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# *Interactive comment on* "Impact of frontal ablation on the ice thickness estimation of marine-terminating glaciers in Alaska" *by* Beatriz Recinos et al.

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We would like to thank Douglas Brinkerhoff for taking the time to read our manuscript and give us a chance to answer to his concerns before the end of the review process. We hope that our response is clarifying the motivation behind our study, and we remain available for further questions.

Here we present a detailed point by point response (the reviewer's comments are given in italics, our answer in normal font).

*RC:* ***Eq. 2****. This equation is not valid for non-rectangular cross-sections. Rather, it is*



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*depth- averaged velocity for a particular location over a cross-section. To make this into depth and width averaged velocity, we need to introduce a parameterization of h (parabolic, for example), and then width integrate. If we do this (assuming a centerline depth of $h_0$ and margin thickness of zero, we get an additional multiplicative factor of $\frac{128}{315}$ (assuming $n = 3$). Thus, fluxes are being overestimated by a factor of nearly 3.*

AR: yes, we should have been more precise in the formulation here. In OGGM we support rectangular, parabolic and trapezoidal bed shapes. For all cases, we compute the ice velocity at the maximum thickness $h_0$, then multiply this velocity by the cross-section area to compute the flux. The section area is $S = \frac{2}{3}h_0 w$ in the parabolic and $S = h_0 w$ in the rectangular case (with $w$ the section width). This is physically not correct (we are missing a non-linear term in the variation of $\tau$ with the parabola), and is indeed an overestimation of the flux with respect to a true parabolic bed.

Nye (1957) gives analytical solutions as to how much this overestimation could be (his Tables IIIa and IIIb). He gives solutions for the section average velocity and the surface velocity at the parabola's center, i.e. providing an upper bound of the error in our approach: this overestimation ranges from a factor 1.48 to 2.25 depending on the parabola's width to depth ratio.

That being said, we have to emphasize here that OGGM is of the "paremeterized" ice model type, aiming at the simulation of a very large number of glaciers with unknown boundary conditions. We could implement the correct solution (and will happily add an option to use it, as we've recently done by implementing lateral drag shape factors following Adhikari and Marshall, 2012, follow **this link** for the implementation).

However, the current implementation has several practical advantages: it allows us to use the same numerical solver for all bed-shapes (Maussion et al., 2018), is computationally efficient and consistent between the inversion and the forward model. This alone should not be used as an argument to justify our method. More importantly, this simple approach reduces the difference in fluxes between different bed shapes, which are unknown a priori (and are neither parabolic nor rectangular in reality). Hence, the sensitivity of the model to this unknown parameter is reduced as well.

In the case of the ice thickness inversion, the flux is prescribed by the apparent mass-balance anyway, so that the implied uncertainties are transferred to the cross-section's thickness and volume (and each section is independent from another). The same equations are used to solve for $h_0$ with a given flux. This results in a polynomial of degree 5 with a unique solution for $h_0$ in $\mathbb{R}_+$. Therefore, for any given glacier with unknown bed-shape and prescribed apparent mass-balance, we will have a volume ratio of approx. $\frac{2}{3}$ between the two cases[1]. In practice, OGGM relies on geometrical considerations to decide if the shape is parabolic or rectangular (Maussion et al., 2018).

Finally, it must be added that the inversion model used in this study is the same as the one used in Farinotti et al. (2017) (where OGGM ranked amongst the best models able to process a large number of glaciers), Maussion et al. (2018), and Farinotti et al. (accepted). While this doesn't mean that the model is error-free (there is no such thing anyway), a systematic error of a very large magnitude is unlikely to have been left unnoticed.

