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Interactive comment on "Surface speed and frontal ablation of Kronebreen and Kongsbreen, NW-Svalbard, from SAR offset tracking" *by* T. Schellenberger et al.

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Summary:

The authors present detailed time series of surface speeds for two large tidewater glaciers located in NW Svalbard, Kronebreen and Kongsbreen, using SAR and GPS observations. The authors show that SAR- and GPS-derived speeds are generally in good agreement but that speeds derived from Radarsat 2 Ultrafine data are more accurate than speeds derived from the "Wide Mode" SAR data. Seasonal changes in speed are correlated with changes in surface melting and rain events, likely due to the effects of increase freshwater fluxes delivered to the glacier bed. Frontal ablation

rates calculated with the speed observations indicate that frontal ablation varied both seasonally and inter-annually over the past several years and that terminus retreat was the dominant mass loss term for the glaciers during periods of rapid change.

The speckle (or feature) tracking method has been applied to SAR data for a number of glaciers and has been shown to produce robust speed records, as the author's data further support. My only major concern in regards to the methods used to estimate frontal ablation is in regard to thickness estimates. Although the radar-derived thickness estimates for Kronebreen are in good agreement with a borehole measurement, the locations of the thickness measurements with respect to the flux gate are unclear. Additionally, I think that the uncertainty in the Kongsbreen thickness estimates is low as described in more detail below.

General Comments:

1) The authors make an effort to provide upper and lower bounds for their ice flux and frontal ablation rates that take the uncertainty in their observations into account; however, I think that the uncertainty in their thickness estimates is underestimated for several reasons. First, the thickness estimates across the flux gates are assumed to be constant in time. Given the seasonal and inter-annual changes in velocity discussed in the manuscript, this assumption is most likely invalid. Second, as described above, the location of the thickness measurements for Kronebreen with respect to the flux gate are unclear. Any spatial offset between the observations and the flux gate will lead to some uncertainty since both surface and bed elevations should vary in the along- and across-flow directions. Finally, the thickness estimates for Kongsbreen partially rely on water depths observed down-fjord from the 2007 terminus position. Given that the rapid retreat of numerous tidewater glaciers throughout the past several decades has been strongly controlled by the presence of bedrock depressions, it is possible that the retreat of Kongsbreen was also topographically controlled. Therefore, although the water depths varied little immediately down-fjord of the 2007 terminus, they did not necessarily maintain a constant depth up-glacier.

In order to increase confidence in the thickness estimates at both glaciers, I suggest that the authors attempt to extract changes in ice surface elevation from repeat DEMs acquired during the observation period (if available). The DEMs could be used to (1) constrain temporal changes in flux gate thickness and (2) test the validity of the assumption that the Kongsbreen calving face height remains constant. Assuming that the ice thickness follows the modified flotation criterion proposed by Vieli et al. (J. Glaciol., 2001) based on observations from Hansbreen (i.e., the ice thickness at the calving front is a constant fraction greater than the flotation thickness), the authors could also assess whether the it is reasonable to assume that the bed across which the terminus retreated has the same depth profile as the bed depths down-fjord from the 2007 terminus position. Therefore, the acquisition of additional DEMs would greatly increase both the accuracy of the ice thickness estimates and confidence in uncertainty estimates.

2) In the Results section, please be more specific/quantitative when describing the data (i.e., replace “most”, “lower”, etc. with percentages or values). Similarly, please define your criterion for “stable” speeds and terminus positions in the methods section. As is, the reader is unsure whether “relatively stable” terminus positions are those that vary with +/- 10, 50, or 100 meters and the time scales over which you are assessing stability. The same comment in regard to speeds. If stability was assessed using the uncertainty in the datasets, please make that clear so that the reader knows stability equates to insignificant change (i.e., change not exceeding uncertainty).

3) Finally, in the Discussion you provide an explanation for the correlation observed between speed and surface melting/rainfall. Although the effects of the seasonal evolution of the subglacial drainage system have been well-documented for a number of glaciers, please provide a more detailed explanation. For example, is there a known lag in water transport from the surface to the bed? Does the water drain to the bed through moulins, crevasses, or both? Investigating the glacial hydrologic network is obviously outside the intended scope of this paper but it would be helpful to the reader

to have a more detailed description of why the observed correlation would exist along with additional references.

