The corresponding editor in The Cryosphere
Ref: MS# tc-2014-55

Dear Dr. Eric Larour,

Thank you for your time to handle our manuscript, and apologies for the delay in submitting the revision; we have asked for an English-editing service. We have attached the revised manuscript, the response letter separately to Reviewer#1, #2, #3 and revised supplementary material as a separate file.

Basically incorporating all of the reviewers' comments, we substantially re-organized and re-wrote the manuscript, and slightly changed the title of some of the sections. Below is the summary of the significant changes comparing with the previous manuscript, and then we show our point-by-point responses to the reviewers.

We believe our revised manuscript is improved suitably for the publication in The Cryosphere.

Introduction

This section is substantially re-written, focusing on the seasonal evolution of ice velocity particularly during wintertime at surge-type glaciers. Moreover, we tried to make the objective much clearer than that in the previous manuscript. This was made possible because we deleted those sentences that were associated with the glacial erosion; we completely deleted descriptions related to glacial erosion.

Data sets and analysis method

We added the detail of ALOS/PALSAR and the measurement uncertainty of pixel offset tracking.

Observation results

One of the significant changes is that we moved the section of surging glaciers to the supplementary material. This is partly because the section could make readers confuse with the results and discussions, and partly because Reviewer#2 and #3 suggested to do so. This modification caused the changes in some figure numbers. In the main text, we mainly focused on the observed winter speed-up in the upstream and downglacier propagation. We explained each observation result in more detail. More importantly, although we did not explicitly mention in the original manuscript, we stressed that the wintertime velocity at some glaciers is comparable to, and sometimes higher than that in spring/early summer.

Discussion

We added some paragraphs in this section. At first, following the comment by Reviewer#3, we mentioned the meteorological information based on Alaska Climate Research Center data. This indicates that the wintertime temperature is significantly below freezing. Moreover, each glacier's location in this area is much higher than that at Variegated glacier. Under such circumstances, our studied glaciers are located in sub-polar region, which indicates that there should be little surface meltwater during in winter season. Thus, the winter speed-up mechanism and downglacier propagation phenomenon is clearly different from the summer speed-up.

Secondly, we commented on referred "mini-surge" described in Kamb et al. (1985) and Kamb and Engelhardt (1987). Although mini-surge is well-known in this area, we mentioned that not all previously reported mini-surges occurred in winter. Indeed, a mini-surge defined in Kamb and Engelhardt's paper indicates dramatically accelerated motion for a roughly 1-day period, which occurred repeatedly during June and July in 1978-81. Moreover, no comprehensive wintertime velocity observations have been performed in the upstream region.

Thirdly, we compared our results to the previous studies that were referring to flow velocity evolution toward winter (Iken and Truffer, 1997; Sundal et al., 2011; Burgess et al., 2013). No studies indicate the spatial and temporal changes in ice velocities in upstream throughout wintertime. Besides, as we noted above, we emphasize that our velocity data do not simply indicate the gradual speed-up from fall to next spring.

Moreover, we indicated that our results cannot be explained by previous understanding based on the surge mechanism proposed by Kamb et al. (1985). This is because we need to consider a mechanism that can trap water in the upstream in winter so that the subglacial water pressure can be maintained high enough to generate basal slip. Our winter speed-up data are complementary to englacial water storage in Lingle and Fatland (2003) because there have been very few winter speed-up observations except those by Lingle and Fatland (2003).

Besides, we added the discussion about the role of till deformation for surging. This is because till deformation is another mechanism to cause surge initiation, and some glaciers in Alaska and Yukon have till layers (e.g., Truffer et al., 2000).

Reviewers #1 and #3 commented the discussion part was too speculative. Given these comments, we removed the description about the glacial erosion and the formation of basal bed topography, and we focused on the seasonal evolution of flow velocities during wintertime at quiescent surge-type glaciers.

References

We added and removed some references due to the re-organization and modifications. Below is the list of changes. Also, some references are moved to the supplementary material.

