

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

Anonymous Referee #1

Received and published: 29 July 2017

The manuscript entitled “Submarine melt rates and mass balance for Greenland’s remaining ice tongues” by Wilson et al. 2017 shows measurements of subglacial melt rate using Worldview images. Using mass conservation in a Lagrangian framework, they measure the spatial pattern of melt rate and conclude that the ice shelf Nioghalvfjerdingsfjorden is losing mass while the two others, Petermann and Ryder, are still close from equilibrium.

The paper is well written. The use of high resolution stereo images such as Worldview to derive melt rate is good, and the results on the melt rate and mass balance of the ice shelves are interesting and important for the community. This paper should be published after revisions of the comments below.

First of all, I think that the title could be slightly different. I do not like the term “remain-

ing” without a date. The title also suggests that you are looking at all the ice shelves in Greenland, but the manuscript does not study Steensby, Ostenfeld, Hagen Brae, Academy or Zachariae Isstrom (I agree that they are much smaller). In addition, the date of the study (2011-2015) should be included in the title.

Secondly, more details on when exactly the Worldview data were acquired are needed and the authors should explain better how they are merged. The readers know that you use data acquired between 2011 and 2015, but were the images acquired only in years 2011 and 2015, or continuously during the 4/5 years? I believe that Worldview are rather small in coverage. Did the authors rely on different stereo images acquired at different time periods to cover entirely the ice shelves? The authors should provide (could be as supplemental material) a table and maps showing the time tags of the data combined here. They should also state that they assume no change in melt rate between 2011 and 2015.

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

Page 4

line 5: The authors should mention which densities (ice, sea water) were used to trans-

[Printer-friendly version](#)

[Discussion paper](#)



late the surface elevation into ice thickness.

line 9: Recent published grounding line from Mougnot et al. 2015 could also have been used for 79N.

Page 5

line 12: Would it make sense to use a firn model to refine the ice thickness estimate? I believe that RACMO has published this type of products.

Figure 1: Is it really the submarine melt or total melt including SMB?

Table 1: The authors mention that the uncertainties on the submarine melt rates are derived from surface elevation. It should also include uncertainties on the SMB. As stated previously, the manuscript would really benefit for a better description on the separation between submarine and surface melt rates.

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

Printer-friendly version

Discussion paper

