

## ***Interactive comment on “Submarine melt rates and mass balance for Greenland’s remaining ice tongues” by Nat Wilson et al.***

**Nat Wilson et al.**

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Received and published: 20 September 2017

We thank the two anonymous reviewers for their comments which make a number of important points and provide to several helpful suggestions. We have responded to these below, and we think the proposed changes improve the manuscript under consideration.

### **Title choice**

Reviewer 1 points out that using the term “remaining” without a specific date and without consideration of a number of smaller but still existing ice tongues is not entirely correct. We propose the more-specific title “*Satellite-derived submarine melt rates and mass balance (2011–2015) for Greenland’s largest remaining ice tongues.*”

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### **WorldView data description**

Both reviewers point out that information regarding the data used is too lacking.

In all, we use a total of 108 DEMs for Nioghalvfjærdsbræ, 97 DEMs for Petermann Glacier, and 36 DEMs for Ryder Glacier. A table has been added to the manuscript including this information (*Methodology*), and a list indicating the scene IDs for all DEMs used can be made available as supplementary information. The acquisition dates of these DEMs is scattered over the period 2011–2015, however there is a seasonal bias as optical imagery is acquired during the melt season. The spatial distribution of the DEMs is not uniform, as shown in the attached maps below (Figure 1).

From these DEMs, we construct 915 DEM pairs for Nioghalvfjærdsbræ, 751 for Petermann Glacier, and 211 for Ryder Glacier. The temporal distribution of these spans is summarized in the Figure 2.

We propose adding these figures and a description to a supplementary materials section.

### **Mass balance clarifications**

As brought up by reviewer 1,

“Another point that would need more details is the distinction between surface and submarine melt rates. It would help the readers if the authors add few lines explaining how they separate the two, most probably using RACMO2.3. Later in the text (line 6, page 6), the authors estimate the annual mean surface melt rates using RACMO2.3, providing one value per ice shelf. Did the authors compute a single mean value for each ice shelves or did they subtract the mean 2011–2015 map of surface melt (which is not spatially uniform) from the total melt? It would also be interesting to know if the authors used an average surface melt over the period 2011–2015, another period, or directly subtract for each pair of worldview images using the exact surface melt between the two acquisition dates. I agree that this correction might be small compared to submarine

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melt, but it would definitively give stronger results if this is done and explained properly.”

We have expanded the manuscript with the following information:

The surface component of the mass balance is extracted from the RACMO2.3 (Noël et al., 2015) model product. We use a single average melt rate computed over the period 2011–2015 for each ice tongue, which is appropriate for mean melt rates computed over multiple years.

### **Ice and seawater densities**

Both reviewers request more information about the densities assumed. To the *Methodology* section of the manuscript we have now included and assumed ice column-averaged density of  $920 \text{ kg m}^{-3}$ , and for seawater,  $1028 \text{ kg m}^{-3}$ .

This work does not build or incorporate a firn model, and firn properties are assumed stationary. We have made this assumption explicit in *Errors and uncertainties*. Reviewer 2 writes

“as noted by other users or proponents of this methodology, it may rely (or not) on a few assumptions, including uniform firn density (both spatially and temporally), ice density (idem), and hydrostasy (negligible bridge stresses). You do mention bridge stresses, but seem to gloss over others (e.g. see below). Can you please elaborate on those other assumptions and their eventual impacts on your results?”

Regarding firn density, we assume no variation in time or space along the ice tongues. The section *Errors and uncertainties* has been amended with the note

We expect [firn-densification] to be small as the ice tongues considered are well below the equilibrium line and any remaining firn layer is expected to be thin and assumed constant. This is supported by field data described by Dutrieux et al. (2014) indicating firn compaction (at PG) to be negligible over a one year period.

If significant firn compaction does occur along the ice tongue flow line, our melt rate

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estimates would tend to over estimate downstream submarine melting, making our conclusions about the concentration of melt at the grounding line stronger.

