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Discussions

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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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 claim show that failing to include this mechanism of mass loss leads to an underestimation of total glacier volume.

Unfortunately, I think that the paper exhibits methodological inconsistencies which preclude me from making a judgement regarding the veracity of their results. It could be

that I have simply misunderstood the authors' work and intent. In this case, I would require a more thorough description of the methods, along with specific justification for their use, in order to be able to proceed to reviewing the results. An accounting of these issues is as follows:

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

Eq. 6 The interpretation of these symbols doesn't make sense. Ω, 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 kg s^{-1}. However, the definition of $F_{calving}$ is in units of volume per time, and thus we have a misfit. This would be (numericall) 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.

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', E_t? 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: H_f 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).

Sec 3.4 It is not clear what this iterative procedure accomplishes, especially if μ^* 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 $H_f \geq 0$. Is it trying to match a specific $F_{calving}$ based on observations? In that case, I can see the utility in changing μ^*. 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.

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.

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