Reply to Reviewer 2 and other author changes: Manuscript #2019-125 by Crawford, Mueller, Crocker, Mingo, Desjardins, Dumont and Babin: "*Ice island thinning: Rates and model calibration with in situ observations from Baffin Bay, Nunavut"*

Original reviewer comments are in black.

Author replies are in blue.

This paper presents measured ablation rates of a tabular iceberg which calved from the Peterman Glacier, North Greenland, which are then used to calibrate a model for melt rates. The *in situ* data set, which comprises repeated ice-penetrating radar surveys, surface mass balance measurements, as well as oceanographic data nearby is very unique, and a valuable contribution to the cryosphere community. Previously, studies on iceberg ablation rates were mostly based on modelling alone, or estimation of ablation rates from satellite altimetry, which is not straight forward because of the unknown surface processes and densification rates of a possible firn layer, which is especially important for the Antarctic icebergs. Thus, I would see it as an excellent contribution to The Cryosphere.

We thank the reviewer for their comments and their appreciation for the unique data set and basal ablation analysis presented in our manuscript.

I would like to rise a few points which might need addressing before publication:

1. At some point (section 3.2.2) it is stated that for the calibration of the model it is assumed that delta u is set equal to the ocean current velocity. As the iceberg is grounded during much of the time this might be a valid assumption. Nevertheless, as the iceberg can be easily tracked by remote sensing data, it would have been possible to look at real drift velocities, and how they compare to the ocean currents. Some studies have suggested (e.g. Lichey & Hellmer, 2001, Jounal of Glaciology) that not only the ocean current is responsible for iceberg drift velocity and direction, but also the wind conditions. A comparison / discussion of these parameters would be an improvement.

We agree with the reviewer, it is the relative speed between the ocean and drifting ice island that is necessary to constrain, and this will be affected by wind speed and direction. It is indeed possible to track ice islands with remote sensing data, and members of the Water and Ice Research Lab at Carleton University made a tremendous effort to do so while creating the Canadian Ice Island Drift, Deterioration and Detection (CI2D3) Database. This database will be used in the future to take on investigations into ice island drift patterns, such as that suggested by the reviewer. We believe that it is much too big of a step to incorporate such an assessment into the current paper. Such a study would also reply upon coarse resolution data for ice island drift velocities and modelled environmental conditions, which deviates from the *in situ* measurements and ocean conditions used in our assessment. We have added a statement to section 5.1 that notes the influence of both wind and ocean currents on iceberg drift and Δu and reference the work by Lichey and Hellmer (2001) brought forward by the reviewer as well as Kubat et al. (2005).

2. In the introduction it is stated that this study is the first of its kind, for Arctic and Antarctic icebergs. However, to my knowledge there was a similar study set up for an Antarctic iceberg (Scambos at al., 2008, Journal of Glaciology) which might deserve a mentioning here.

Scambos et al. (2008) set up automated meteorological stations on two Antarctic icebergs, and paired one station with a stationary ice penetrating radar. Unfortunately, their ice thickness data was unusable as a result of interference from nearby instruments. Due to this, our dataset still includes the first long-term *in situ* ice thickness measurements from an iceberg in either Polar Region. We also present the first spatially distributed thickness change values, which were possible with our repeated mobile ice penetrating radar transects. The study by Scambos et al. (2008) incorporated novel field data and we agree that the work deserves mentioning as it is an important pre-cursor to our work. We have included text that describes this work in our Discussion (section 5.2).

3. In the discussion the big difference between the ration of basal and surface ablation rates from results of a former study is mentioned. When comparing these results it has to be considered the in case of the other study the Antarctic tabular iceberg started off with a firn column, while the Peterman iceberg did not have any snow cover, so in fact a blue-ice surface. If there is a firn column, surface melt water can percolate into the firn and refreeze, so the mass is not immediately lost. While on blue ice it is more likely to run off. The problem of refreezing melt water and firn densification is the biggest contribution to uncertainty for previous studies estimating ablation of tabular icebergs from altimeter data. For this setting it would be immensely helpful to have an in situ data set like the one presented here. This might be added to the discussion.

We agree that it is important to note the difference in surface conditions between our study subject and the large tabular icebergs in Antarctica. This is included in section 5.2. With this note, we also mention the iceberg firn observations of Scambos et al. (2008) and suggest that a follow-up study with concurrent ice thickness measurements would be of high value.

Author changes:

Greg Crocker's affiliation was corrected to Carleton University.

In the Abstract, "The calibration of the basal ablation model, the with such field data…." has been changed to "The calibration of the basal ablation model, the first known to be conducted with field data…"

We changed "deployment" to "collection of a long-term *in situ* dataset of ice island thinning" in the last paragraph of the Introduction.

The reference to Mingo et al. (forthcoming) has been updated in section 3.1 and References.

Minor edits to language and sentence structure have been made in section 3.2.1. We have added a reference to Oziel et al. (2019) in regard to our ocean velocity measurements in the same section. We also explicitly state that keel depth was derived using the measured ice thickness.

Edits have been made to ensure that 'data' are plural

T_A was changed to T_a in the Results to maintain consistency throughout the paper.

We explicitly state the length of time that basal ablation was estimated in section 4.1. A redundant sentence at the end of this section was removed as well.

We have modified the language at the end of section 4.2.1 to reflect our decision (not recommendation) to apply corrections to the oceanographic data.

Weeks et al. (1973) was corrected to Weeks and Campbell (1973) at the start of section 5.1.

An additional Løset (1993) reference has been added in regards to the assignment of ice temperature values in section 5.1. The two Løset (1993) reference are now assigned 'a' and 'b'. We now cite and reference FitzMaurice et al. (2016) and FitzMaurice and Stern (2018) in the same section.

We have corrected a reference to Ballicater (2012) to Ballicater Consulting (2012) at the end of section 5.2.

The reference to Sazidy et al. (forthcoming) has been updated in section 5.2 and References.

"…after an ice island stops freely drifting…" was modified to "…after an ice island stops drifting freely…" in the last paragraph of section 5.2.

A small amount of text was removed from the Conclusions as it was redundant with the preceding section.