We make a similar assumption for ice density. Again, densification downstream would result in an increased the melt rate gradient in our estimates. Snow presents a difficulty and prevents us from making conclusions about seasonal melt variations. Averaged over a year or several years density variations from melting or accumulating snow layers are assumed to cancel, which requires there to be no bias introduced by a regular trend in snow depth along the flow line.

### **Time averages**

Reviewer 2 asks,

“how are the time average computed? As the mean of monthly binned differences?”

Time averages are computed by aggregating a monthly means, described in *Methodology: Melt rates*. We modify the text to make it clear that we are averaging over monthly means.

We compute a temporal mean by averaging over average monthly binned estimates of  $Dh/Dt$  (January–December) in order to offset bias due to the optical imagery being more available during seasons with more daylight.

As the DEM pairs used extend over multiple months, we weight each month according to the fraction of the month contained within each time span and use the weight to distribute the contribution of each month to submarine melting.

### **Spatial identification of melt**

Reviewer 2 asks

“where is the result from one difference between 2 scenes assumed to reside? At one location between the start and end point, following a streamline? At the starting point? Does that choice impact the end result?”

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When calculating the Lagrangian ice thickness change, we simplify by assigning the computed difference to the starting point of the streamline. In reality, we expect that the melting is distributed over the entire streamline. This information could be used in the averaging and interpolation of melt rates, or a better approximation assigned the melt to some midpoint along the streamline could be made. To estimate the effect that this improvement would have we consider scenarios in the two following contexts:

- a point in the center of Nioghalvfjærdsbræ
- a point near the grounding line of Nioghalvfjærdsbræ

For a central point on Nioghalvfjærdsbræ, a typical surface ice velocity estimated from our data is  $650 \text{ m yr}^{-1}$ . For a one-year temporal baseline, this would shift the estimate in Figure 1 downstream by just over one 512 m pixel. For a point at the grounding line, where surface velocities are closer to  $1.3 \text{ km yr}^{-1}$ , the correction would be approximately two pixels. We think based on these examples that at these scales the difference in general is not large.

#### Miscellaneous comments, reviewer 1

1. Figure 1 shows our computed submarine melt rates, i.e. the caption is correct.
2. In Table 1, the volume fluxes do include estimates of the surface mass balance uncertainty, which is added to the  $Dh/Dt$  uncertainty to arrive at the final submarine melt error. This can be made more explicit in the text and caption. The uncertainty chosen ( $0.8 \text{ m.w.e yr}^{-1}$ ) at all locations was estimated from Table 4 in Noël et al. (2015). These measurements (made along the K-transect in western Greenland) are far from the ice tongues considered.

#### Miscellaneous comments, reviewer 2

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1. (Re: accidental comma) Corrected.
2. (Re: reference for statement that velocity is depth-independent) If the assumption of low lateral drag is permitted, then Weertman (1957) is an appropriate reference for this statement.
3. (Re: hydrostasy) Yes, although we have tried to make the case that hydrostasy may be reasonable at low resolutions that are nonetheless higher than the full ice tongue scale. Proper quantification of this error is non-trivial and requires an in depth analysis, as the degree to which any portion of the ice tongue is out of hydrostatic equilibrium depends on the spatial scale of thickness variations, the local melt rate history, and over which the ice has relaxed toward hydrostasy. While not the rigorous analysis that this problem deserves, we could point to modelling in Drews (2015) in which they show that under a specific set of circumstances, a  $\sim 1 \text{ km}$  channel in 280 m thick ice is nearly in balance while a  $\sim 1/2 \text{ km}$  channel is not.
4. (Re: PG channelization (Dutrieux, 2014)) Thank you for reminding us of this work.
5. (Re: PG pre-calving melt rates) Moving this to the discussion is one possibility, but breaking it up seems to harm clarity. Instead, we have rephrased this to de-emphasize it as a result and to ground it in what are safe assumptions (“...we do not observe submarine melting at PG to be a driver of substantial net volume loss in its current configuration. However, based on a conservative estimate of melting under the former terminus of PG of  $5\text{--}10 \text{ m yr}^{-1}$  and a calved area of approximately  $250 \text{ km}^2$ , we estimate that the pre-2010 melt flux may have been around  $13 \text{ km}^3 \text{ yr}^{-1}$  w.e. It is therefore possible that the imbalance between melting and advective replenishment was greater prior to 2010.”).
6. (Re: non-independence of draft and slope) Certainly, and this should be noted. We include minor revisions of the text to make this clear.