Detailed Comments:

p. 6195, lines 14-18: It would be helpful to clarify that the cited studies all examine subaqueous/submarine melting of the terminus.

p. 6196, line 2: Change “offset tracking on” to “offset tracking of”.

p. 6196, line 11: “deployed”? I think “employed” would be more appropriate.

p. 6196, lines 18-21: I found that the time referencing is a bit confusing here. Was the second surge just in 1995 or from some year before that until 1995. If only in 1995, remove “until”. Also, please replace “at that time” in the second half of the sentence with the year(s) for the surge you are referring to here. As is, it is unclear which surge if being referenced.

p. 6198, lines 5-6: Please reword the second half of this sentence so it is clear that polar night and cloud cover inhibit the use of optical imagery in the winter and summer, respectively.

p. 6198, line 23: Please be more specific than “sub-daily”. The frequency of the GPS observations isn’t likely to lead to any offset in your speed comparison since the SAR-derived speeds are averaged over several days, but the reader cannot assess its influence without a quantitative value.

p. 6199, section 3.3: As described in detail in my comments above, I think some additional work can be done to estimate ice thickness and constrain uncertainty. At the very least, this section should describe where the Kronebreen thickness observations were acquired with respect to the flux gate and the fact that the glaciers may have undergone rapid retreat because they were initially grounded across basal depressions, meaning the ice thickness may not be uniform along flow.

p. 6200, line 22: How do you assess whether speeds are erroneous? Do you manually remove outliers identified by visual inspection? Do you remove values that exceed the mean +/- 3 standard deviations for a small sample region?

p. 6201, lines 23-24: I assume that the variations in the location of the flux gate with respect to the terminus are due to fluctuations in the terminus position. Please clarify in the text whether that assumption is true.

p. 6202, line 15: Change “speed” to “speed correction factor”.

p. 6205, line 3: Replace “most” with a value (percentage or “X of Y”).

p. 6205, lines 7-8: What were the seasonal amplitudes?

p. 6205, line 10: Maximum, mean, or minimum summer speed?

p. 6205, lines 10-11: Please be more specific. How much lower and higher were speeds? Does “lowest level” mean “slowest speed”? If so, also list the speed.

p. 6205, Section 5.1.2: I suggest that you define terminus stability in your methods section since you frequently describe the terminus as “relatively stable” here and in section 5.2.2. Please see comment #2 above.

p. 6206, line 14: What was wrong with that image pair? Please state how you assess the quality of the speed maps.

p. 6206, line 16: The values listed are frontal ablation rates (Gt/a) so the beginning of the sentence should be change to “Total frontal ablation rates. . .”.

p. 6206, lines 20-25: If these frontal ablation rates are calculated using a different speed dataset than was used in the previous paragraph, please make that clear.

p. 6207, line 6: Be more specific than “most”.

p. 6207, line 6: Change “This data indicates” to “These data indicate”.

p. 6207, line 7: As with terminus positions, please define the criterion for “stable”

speeds in the methods.

p. 6207, lines 9-10: How far inland did the speed-up reach (in km)?

p. 6207, lines 12-15: This sentence is a little confusing since you just said the lowest speed at P#1 occurred in autumn 2011. Were the speeds comparable before and after the event at P#1 and slightly lower at P#2? Please clarify.

p. 6208, lines 1-17: Replace “major” and “minor” in this section with values. Are “major” changes those that exceed uncertainty or some set amount?

p. 6208, line 6: Define “typical”.

p. 6208, line 8: I’m unsure of what you mean by “lacking behind”.

p. 6208, Section 5.2.3: As in the Kronebreen section on frontal ablation, please insert the word “rate” after “frontal ablation” since you are presenting Gt/a values.

p. 6209, line 1: How do you define significance?

p. 6209, Section 6: I suggest replacing “linked to” with “correlated with” since your data show a correlation between the variables. Also, I suggest ending the first sentence in paragraph 2 of the discussion after “melt water input and rainfall”. Since you are inferring that the correlation between the variables is due to meltwater effects on basal hydrology, I suggest you remove the word “influencing” and start the second sentence with something like “We attribute the observed correlation to the influence of melt water and rainfall on the water pressure at the bed and. . .”.

p. 6210, lines 5-12: I would think that creep would close the subglacial channels if water pressures decline after the January rain event. Please provide an explanation as to why the channelized system would persist during a several month period prior to the onset of the subsequent melt season.

p. 6210, line 15: “1990s”

p. 6212, line 19: Change “vastest part” to “fastest period of retreat”.

p. 6212, line 21: “The frontal ablation rate. . .”

p. 6212, line 25: What is the “actual” flux? Is this the calving flux?

Figs. 7&11: Change “Dez” in legend to “Dec”.

Interactive comment on The Cryosphere Discuss., 8, 6193, 2014.