Added

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ruptures involved in the 2008 Sichuan earthquake inferred from SAR image matching, Geophys. Res. Lett. 36, L07302, doi:10.1029/2008GL036907, 2009.
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Sole, A., Nienow, P., Bartholomew, I., Mair, D., Cowton, T., Tedstone, A., and King, M. A.: Winter motion mediates dynamic response of the Greenland Ice Sheet to warmer summers, Geophys. Res. Lett., 40, 3940–3944, doi:10.1002/grl.50764, 2013.
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Deleted

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Berger, A. L., Gulick, S. P. S., Spotila, J. A., Upton, P., Jaeger, J. M., Chapman, J. B., Worthington, L. A., Pavlis, T. L., Ridgway, K. D., Willems, B. A., and McAleer, R. J.: Quaternary tectonic response to intensified glacial erosion in an orogenic wedge, Nat. Geosci., 1, 793–799, 2008.
Egholm, D. L., Nielsen, S. B., Pedersen, V. K., and Lesemann, J. E.: Glacial effects limiting mountain height, Nature, 460, 884–887, 2009.
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Yukon Geological Survey: available at: http://www.geology.gov.yk.ca/821.html (last access: 24 May 2013), 2011.

Figures

In Figure 1, we deleted some flowlines at Klutlan, Barnard, Malaspina, Kaskawulsh, and Kluane Glaciers bacause we don't show the data along the flowlines in the manuscript. Moreover, we added the name "Ottawa" in the figure due to the Reviewer#1's comment.

The original Figures 2-5, 7 in the previous manuscript are moved to the supplementary material and renamed as Figures S1-S5. We thus re-assigned the figure numbers.

In the new Figure 2, we put alphabets to each panel (a-z) so that it will help readers to clarify to which panel we refer. Moreover, we added the black circles in some panels where the Ottawa glacier undergoes surging.

In the new Figure 5, the result of Agassiz and Donjek Glaciers are combined in the one figure.

Supplementary material

In this material, we first explain the RGB method that allows us to visualize how SAR intensity changes due to surge initiation. Next, we show the spatial and temporal velocity changes at three surging glaciers during their active phase. We described the details in the supplementary material and added the references section.

Added references

Bevington, A., and Copland, L.: Characteristics of the last five surges of Lowell Glacier, Yukon, Canada, since 1948, J. Glaciol, 60(219), 113-123, 2014.

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The Point by point responses to the reviewers

Below are our responses to the three anonymous reviewers. The blue sentences in italics indicate the reviewers' comments, followed by our replies that have already been uploaded on the *TCD*. We indicate in red where the additional explanations are inserted in the revised manuscript.

Reply To Reviewer #1's comments

My primary concern is that most of the observed variations in flow speed, while interesting, aren't of great surprise and appear to be, at least as shown here, as examples of already published dynamics. As such, I think what this paper contributes, is not the mechanics, but an example of how variable velocities are in the St. Elias because of these mechanics. For example, it appears mini surges are common throughout the range.

While we agree that "mini surges" appear to be common around the study area, there are, to our knowledge, no comprehensive observations of them in terms of both spatial and temporal coverage. We could not find any previously published velocity observations throughout wintertime in the upstream. More importantly, as we discuss below, we do not think that the observed variations in flow speed could be explained by the previous understandings on basal sliding and glacier hydrology. However, this discrepancy is probably due to our insufficient review of previous studies, and thus we will significantly modify the Introduction in the revision. Also, we will describe in more detail that the observed winter speed is nearly comparable to or even faster than the summer speed at some of the glaciers.

We added this content at P7L27-P8L10, comparing with some previous studies.

I think perhaps some of the confusion here is that the seasonal minimum in flow velocity is actually in fall, and through winter gradual acceleration occurs as cavities close and water pressure increases. (Iken and Truffer, J.Glacio.1997; Truffer et al., J.Glacio., 2005; Sundal et al.,Nature, 2012; Sole et al., GRL, 2013; Burgess et al., GRL, 2013).

While we had read some of the suggested references, we examined them again. We agree that the seasonal minimum is in late summer to fall, which is referred in those papers. We explain below how our winter speed-up data are different from the previously published data.

