

Dear Referee #3

Thank you so much for your time in carefully reviewing our manuscript. We believe the comments will greatly help revise our original manuscript.

*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°C, and December, January, February is about -2°C. 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.

*-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.

*-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.

*- 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-sections of the Study Regions Section*

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

*-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.

*-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.

*-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.

*-Define YGS before using in a sentence.*

Thank you. We will add the definition of YGS.

*-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.

*-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 et al., 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.*

Thank you for understanding our intent. We will move this part to the discussion.

*-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 mentioned that the summer data quality is low due to low coherence, with which we agree with it. However, as pointed out, the citation should not be done in this section.

*-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-write the texts.

*-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.

*-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 revision.

*-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.

*-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.

*-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.

*-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.

Best regards,

Takahiro Abe and Masato Furuya