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Interactive comment on "Large spatial variations in the frontal mass budget of a Greenland tidewater glacier" *by* Till J. W. Wagner et al.

Anonymous Referee #1

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Review of: Large spatial variations in the frontal mass budget of a Greenland tidewater glacier Authors: Wagner et al.

This study uses field data, satellite observations, and numerical modeling results to investigate spatial variations in iceberg calving and submarine melting. This knowledge is useful because it provides information on how submarine melting might affect calving and terminus retreat. I think this is an interesting way of thinking about the ice-ocean interface.

The paper essentially boils down to an analysis of a mass continuity equation (eq. 1 in the manuscript), in which the rate of terminus retreat is related to the glacier velocity, calving rate, and submarine melt rate.

The left-hand side of equation 1 contains the ice thickness, the rate of retreat, and the ice velocity. These quantities were determined using fairly traditional methods, and as such I feel confident in the results. I have much more trouble with the right-hand side of equation 1, which contains the submarine melt rate and calving rate. Specifically:

1. The submarine melt rate was determined by some combination of hydrographic observations and numerical modeling. How this was done is not clear, as the modeling is apparently presented in a separate paper that is in review. Without access to that paper and very little description of the model, I am basically asked to take the model results at face value. Even if the modeling paper was already published I would still appreciate to have more details of the model in this paper.

2. The calving rate was estimated by (i) locating calving events with measurements of ocean waves and (ii) converting measurements of calving events into calving rates by somehow scaling the number of events (and amplitude of the waves) so as to roughly balance equation 1. Given the wide variety of types of calving events and iceberg geometries and poor understanding of wave generation by calving events, it seems dangerous to assume any relation between wave amplitude and calving event size.

Using this method, the authors are unable to close the mass budget (see Fig. 6c) along the southern part of the terminus, which they attribute to the style of calving that they observe there. That's fine, I suppose, but elsewhere along the terminus the left- and right-hand sides of equation 1 differ by a factor of 2 or more, which raises questions about the validity of their results — and in particular the modeled melt rates and the estimated calving rates.

Its not clear to me why the calving rate wasn't just calculated by subtracting the melt rate from the left-hand side of equation 1, which would at least ensure that mass is being conserved. Nonetheless, a major conclusion in the paper is that frontal ablation is dominated by melting in a couple of specific locations but is otherwise dominated by calving. That is an interesting result, but I think that point can be made much more simply, without the need to convert wave amplitudes to calving event sizes, and may already be apparent in the modeling paper that is in review. For example, in figure 6b it is apparent that the modeled melt fluxes are highly focused on certain regions of the terminus, which already implies that the calving fluxes must be comparatively large everywhere else.

I include some detailed comments below, focusing on areas where I found the descriptions to be somewhat ambiguous. In addition, all errors are discussed in the text, I think it would be helpful to include error bars or confidence intervals in the figures (especially figure 6).

Specific comments: p. 1, l. 14: Tidewater glaciers aren't boundaries per se but rather a part of the ice sheet. Also, tidewater glacier termini (and the bottom of the remaining ice shelves) are really the only boundary between the Greenland Ice Sheet and the ocean.

p. 2, l. 1-2: Its a little confusing how these sentences jump from ablation (in general) to specifically talking about frontal ablation.

p. 2, l. 15: You could also mention that ocean surface gravity waves (both short- and long-period) have been used to observe calving events in Greenland in previous studies.

p. 2, l. 26: It wasn't clear to me how the hydrographic data was used to infer melt / constrain the submarine melt model.

p. 4, l. 1: In what way is the plume amplified? The drainage event is relatively short-lived, so I suspect it wouldn't have an impact on the plume over time-scales longer than about a week.

p. 4, l. 28: Did you correct the DEM to account for the difference between the ellipsoid and the geoid?

Figure 2, caption: It doesn't really make sense to talk about the flux being into the page

here. Do you mean that the velocity profile is from the perspective of somebody looking down glacier?

p. 5, l. 11: Speculation shouldn't be part of the methods section.

Figure 3b (and elsewhere): I'm not sure if its correct to refer to this quantity as a flux. Should it be a flux per unit width?

p. 7, l. 7: You can be more specific here. For an infinite slap with no sliding and uniform temperature, the depth-averaged velocity is 80% of the surface velocity. The percentage goes up for rapidly sliding glaciers and those that have concentrated deformation at depth, such as tidewater glaciers.

Figure 4c: For clarity, considering specifying that negative retreat rates indicate advance. (My personal preference is to plot dL/dt, so that positive indicates advance and negative retreat, but this is fine too.)

p. 8, l. 9: Don't you mean across-glacier variability?

p. 9, l. 1-2: Do you actually calculate the retreat rate perpendicular to the initial terminus, or is it perpendicular to a straight line fit through the initial terminus?

p. 9, l. 8-14 (and elsewhere in the paper): "rates" and "fluxes" are conflated in several places, which may be confusing. For example, in equation (1), H*R is not the retreat rate.

p. 9, l. 25: derived calving record by. . . ? Should "by" be deleted? Or is something missing here?

p. 10, l. 16: How do you do this scaling? Through a minimization procedure?

p. 10, l. 13-15: Laboratory experiments by Burton et al. (2012) suggest that the wave amplitude of waves produced by capsizing icebergs depends on the energy released during the capsize event, which scales nonlinearly with iceberg geometry.

p. 10, l. 24: I assume that the modeled melt rates are constrained by hydrographic observations. Is that correct?

p. 12, l. 21: Doesn't the thickness have to go to 0 at the margins?

p. 12, l. 29-30: Seems self-evident to me.

p. 14, l. 4-5: Can you quantify the amount of ice in the fjord and how it varies seasonally (even in a rough sense by looking at satellite imagery)? This glacier is pretty slow, so the calving fluxes would have to be pretty small.

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