Iken and Truffer (J. Glaciol., 1997) demonstrated gradual speed-up from fall to winter at the ~2km long downstream section of the temperate Findelengletcher in Switzerland, and the velocity continues to increase and reaches maximum in summer. In contrast, our observed winter speed-up was taking place in the upstream, and the winter velocity does not further speed-up toward summer. For instance, the winter velocity takes nearly the seasonal maximum at Anderson glacier (Fig. 8a). Hence, the observed winter speed-up is qualitatively different from the previously known signals, and cannot be explained by standard theory.

Truffer et al. (J. Glaciol., 2005) showed the relationship between mass balance and winter velocity during 2004-2005, when there is heatwave in the summer. Mean annual velocity is lower in 2004/2005 than other years. However, they did not show any data set that indicate winter speed-up.

Sundal et al. (Nature, 2011 (probably not 2012)) examined how ice speed-up and runoff are related at land-terminating glaciers in Greenland. We agree that ice speed-up can be affected by the amount of surface runoff at each year, and can see the differences between high and low melting year. The results indicate that the ice velocity at high melting year gradually increase from fall to winter (Fig. 3a of Sundal et al.). However, the ice velocity does not accelerate in low melting year (Fig. 3b of Sundal et al.). Moreover, they did not report how the speeds evolve in space during winter, and the maximum speed is apparently observed in early spring to summer.

Sole et al. (GRL, 2013) found a negative correlation between summer melt and winter displacement at a land-terminating glacier in Greenland. The Fig. 2 in Sole et al. indicated the velocity evolution at each site from 2009-2011. While we agree with summer speed-up as cumulative PDD increases, the Fig. 2 doesn't indicate acceleration from fall to winter and the maximum is clearly in summer. Besides, there are no data from December to March. The Fig. 3d in Sole et al. indicated displacements at each site in 2009, 2010, and 2011 for early summer, late summer, and winter. This figure seems that displacements in winter are larger than those in early and late summer. However, the winter period is defined as that from September to April (as written in the Supporting Information), which is much longer than the period during early and late summer. Thus, it is unclear whether winter speed is faster than summer speed.

Burgess et al. (GRL, 2013) reported inter-annual changes in the winter velocities, with which they associated with the amount of summer melt throughout Alaska. We agree with the negative correlation between summertime PDDs and January velocities. While they showed the relationship between velocity anomaly and PDDs anomaly (Fig. 2b of Burgess et al.), we cannot find any data sets that indicate winter speed-up.

At P2L8-14 in the introduction section, we mentioned the winter velocities in some previous studies (Sole et al., 2013; Burgess et al., 2013). However we made it clear that there have been fewer comprehensive velocity measurements throughout wintertime particularly in the middle-to-upstream region of mountain glaciers. In the section of Observation results, we compare our data with the previous studies at P7L27-P8L10.

Your observations of "summer" velocities are actually late summer to fall thus I don't see it surprising that velocities are lower in Aug-Oct than mid winter. Also, as you say, many surges begin in winter, and such is likely the case for mini-surges as well.

We are aware that the seasonal minimum is in late summer to fall, and agree that the surge "initiation" or, initiation of winter speed-up can be explained by cavity closure and subsequent water pressure increase. However, it is still an open question why and how the water pressure increase and subsequent speed-up can be maintained without any input of meltwater from the surface; we consider that this is an important point that we should clearly address in the revision. Indeed, Kamb (JGR, 1987) stated in the Introduction of his seminal paper, "The discussion concentrates on the mechanisms of surging in spring and summer when relatively large amounts of water are available to the basal water conduit system." Kamb's theory is based on the observations of the 1982-83 surge at the Variegated Glacier. The figures in Kamb et al (Science, 1985) actually indicate that the flow velocity seems constant during January to March but reveal acceleration only after April.

We added more explanations about our data and explained how our datasets were different from previous studies at P2L15-27. Our interpretation is added in a new paragraph at P8L11-23 in Discussion.

Thus the downstream propagation on Anderson for example appears to be as a excellent picture of annually recurring mini-surges.