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7. (Re: channelization and heterogeneity) By heterogeneity, we attempt to summarize results in Sergienko (2013) indicating that across-glacier variability in flow and upstream geometry can lead to the development of channels. Our comment here may not add meaningfully to the discussion, and has been replaced with a discussion of methodological limitations. In terms of methodological constraints on detecting melting, we are limited by imperfect knowledge of the surface mass balance (drifting snow settling in surface depressions would mute the measured melt rate) and hydrostasy (bridging effects in regions of rapid melting would also mute the measured melt rate).
8. (Re: conclusion) We absolutely agree on the limitations of a four year record in drawing conclusions about climate, and would like to re-emphasize that these final clauses are speculative based on available data. The dynamical response of the grounding ice sheet to a melt signal is not a satisfactorily resolved question either, resulting in large uncertainties. We have slightly adjusted the *Conclusion* to make it clear that we recognize the limitations in our data for making inferences about climate (“While our mass deficit estimates are based on an average over a relatively short (4 year) time span, high rates of mass loss lead us to speculate that major changes will take place at 79N in the future as the ice tongue thins and eventually becomes ungrounded at its terminus.”).

## References

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Please also note the supplement to this comment:

<https://www.the-cryosphere-discuss.net/tc-2017-99/tc-2017-99-AC1-supplement.pdf>

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-99>, 2017.

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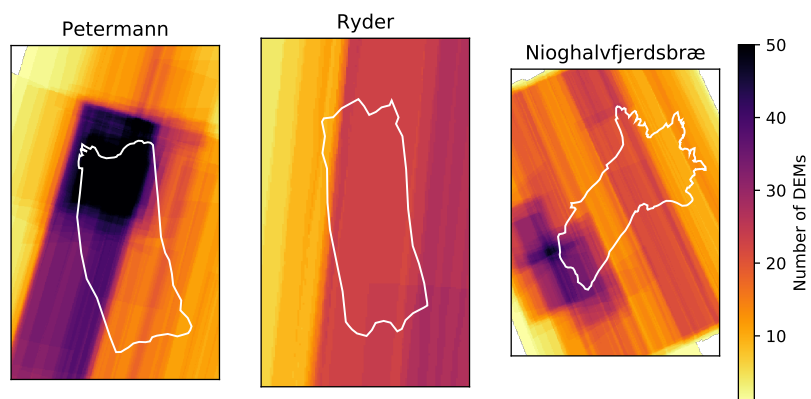


Fig. 1.

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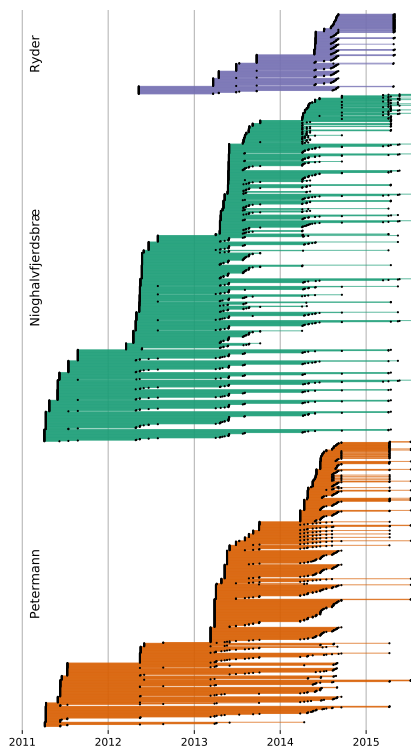


Fig. 2.

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