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

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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 much improved over its first version, however I remain skeptical on a few points, some new, some remaining from the paper's first submission.

- Regarding my discussion of a missed factor in the handling of width-averaged fluxes.

  This reply isn't entirely satisfactory. At the end of the day, the authors' stated equations are incorrect, and they should be corrected before publication: u is stated in Eq. 2 as the average cross-sectional velocity, while it is clearly not, *so long as the authors allow for their modelled cross-sections to be non-rectangular.* The authors should either acknowledge that their equations are an approximation that could be off by a substantial factor, or they should implement the analytical solution to u as a function of cross-section shape. The latter would be preferable, but the former is also reasonable. However, simply writing an incorrect equation and then making an appeal to antiquity ("A systematic error a very large magnitude is unlikely to have been left unnoticed") isn't defensible.

  The argument that by specifying the flux via integration of the upstream effective mass balance, this makes the errors in the resulting thickness go away is also not correct. Here, we have that $q = uS$, where u is the depth and width average velocity given by

$$u = \int_W \bar{u}(h(y)) \, \mathrm{d}y = k\bar{u}(h_0),$$

where (in the absence of lateral drag) $k \leq 1$, with $k = 1$ for a rectangular cross section, and $k = \frac{128}{315}$ for a parabolic one. This leads to the equation

$$q = p(kf)wh_0^{n+2},$$

where $p = \frac{2A}{n+2}(\rho g \alpha)^n$, f is the ratio of the cross-sectional area to a rectangular one (i.e. 1 for rectangular, 2/3 for parabolic), and w is the width. Solving for h_0 gives us

$$h_0 = (kf)^{-\frac{1}{5}} \frac{q}{pw}^{\frac{1}{5}}.$$

For the rectangular case, of course the geometric prefactor is unity, but for the parabolic case, it's around 1.3. *Since this factor is so straightforward to calculate, why not just include it?* It requires no separate solver, just the imposition of the correct geometric pre-factor when estimating h_0. If the bed shape isn't known (which it usually isn't), then the fact that the pre-factor is unknown should be included in error estimates.

- Regarding the definition of depth.

  I think it would be better to develop the depth definition such that it could be applied to situations in which the water elevation isn't zero, e.g. for lakes. Thus, we would have that

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

  where z_w is the water surface. After such a definition, if there is in fact no way to measure the surface elevation of water bodies that are not the ocean, then one can explicitly set z_w to zero.

- Iterative calibration procedure. I think that the five steps outlined in this section are a bit confusing because some of them are done several times. Would it be possible to put this in pseudocode, as is typically done in math/CS papers? The LaTeX algorithm environment is designed for just such a task.

  Also, after a fair bit of fiddling with equations, I managed to convince myself that this problem does have a unique solution, and that this solution induces a unique value of μ^*. Indeed, the explanation as it appears in section 3.5 is, I think, correct. However, it's a little bit difficult to understand, and should also be placed in Sec. 3.4. I would explain it as such: there are two equations in this model that can be used to estimate the ice flux given the thickness: Glen's flow + sliding law (quintic in thickness), as can the calving law (quadratic in thickness) under the assumption that the amount of ice calved is equal to the amount delivered to the terminus. These fluxes must be equal, which leads to an equation which is quartic in thickness, the unique real-valued solution of which yields the thickness value consistent with both Glen's and calving laws. Inverting either of these laws to get an equation for terminus flux and applying the computed thickness value leads to the one and only terminal flux that is consistent with both thickness estimation methods. The authors impose this specific flux in OGGM by adjusting the degree day factor μ^*. Interestingly, being quartic, I think that the optimal thickness has an analytical solution: instead of computing it directly, the authors are using a fixed-point iteration to find the roots (which I don't object to at all).

In any case, a clearer description of what exactly this calibration procedure is doing mathematically would help the skeptics amongst us. Also worth noting: instead of adjusting the degree day factor, one could adjust other parameters (i.e. sliding law, calving parameter) such that this relationship is satisfied as well. A little bit better justification of why μ^* is the one to fiddle with is in order.

- "This can be due to two equally likely factors: solid precipitation is underestimated, or the frontal height and therefore frontal ablation is overestimated". Alternatively, k is overestimated or the calving law is wrong.

- 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.

- In Section 4.3, the authors compare the velocities produced by the model to the case where frontal ablation is not considered, which is useful. More useful, I think, would be comparing these velocities to measured ones (e.g. from GoLive) in order to get a sense of whether the physics being used by OGGM are consistent with reality.

- In Section 4.4, the authors perform a sensitivity study over various physical parameters. This is an important step, but I think that the range of possible basal tractions is far too small. Velocities often vary between tidewater termini by two orders of magnitude. I would like to see the basal traction parameter vary over similar ranges, primarily because this parameter aliases differences in effective pressure: alternatively the sliding law used in this paper could include effective pressure explicitly, computed using height above overburden as an upper bound.

- The Discussion isn't really a discussion, it's another methods/results section in which the authors include additional observations in their inversion. I would suggest relabeling it as such.