In order to correctly put our study in the context of glaciology, we re-examined the previous literatures on "mini-surges", which were discussed in Kamb et al. (1985) and Kamb and Engelhardt (J. Glaciol., 1987). We found, however, that the observed "mini-surges" occurred in summer prior to the 1982-83 surge at Variegated Glacier. Although Kamb et al. (1985) noted "wintertime velocities began to show an anomalous increase" after 1978, the actual measurements by Raymond and Harrison (J. Glaciol., 1988) have been performed only two times a year in September and June, and thus it could contain the spring speed-up signals (Harrison and Post, Ann. Glaciol., 2003). As such, it should be noted that not all the mini-surges in the previous literature were occurring in winter. Incidentally, the speed-up episode we detected at the upstream of Hubbard Glacier in 2009 surely occurred in winter.

We referred to 'mini-surge' in Kamb et al. (1985) and Kamb and Engelhardt (1987) at P7L17-26 in the discussion section.

Burgess et al., GRL 2013, also identified wintertime seasonal acceleration in Alaska; it would be worth seeing how these changes compare. I encourage the authors to consider their results again considering previous knowledge more carefully.

As we understood, what Burgess et al (GRL, 2013) reported is not wintertime acceleration but inter-annual changes in the winter velocities, with which they associated the amount of summer melt. The Referee#1 might be confusing Burgess et al (GRL, 2013) with Burgess et al (Nat. Comm., 2013), because we found a phrase in Burgess et al (Nat. Comm., 2013), "... have been observed to have velocity maximums in spring, minimums in fall and intermediate speeds in mid-winter." Examining our velocity data again more carefully, we are now sure that the upstream velocities of some glaciers (e.g., Anderson, Chitina, Walsh) in winter are nearly comparable to those in early summer. We will more clearly state this point in the revision.

We described our results in more detail and the differences between our data and previous studies at P7L27-P8L10 in the Discussion.

If done well, I think these results would be a substantial contribution. Writing clarity in this version was also an issue, but given the substantial changes needed, I will mostly comment on more substantive points.

Update intro with proper literature review.

Thank you again. It is now clear that the original manuscript failed to adequately deliver our findings, probably because the reviews of previous literatures on glacier surge dynamics were lacking in the Introduction.

We substantially re-wrote the manuscript, particularly focusing on seasonal evolution of ice velocity particularly during wintertime at non-surging surge-type glaciers.

P2614 L8 How did you correct for the stereoscopic effect?

L11 How does your elevation-dependent correction work?

We used ASTER DEM to correct for the effect, and confirmed that there remained few topography-correlated artifact offsets. Our approach is basically the same as described in Kobayashi et al's Wenchuan earthquake paper (GRL, 2009).

The statement of the stereoscopic correction is added at P4L14-20 in the section of Data sets and analysis method.

What are your uncertainties in flow velocity?

The uncertainties of offset tracking data have been estimated to be ~0.3–0.4 m at the rugged terrain on the basis of two data images with ALOS/PALSAR's 46-day intervals acquired at non-deforming areas (Kobayashi et al., GRL, 2009). Assuming linear temporal evolution, the errors in the velocity estimate can be inferred approximately less than 0.1 m/d. In fact, we derived the average velocity data over the 350m×350m area along the flow line and estimated the measurement error to be less than 0.1m/d from the standard deviation at each area.

We added the explanation at P4L29-P5L2 in the section of Data sets and analysis method.

Section 3.1.3 This tributary actually has a name: Ottawa Glacier

Thank you for the information. We will replace "the tributaries" with "Ottawa Glacier".

P2617 L15 This is typical of surging glaciers in quiescent phase (Burgess et al., Nat-Com 2013)

What we would like to tell was that while the lower reaches behave like normal summer speed-up, the upper reaches exhibit winter speed-up at each glacier. Thus, the summer speed-up seems to take place every glacier at least in the downstream.

We re-wrote the sentences so that they will be much clearer to the readers at P6L15-20.

L25 I have never heard any reason why winter speed up would be dependent on glacier size, it occurs on ice sheets as well (Zwally et al., Science 2002)

There seems to be a confusion here probably because of our poor wording. We agree with you and consider the winter speed-up occurs regardless of a given glacier's size.

This statement is written at P6L25-27.

L28 Looks to me to be late winter to early spring, could this be the spring acceleration?

As pointed out, the signal includes spring acceleration. However, the velocity changes are small, but the speed, especially in the black-square section, gradually increases as winter approaches.

