

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

### **Anonymous Referee #2**

Received and published: 30 July 2017

This manuscript presents an analysis of ice shelf melt rates and its spatial distribution from an interesting database of high resolution surface elevation maps over the three main ice shelves in Greenland. The manuscript is well written, and apart from methodological details that need to be addressed is of high interest to the community. So I recommend publication after relatively minor revisions.

Methodology and relevant details to be incorporated:

- for each ice shelf: temporal distribution of DEMs (one full DEM per year?); is there really so many DEMs that temporal aliasing issues are irrelevant? Could you please add more details on the subject?
- how are the time average computed? As the mean of monthly binned differences?

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- 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?

- as noted by other users or proponents of this methodology, it may rely (or not) on a few assumptions, including uniform firm 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?

Other comments:

Page 2, Line 22: remove comma before Falkner ref.

Page 3, Line 24: Reference? I hear this statement a lot, and it does make some sense, but how correct is it?

Page 5, Line 19: I agree, but at the very least you are trying to look at the spatial distribution of melt, so stating that hydrostasy works at the entire ice shelf scale does not really help your case. I understand that quantifying errors resulting from methodological assumptions is difficult, but you probably don't want to underestimate the impacts of such assumptions.

Page 6, Line 5: an independent source of quantification for PG channelized melt can be found in [Dutrieux et al., 2014], with some values that appear to be broadly consistent with yours.

Page 6, Line 21-24: This is all rather speculative. And this raises an important point: that of the spatial distribution of melt and its temporal variation, especially if a major change in the geometry like a calving event occurs. Maybe move to the discussion? Or at least attempt to clarify?

Page 6, Line 25: Ok. But is there a cross-correlation between draft and slope? If so (or not), those are not independent parameters, and may be worth noting.

Page 7, Line 16: What do you mean by ‘heterogeneity’? And also, how do your methodological assumptions impact the channel melt signal? Would you expect it to be smoothed? Enhanced? Can you trust it?

Your conclusion section/paragraph:

Can one use a 4-year record and deduce climatic trends? Shouldn't one expect to see temporal variability of melt? And if so do we know how this melt signal translate to ice dynamics? I would agree that there is a potential for a dynamical response if all things were stable in time, but they probably won't, and you may have sampled a particular time period, or not. So I think readers would benefit from a statement on the numerous possibilities ahead here.

Dutrieux, P., C. Stewart, A. Jenkins, K. W. Nicholls, H. F. J. Corr, E. Rignot, and K. Steffen (2014), Basal terraces on melting ice shelves, *Geophys. Res. Lett.*, 41(15), 5506–5513, doi:10.1002/2014GL060618. [online] Available from: <http://doi.wiley.com/10.1002/2014GL060618>

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

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