The interested reader can find the corresponding code implementation at the following locations (links):

- **ice thickness inversion**
- **forward model**
- **tests of the forward model** where we compare our implementation to the solver by Jarosch et al., 2013.
- **tests of the inversion model in ideal cases simulated by the forward model**

[1] $\frac{2}{3} \times \frac{3}{2}^{\frac{1}{5}} = 0.72298$ to be exact, in the case $n = 3$ and without sliding





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*RC: **Eq. 6**. The interpretation of these symbols doesn't make sense. $\Omega$, in this case needs to be the contributing area for a given cross section, not the cross-section itself. This correct form leads to units of $kgs^{-1}$. However, the definition of $F_{calving}$ is in units of volume per time, and thus we have a misfit. This would be (numerically) fine if this parameter were solved for because this error could be absorbed into $k$. However, the authors set this to a value previously computed by Oerlemans and Nick, and thus the scaling of the terminal versus surface fluxes is incorrect.*

**AR:** you are fully right, $\Omega$ is of course the contributing area (we call it "catchment area" in the model code), this was a bad typo. We convert between volume and mass using an ice density of 900 kg m$^{-3}$. The conversion is implemented **here**.

*RC: **Eq. 10**. This expression for depth makes no sense to me, partially because the terms included are not well defined. What is the 'elevation of the glacier terminus', Et? We're dealing with vertical ice cliffs here, so is this the base of the cliff (i.e. bedrock elevation) or the top? In either case, the resulting d is not consistent with the definition of depth used in Oerlemans and Nick frontal ablation parameterization. Also, I fail to understand the difficulty implied about lake terminating glaciers. The definitions are fairly simple: Hf needs to be the terminus ice thickness, d needs to be the water depth. Neither depend on sea level being zero. (This is not to say that there is no difference between marine and lake-terminating glaciers; k should be different between them).*

**AR:** the water depth $d$ is estimated from free-board, using the terminus elevation obtained by projecting the RGI outline to the DEM. I.e., the terminus elevation is the top of the cliff. The Terminus elevation ($E_t$) minus the thickness of the ice ($H_f$) is the water depth (negative values) at that point, or as Oerlemans and Nick (2005) definition of $d$ (bed elevation with respect to sea level, See Figure 1 from Oerlemans and Nick, 2005). We are not able to estimate a water depth for lake-terminating glaciers, because for that we would need to know the free-board of the glacier terminus, i.e. the elevation of the glacier lake surface (for the elevation of the ocean surface, we assume that it is 0 m a.s.l.).

We leave the discussion about $k$ for the next section.

*RC:* ***Sect 3.4*** *It is not clear what this iterative procedure accomplishes, especially if $\mu^*$ is being altered, as is indicated. It seems that for a fixed surface mass balance and terminus position, there are any number of valid solutions that respect the constraints that $H_f \geq 0H$. Is it trying to match a specific $F_{calving}$ based on observations? In that case, I can see the utility in changing $\mu^*$.*

**AR:** Thanks for this comment - we will have to better explain our intent in the manuscript, as this is also a point that Reviewer #1 was asking to improve. We will make further experiments and graphics to explain the procedure, but to expedite the review process, we attempt an explanation here:

First of all, all inversion methods based on mass-conservation assume a mass-flux of zero at the terminus, unless constrained either by a prescribed calving value (e.g. Huss et al., 2015) or by observed ice velocities (e.g. Fürst et al., 2017). In OGGM, we use the equilibrium assumption to calibrate a first guess $\mu$, which assumes an ice flux of zero at the terminus as well. Therefore, if $k$ and water depth (or the calving flux) would be known, we could derive $\mu$ from them.

In the absence of observations, we go the other way around. The main objective of the iteration is to find a frontal ablation flux and ice thickness compatible both with the frontal ablation parameterization and with mass conservation, in order to compute the first ice thickness inversion and initialize the glacier in the model (black line of Figure 3c). This allows calving glaciers in OGGM to have a terminus thickness bigger than zero and a non-zero ice flux at the end of the glacier (Figure 3c and Figure 5), which is then fed again to the mass-balance model to re-calibrate $\mu$.

Importantly, the experiments described in Sect. 4.1, 4.2 and 4.3 use OGGM's default configuration set up (described in Sect 3.1 and 3.3). During these experiments, we do not make use of any observations[2] to restrict or calibrate $F_{calving}$. One of the most im-

[2]In the second part of the experiments (Sect. 4.4 - 5) is when we make use of previously calculated frontal

portant result of our study (the ratio of underestimated ice volume because of calving) is robust to the parameter choice and remains approximately constant regardless of the true total ice volume.