This statement is written at P6L28-P7L1.

P2618 L2 Don't really see this downstream propagation here. More just an acceleration.

We think you pointed out at Fig.10. From 2008 to 2009 and from 2010 to 2011, the down propagation is clearer to us

This statement is written at P6L28-P7L1.

L11 These variations are clearly not due to snow accumulation. This test is unnecessary.

We removed the sentences.

Discussion should be rewritten considering previous knowledge.

We will substantially clarify our new findings in the discussion.

We substantially re-wrote our new findings over the discussion section, comparing to the previous studies at P7L6-P10L2.

P2620 L9-21 This all seems tangential and too speculative given your results.

As pointed out, this part might be overly speculative. We will substantially rewrite the Discussion section, but consider that some speculation would be helpful and necessary.

We removed some paragraphs about the glacial erosion and the formation of an overdeepened concave basal topography, and focused on the winter speed-up mechanism comparing the surge initiation mechanism proposed by Kamb et al. (1985). However, we mentioned the presence of basal crevasses that keep the large amount of summer meltwater at P9L24-P10L2.

Figure 6 Unclear it dates are by column or row, or how these maps progress in time? Is there temporal overlap here?

We apologize for the inconvenience. To derive the velocity data, we use two adjacent satellite paths, and there are some temporal overlaps in this glacier. The original Figure 6 is now modified as a new Figure 2.

We assigned alphabets to each panel (a-z), and the more explanation is added in the captions and the main text. This improvement helps readers to clarify what they see on which panel they focus.

Fig 10 State what the box is in the caption.

The box indicates the section where we can clearly observe seasonal changes. We will rewrite the caption.

We explained about the black box at P6L32-P7L1.

Reply To Reviewer #2's comments

This short paper delivers a set of surface velocity time series data spanning 5 years. The spatial coverage is good, with data from 3 likely surging and 7 non-surging, surge type glaciers near the Alaska-Yukon border. This data set should find its way into publication, as it may offer some new insights into how surge behavior may be initiated and terminated as well as how the surging state differs from the quiescent phase behavior.

Thank you for your comments.

Unfortunately, I think the manuscript requires major revisions before it is ready for publication. Setting aside the writing, which currently makes the manuscript very challenging to read, the discussion and conclusions do not fit well with my understanding of surge type glaciers. The authors could do much more in terms of investigating their results in comparison to others, and providing better discussion of when and where the patterns of change they observe are consistent or different with what others have seen.

It became clear that we failed to deliver our findings correctly.

We re-organized and re-wrote the manuscript, particularly focusing on seasonal evolution of ice velocity particularly during wintertime at non-surging surge-type glaciers.

I also think that too much emphasis is being placed on the description of the winter speed and subsequent down glacier propagation of the anomaly. This phenomena does not appear in all the data sets provided, and does not seem to be such a novel finding. Reviewer one has already done a good job of explaining this. Moreover, I would suggest the authors look more deeply into the critical transition period in the fall, and reevaluate accordingly.

As we discussed in the Reply to Referee#1, we consider that our observed winter speed-up signals are qualitatively different from the previously known signals. It is true, however, that not all the glaciers clearly exhibit the winter speed-up signals, and thus we will substantially modify the texts and possibly the figures.

We added more explanations about our data and explained how our datasets were different from previous studies at P2L15-27.

I'm struggling to interpret the RGB composite intensity images. They all seem to be a little too conveniently homogeneous; as if the glacier is either entirely surging or entirely quiescent. Other data presented in the paper seems to be telling a more complex story. The authors should do a better job of explaining the significance of these data, and how the reader is to interpret them.

Our explanations were not helpful enough to interpret the results. The RGB method is used to visualize the temporal changes of surface intensities in a single image. Yasuda and Furuya (RSE, 2013) demonstrated significant changes in the surface backscatter intensities, which they attributed to the emergence of crevasses associated with the glacier surge. We will add the details in the revision.

We described the RGB method at P1L10-24 in the Supplementary material.

The figures are inconsistent, and some present outright puzzles to the viewer. This is especially true of figure 6, which I still can not understand the dates for. Also try and capitalize on other data in the figures. For example, place a colored (red?) border around frames where a surge is occurring. Consider providing similar clues to the reader for frames that contain mini-surges, or other phenomena discussed in the text.

