface from some other arbitrary datum than mean sea level? I think that it would, and by using the above definition, you retain that capacity, even if you don't use it.

- I wrote in my previous review: *In the Columbia Glacier case study, would it be possible to report the terminal velocities that OGGM predicts, as a way to see whether these are remotely consistent with observations? Columbia is, of course, very fast, and if the velocities are too low, this could lead to an overestimation of near-terminus thickness.* I didn't write this comment out of pure curiosity, but rather because I think it needs to be demonstrated that this model produces sane results for these tidewater glaciers. Thus, I think that the authors' response to this point should be included in the paper somewhere.

- In Section 4.2, paragraph 2, the authors state that smaller glaciers change relatively more than large glaciers for a specified frontal ablation. I think that the different scale of the glaciers overwhelm potential differences in dynamics, making these results less interesting than they could be. To me, it's rather obvious that specifying a given and fixed calving flux would have a larger relative effect on a smaller glacier, because it's a relatively larger portion of the mass budget. A more interesting experiment might be to specify a range of frontal ablation fluxes as fractions of the total accumulation, e.g. $q_{calving} = f \int_{\Omega} p_f P^{solid} \mathrm{d}A$ for $f \in [0, 0.5]$. Thus the frontal ablation would naturally scale with the glacier size and we could see the result of the differences in physics between small and large glaciers.