

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 #2

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Overall I think this is an interesting paper. It's a tricky topic, but an important one, and so it is nice to see people try to improve our knowledge. However, I do have a couple of major concerns about the fundamental approach. The authors are essentially trying to determine the balance between ice losses from calving and those from melting. However, it seems to be that both of these things are poorly constrained:

The melt rates are calculated using a model and observations, but these are from Slater et al., which is currently 'submitted'. As such, it is not accessible to me and I also think it is inappropriate to treat this as accepted, when it hasn't been peer-reviewed. I'm not saying this is the case, but if Slater et al were to be rejected or the technique deemed in appropriate, it would mean that this paper also had the same issues.

The other term is calving, for which the authors have frequency (which is a useful
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dataset) but no volume / area data. They therefore take it as the residual of the left hand side of the equation, minus melting. As noted above, the melt calculations may have quite high errors and the method is actually not published. As such, I think this limits the confidence we can have in the results.

The approach of using waves to ID calving events has been demonstrated in Minowa et al 2018 but they also had time-lapse imagery, which meant they could determine the type of calving (topple etc) and they did not need size. Here, I think the size is needed, so that the calving term is more robust.

A more minor point is the paper structure: the methods and results are mixed together. I'm not totally against this and I can see why the authors have taken the approach here (as you need to get the results from one part to do the next), but I think it does reduce the accessibility for someone trying to repeat the experiments, who just wants to get at the methods. As noted above, I think it needs to at least briefly describe the methods in Slater et al submitted here (to avoid the reader having to go and find another publication). Slater et al should also be through the peer review process before it is cited, as it's central to the argument.

Finally, I think the abstract is a bit misleading over time frames. You have the calving data for July, but the earlier material implies that this is a year-round budget.

I've attached some minor comments in the annotated pdf.

Please also note the supplement to this comment:
<https://www.the-cryosphere-discuss.net/tc-2018-143/tc-2018-143-RC2-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-143>, 2018.