We apologize for the inconvenience. We will change the Figure 6 and its caption so that it will be much clearer.

The original Figure 6 is now renamed as Figure 2, in which we assigned alphabets to each panel (a-z). We also added more explanations in the captions and main text.

The paper should be read aloud, several times, by a native English speaker, and the numerous problems might be corrected.

The revised manuscript will be checked by English editing services again.

This revised manuscript is modified based on an English editing service.

The data remain a potential asset to the community, but a fair amount of work must be done before they can be presented.

Thank you for your evaluation. We are sure these observation data is valuable to better understand glacier dynamics.

Reply To Reviewer #3's comments

The observations are compelling and well established based on conventional methods for features tracking from SAR imagery. The arguments establishing a theory on observed winter-time speed associated with surging behavior is circumstantial but compelling. I think the addition of (if available) time series of near surface air temperatures would be a solid addition to substantiate claims that winter-and fall conditions were not conducive for the production of surface melt (its likely they are correct), but an examination of both summer and winter air temps commensurate with the velocity measurement periods from 2007-2011 would be illuminating.

We have checked the average air temperature at Yakutat Airport on November, December, January, and February from 2006 to 2011 using The Alaska Climate Research Center data (http://akclimate.org). The monthly average temperature on November is about 0.2℃, and December, January, February is about -2℃. Almost all of our study area is located at the elevation above 1000 m a.s.l. except the Agassiz Glacier that is located from 450 to 1100 m a.s.l. Thus, the winter-time temperature is significantly lower than the freezing point.

We inserted a new paragraph in the beginning of the Discussion.

-Though understanding subglacial erosion rates is certainly a worthwhile endeavor, I am amiss at what the authors reattempting to accomplish in the paper. Is the overarching justification for this study to better understand mechanisms of surging behavior in glaciers with implications for surge-behavior on mass balance and mass contribution to sea level or is to better understand how surge behavior impacts basal erosion rates/till production and redistribution? Perhaps the authors are attempting to state how understanding surging behavior will have broader ramifications for understanding mass balance and sea level contributions in addition to improved understanding of such behavior on glacial geomorphology. If so, than the Introduction section should be restructured to make this clear. It currently does not read in such a way as to clearly articulate the significance of the research effort

As pointed out, understanding surge mechanism has broader ramifications, such as mass balance, basal erosion, and sea level contributions, and that's why we mentioned. We will concentrate more on the mechanisms of glaciers surging, and thus will re-write the Introduction section to make this point clear.

We re-wrote the introduction, focusing on the seasonal velocity evolution of non-surging surge type glaciers and deleting the original texts that were related to erosion.

-might be worth adding either an Objectives section or to clearly state the research goals of the paper in a paragraph at the end of the Introduction section.

Thank you. We will clarify the objectives of our study and add the sentences in the Introduction.

We wrote this part at P3L3-20 in the end and the previous paragraph of the Introduction.

- I would recommend adding a Study Region Section in which I would provide details about the glaciers examined in the study, the glaciological context and historic behavior which is included in the Results section. A lot of that material should not be in that section. Section 3.2 looks like the beginning of such a section and the other sections (3.1.1, 3.1.2) should be sub-section sf the Study Regions Section

Thank you for your suggestions. We will reorganize the sections in the revised manuscript.

Although we did not add a study region section, we added three new paragraphs in the Introduction, in which we briefly mentioned previous studies about glacier surge in the St. Elias Mountains at P2L28-P3L2. Moreover, we wrote the significances of studying surge-type glaciers at P3L13-20, in the end of the Introduction.

-page 2613, "..the dynamics of basal water.." what is meant here? Should be stated with greater clarity

We meant that basal hydrology remains poorly observed, which hampers our understanding the short-term glacier dynamics. We will clarify the sentences, focusing more on the dynamics of surge-type glaciers.

We deleted the sentence. The introduction is re-written focusing more on the dynamics of non-surging phase of surge-type glaciers.

-the Data and analysis section reads very poorly. The section should state early that measurements of surface velocity were assessed as derived from PALSAR data, then continue with detailed description of the methods to process the data as generally included.