Physically, $F_{calving}$ in these experiments (and during the iteration) is limited by the amount of annual accumulation. $\mu^*$ changes at each step during the iteration in order to reconciliate the (now non-zero) frontal ablation flux with mass conservation, while the boundary conditions (most importantly, accumulation) remain fixed. To illustrate how $\mu^*$ varies, we refer you to Fig. 4b and Eq. 5, as well as further experiments that will be realized as a result of the questions raised by Reviewer #1.

*RC: But it seems to me that altering $k$ would be more reasonable, since frontal ablation parameterizations are far more uncertain that surface mass balance parameterizations.*

**AR:** Indeed it will be easier to modify the value of $k$ to match previously calculated regional estimates, or individual calving flux observations. But the ultimate goal of OGGM and our iteration method is to compute a frontal ablation flux for any calving glacier of the world (we are not there yet). Many areas do not have regional estimates or with enough individual calving flux observations to constraint $k$.

We will add an extra section and figure to the revised manuscript in order to illustrate the importance of this iterative procedure when we lack frontal ablation observations or additional data (e.g., depth and terminus width) to restrict our boundary conditions. We will also add some idealized experiments to help the readers to understand this section.

*RC: The above issues are problematic individually, but taken together, they call into serious question the validity of the results. I forego further comment until such a time as they are addressed.*

Thank you for your comments. We hope that we were able to restore your confidence

ablation fluxes from McNabb et al, 2015.

in our results and we will do our best to clarify those points that caused confusion in our manuscript.

**References:**

Adhikari, S. and Marshall, S., 2012, Parameterization of lateral drag in flowline models of glacier dynamics, J. Glaciol., 58, 212, 1119–1132,
doi: 10.3189/2012JoG12J018

Farinotti, D., Brinkerhoff, D., Clarke, G. K. C., Fürst, J. J., Frey, H., Gantayat, P., . . . Andreassen, L. M. (2017). How accurate are estimates of glacier ice thickness? Results from ITMIX, the Ice Thickness Models Intercomparison eXperiment. The Cryosphere, https://doi.org/10.5194/tc-2016-250

Farinotti, F., Huss, M., Fürst, J.J., Landmann, J., Machguth, H., Maussion, F., and Pandit, A: A consensus estimate for the ice thickness distribution of all glaciers on Earth, accepted for publication in Nature Geoscience

Fürst, J. J., Gillet-Chaulet, F., Benham, T. J., Dowdeswell, J. A., Grabiec, M., Navarro, F., . . . Braun, M. (2017). Application of a two-step approach for mapping ice thickness to various glacier types on Svalbard. The Cryosphere, 11(5), 2003–2032, https://doi.org/10.5194/tc-11-2003-2017

Huss, M., and Hock, R. (2015). A new model for global glacier change and sea-level rise. Frontiers in Earth Science, 3(September), 1–22,
https://doi.org/10.3389/feart.2015.00054

Nye, J. F. (1965). The Flow of a Glacier in a Channel of Rectangular, Elliptic or Parabolic Cross-Section. Journal of Glaciology, 5(41), 661–690.
https://doi.org/10.3189/S0022143000018670

McNabb, R. W., Hock, R., and Huss, M.: Variations in Alaska tidewater glacier frontal ablation, 1985-2013, Journal of Geophysical Research: Earth Surface, 120, 120–136, https://doi.org/10.1002/2014JF003276, 2015.

Maussion, F., Butenko, A., Eis, J., Fourteau, K., Jarosch, A. H., Landmann, J., Oesterle, F., Recinos, B., Rothenpieler, T., Vlug, A., Wild, C. T., and Marzeion, B.: The Open Global Glacier Model (OGGM) v1.0, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-9, in review, 2018.

Oerlemans, J. and Nick, F.: A minimal model of a tidewater glacier, Annals of Glaciology, 42, 1–6, https://doi.org/10.3189/172756405781813023, 2005.

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