

Interactive comment on “Frontal ablation and temporal variations in surface velocity of Livingston Island ice cap, Antarctica” by B. Osmanoglu et al.

Anonymous Referee #2

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General points: In this paper, the authors estimate ice frontal ablation of the ~700km² glaciated portion of Livingstone Island, situated off the north west tip of the Antarctic Peninsula, a study area. The authors use a flux-gate approach to estimating total ablation, which estimates ice mass flux from depth-averaged ice flow near the margin from observations of ice velocity. Due to a dearth of ice thickness measurements, flux gate geometry is estimated from the observed velocity field by making various assumptions about the ice flow dynamics. Large uncertainties in the resulting ablation estimates arise from inaccurate flux-gate geometries, which in turn result from assumptions made when estimating depth-averaged velocity from observed velocity and the fact that longitudinal coupling is neglected. Furthermore, the authors point out

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that there is significant temporal variability in observed ice velocity, which introduces additional uncertainty into both the estimates of ice flow through the flux gate, and the geometry of the flux gate itself. While these various uncertainties are acknowledged, they are not fully explored, which calls into question the robustness of the frontal ablation estimates. The authors have clearly performed a large amount of analysis, which could be split into 2 or more separate studies. The inclusion of the various strands of analysis in a single study means that the manuscript feels a little disjointed and may benefit from being re-structured.

The subject area sits firmly within the scope of the TC and will be of interest to the wider scientific community. New data is presented for an under-studied glaciated region, though the wider significance of the study should be emphasised. Uncertainties arising from lack of primary observations prevent substantial conclusions from being made, though these limitations are well discussed. The methods and assumptions are outlined clearly and concisely, though could be elaborated on in some areas. Overall, the paper is well written and figures are clear on the whole, though I recommend a few changes be made.

Specific points: The introduction attempts to emphasise the importance of frontal ablation of this region but I found some of the points unclear. For instance, it is not clear if the authors' definition of frontal ablation includes subaerial melting – on line 30 they define it as “the sum of iceberg calving and submarine melting”, while on line 66 it is defined as “the loss of mass from the near-vertical calving fronts of the marine-terminating glaciers, including loss by calving, subaqueous melting and sublimation”. As stated, the mass flux method used cannot discriminate between the different mechanisms of ablation, so the latter definition appears correct. If so, the relevance of the statement on lines 51-54 is not clear – if the method used cannot discriminate between the different ablation mechanisms, how can this study “better quantify the dynamics mass losses” Also, does the cited Shepherd et al., (2012) estimate of mass loss include Livingstone Island? If so, this should be stated, and if not, the relevance of the

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present study to the glaciers of the Antarctic Peninsula should be clarified.

A concise summary of relevant previous studies in the study area is given, but it would be informative to include here e.g. mean annual temperature, typical annual accumulation rates and variability, any accumulation gradients etc. Also, I feel more could be said about the ice cap in relation to other ice-covered areas on the periphery of the Antarctic peninsula - how applicable are the results of this study to other ice masses in the region? What is the estimated total ice mass (or ice area if insufficient ice thickness estimates) of the South Shetland islands for instance?

In sections 3.1 and 3.2 the authors describe ice thickness and ice velocity measurements acquired at the Hurd peninsula. The peninsula appears to be covered by thin, slow-flowing, land-terminating ice, so it is not clear what these measurements can tell us about frontal ablation at the ice cap. The relevance of these measurements should be clarified.

The various SAR data used in the study are described in section 3.3 – citations should be provided that describe the satellite sensors in detail.

Figure 1: Caption states “Green colour denotes ice free areas”. In my copy, the area appears to be brown rather than green. I think it is important to indicate the ice-free areas in subsequent maps of the ice cap (figs 2, 3 and 5).

Line 167: The authors state that “Details on the exact penetration depth of the SAR signal are unknown” but I feel this warrants more discussion. In particular, it would be useful if this led into a discussion of typical firn depths at the ice cap as this will have implications for the depth-averaged density used in the flux gate calculations.

Line 209: The justification and reasoning for scaling the values between 0.75 and 1.25 should be clarified.

Line 221: Suggest change “done” to “performed” and “put on” to “resampled to”.

Line 225: Suggest change “We further...” to “Further, we...”.

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Equation 2: The chosen value of gamma should be explained and justified. This value is likely to vary across the ice cap and will affect the calculated ice flux value. Although the authors acknowledge this in section 6.2, I feel that more could be said on this point, especially in relation to section 3.2 and figure 5.

Equation 3: Value of rho(ice) should be justified – given the low ice thickness of some of the flux gates (9 are less than 100 m thick) and relatively high accumulation rates at the ice cap (should be stated), I suspect the depth averaged density is lower than 900 kg m⁻³. Clearly, this will reduce the calculated discharge values.

Line 395: What is the scientific basis for separately fitting equation 4 to the different areas based on flow speed? This should be explained.

Line 407: the authors state that 3 of the 4 error sources can be quantified by comparing the estimated ice thickness with available data, yet have already acknowledged the lack of ice thickness measurements. This seems unsatisfactory, especially as the only ice thickness measurements are in areas of slow-flowing and predominately thin ice. Figure 4: More discussion of the spread of the slow, medium and fast glacier data is warranted. What is the significance of the beta value?

Line 433: suggest change “the fits are” to “the fit is”.

Line 445: suggest change “the fit” to “the poor fit”.

Section 6.2 does a good job of highlighting the various sources of uncertainty. It would be interesting to include a sensitivity analysis for each source.

Section 6.3 introduces observations of temporal variations in ice flow and discusses ideas relating to temporal changes in the ice flow regime resulting from e.g the effect of rainfall on the basal hydrological system. This introduces additional uncertainties into the preceding analysis (e.g. parameter values in equations 2 and 4) which do not appear to be fully explored. It would be instructive to know the acquisition dates of the SAR data used to derive the ice flow fields and ice thickness estimates, in relation

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to temperature and precipitation data from the nearby met station, as well as in relation to the periodic signals shown in figure 6. The variability in ice flow, which occurs throughout the year, calls into question the validity of using SAR image pairs with temporal baselines of 46 days and longer to estimate a representative ice flow field. More discussion of these issues is warranted.

Table 1: It would be interesting to see the mean values of ice flow at the fluxgates and the values of \hat{A}_{SAR} expressed as a percentage of the mean flux gate velocities.

Figure 6: only a few of the flux gates show significant r^2 values. Discussion of this variability if the r^2 and phase values is warranted.

Line 659: suggest change "estimation" to "estimates".

Line 666: Suggest change "account the temporal" to "account temporal".

Figure 7: I am not convinced of the merit of including this figure.

Interactive comment on The Cryosphere Discuss., 7, 4207, 2013.