Thank you. We will first describe the detail of the ALOS/PALSAR data, followed by the details of the method for data processing.

We added a new paragraph about ALOS/PALSAR at P3L24-P4L2 in the section of

Data sets and analysis method.

-Should articulate why the analysis period was selected (December 2006-March 2011) in the Data/Analysis section.

The ALOS satellite was launched on January 2006 and terminated on May 2011. Thus, the data sets for the study area were acquired only from December 2006 to March 2011.

We explained it at P3L27-29.

-Define YGS before using in a sentence.

Thank you. We will add the definition of YGS.

We added the definition of YGS at P2L4 in the supplementary material.

-RGB method as cited by (Yasuda and Furuya, 2013) should not only be defined (the acronym) but a summary of this methods should be detailed in the methods section, not mentioned in passing in the Results section. There is not mention of this method, what it is used for, how it is derived, and how it provides something important in accomplishing the intended research goals.

All right. The RGB method is used to visualize in a single image the temporal changes of surface intensities. Yasuda and Furuya (RSE, 2013) demonstrated significant changes in the surface backscatter intensities, which they are attributed to the emergence of crevasses associated with the glacier surge. We will add the details in the revision.

We added the detailed explanations on the RGB method at P1L10-24 in the supplementary material.

-not sure why you cited this statement, "..Moreover, in contrast to the upglacier propagation of summer speed-up (Zwally et al., 2002; Sundal et al., 2011; MacGregor etal., 2005), the higher-velocity region was observed to expand from upstream in fall to downstream in winter: : :" in the results section. I assume you are reporting results as findings from your analysis, not from the analysis of others. This kind of reference should not be made in the Results section of your paper. If your intent is to contextualize your results based on previous work than this kind of reference should be more clearly articulated in the Discussion section.

We cited the observation result at the western margin of Greenland, because it was markedly different from ours. We wished to demonstrate the contrast between the flow evolution in our study and that in Greenland.

-again, I'm confused by such a reference, ".. Although we could not obtain quality summer velocity data for each year (Burgess et al., 2013),..". Why are you referencing other work when discussing your data and your analysis?

Burgess et al. (Nat. Comm., 2013) reported the flow velocity map in Alaska/Yukon using the same ALOS/PALSAR data. They also mentioned that the summer data quality is low due to low coherence, with which we agree and that's why we cited. However, as pointed out, the citation should not be done in this section.

We deleted the citation.

-needs to be written clearly: : :its ambiguous what you are trying to say I this statement found in the2 Paragraph in the Discussion section, ": : :However, downglacier propagation of the winter speed-up will require such an efficient drainage system in the upstream that is usually found in the downstream closer to the terminus (Raymond et al., 1995): : :"

What we wished to say is the following. If our observed winter speed-up could be explained by the same mechanisms proposed by Kamb et al. (Science, 1985) at the Variegated Glacier, there has to be an efficient drainage system. As noted in Raymond et al. (1995), however, such an efficient drainage system is often observed near the terminus. The winter speed-up is, however, observed in the upstream. Thus, the surge initiation mechanism by Kamb et al. may be inadequate to interpret our observations.

We re-wrote the texts at P8L16-23.

-page 2619, paragraph 2: ": : :Using the few ERS1/2 tandem radar interferometry data with the 1–3 day's observation interval, Lingle and Fatland <ADD CITATION DATE HERE>: : :" and then remove the citation at the end of the sentence.

Thank you. We will correct it.

We re-wrote the sentence at P8L24.

-page 2619, paragraph 2: "Moreover, the detected bull's eye-like: : :" likely a better way to identify these features than use of such a colloquial statement: : :

Lingle and Fatland (Ann. Glacio., 2003) used this expression, but also expressed it as "circular motion anomalies". We will reconsider the wording in the revison.

We decided to use "circular motion anomalies" at P8L27.

-page 2619, paragraph 3: sentence is ambiguous, "We consider that our velocity TYPO<measuments>are complementary to the limited observations and revitalize the englacial water storage hypothesis: : :" revise.

There have been very few winter speed-up observations except those by Lingle and Fatland (Ann. Glaciol., 2003), which seem to have limited the applicability of englacial water storage mechanism. The velocity data in the present study, however, demonstrate that the winter speed-up is never a rare phenomenon. That is why we consider that our velocity data are complementary to the Lingle and Fatland's data, and may support the englacial water storage mechanism.

We added more explanation at P9L1-9.

-page 2619, paragraph 3: "vertical glacier surface motions"..what are you referring to here: : :did Lingle and Fatland (2003) measure verticle (i.e. vertical gradients in horizontal velocity), you're not referring to vertical displacement along the z-axis. This is confusing: : :IN fact this whole paragraph is confusing. I' not sure what "verticle motion" you are referring to as your measurements only are able to resolve btoh horizontal component of surface velocity and you did not present any evidence of such data beyond the resultant velocity magnitude (not even direction vectors are shown in figures). So I am confused by what you mean by vertical motion.

Okay, we agree with you, and the confusion is caused by the vague meaning of the term "vertical motion". We will not use the term in the revision. We understand that the signals shown by Lingle and Fatland are local uplifting and/or subsidence, which are probably caused by transient subglacial hydrological processes. In contrast, what we have shown in the velocity data are mostly due to the horizontal displacements because the observation interval, 46 days, is much longer than the case in Lingle and Fatland. However, we interpret that both Lingle/Fatland and our observations are caused by the same physical processes. This is because the locally increased basal water pressure will enhance basal sliding and contribute to larger horizontal displacements. Although our data may also include the effects of local uplifting and subsidence, we consider that such contributions will be much smaller than the horizontal displacements.

We replaced "vertical motions" with uplifting and/or subsidence at P8L24-P9L9.

-Van der Veen, 1998 paper described criteria for formation and propagation of air and water-filled crevasses that form at the surface of a glacier. This is a different mechanism than the formation and maintenance of basal crevasses.

There are two papers written by Van der Veen published on 1998; the Referee #3 seems to be confusing the two papers. One of those is the referred paper, whose title is "Fracture mechanics approach to penetration of *surface* crevasses on glaciers". The other is "Fracture mechanics approach to penetration of *bottom* crevasses on glaciers". While these papers have been published on the same journal (Cold Regions Science and Technology), we are referring to the latter paper.

-The last part of the discussion section (end of last paragraph) is rather speculative. The inference of a concave basal topography may be reasonable but is unsupported by evidence. The additional speculation of high geothermal heat fluxes without a knowledge of what the actual flux rates are is also quite speculative.

We have decided to focus more on the dynamics of surge-type glacier rather than the process of glacial erosion. Thus, those speculative discussions will be significantly reduced or could be deleted in the revision.

We removed some paragraphs about the glacial erosion and the formation of an overdeepened concave basal topography, and focused on the winter speed-up mechanism comparing the surge initiation mechanism proposed by Kamb et al. (1985). However, we mentioned the presence of basal crevasses that keep the large amount of summer meltwater at P9L24-P10L2.

-It appears that the major argument in hits paper is that observed spatial and temporal variability in winter-time velocity for known surging glaciers is anecdotally explained by references to other work that has explained surging behavior through the injection of stored melt water in bottom crevasses. The argument purported here is circumstantial, yet compelling. An additional way to assist in establishing the foundation of the argument that is specific to the glaciers under analysis in this study would be for the authors to consider using the linear elastic fracture mechanics (LEFM) approach to determine the spatial distribution of conditions necessary for bottom crevasse to form as indicated in the works of van der Veen (1998a) and Nath and Vaughn, 2003. I'm not suggesting this as a necessary condition for publication as it might require a substantial amount of work but if feasible it might strengthen the argument established in this paper.

Thank you for the constructive comments. We will consider it for our future work.

I think the paper presents interesting findings. The argument to explain observations is anecdotal but plausible. The paper suffers from serious organizational and language problems which would require attention before the manuscript would be acceptable for publication. I recommend revising and resubmitting after major issues have been addressed.

Thank you again. We will substantially change the organization of this paper and correct the language problems to deliver our findings.

We substantially changed the organization of this paper and corrected the language problems.

Best regards,

Takahiro Abe and Masato Furuya