Dear Anonymous Referee #2

Thank you so much for your time in carefully reviewing our manuscript. This is our response to your comments. The blue sentences in italics indicate *your comments*.

General Comments The paper by Abe et al. provides a description of the last three surges of Donjek Glacier, which have occurred at twelve-year intervals. Donjek is a well-studied glacier and, like Referee #1, I was surprised the authors did not make much use of previous literature. For example, Johnson (1972, Arctic) describes how the history of the glacier has been well-documented photographically since 1935. While such records may not provide the same velocity data that Abe et al have relied on in the current study, they would certainly provide some better long-term constraints on surge cyclicity. Further, Clarke and Holdsworth (2002, USGS Publication) report about surges in 1974 and 1978. The authors comment on the 1974 surge but, oddly, make no mention of the 1978 one, which would actually lend evidence to the 12-year periodicity argument. Previously reported surges in 1935, 1961 and 1969 however, Discussion Paper don't necessarily support the 12-year periodicity unless it has evolved over the past century (which is no doubt possible!).

Thank you for the valuable comments. We will refer to the previous studies in the revision. However, as we state in the Reply to Referee#1, we cannot consider whether the reported "surges" before the 1960s were truly equivalent to those we report here or like mini-surges, or just advances without speed-up, because the data sources are entirely different from ours in terms of both quality and quantity; no detailed information on the previous observations were available. Although Johnson (1972, Arctic) notes that the history of glacier from 1935 to the present is well-documented, his statement is based on a personal communication with Wood and Post; no documents are accessible at present. Moreover, we had mentioned the first fluctuation of terminus area corresponds to the 1978 surge reported in Clarke and Holdsworth (2002) at P5948, L17-20 of the manuscript, but it seems the explanation was unclear. Thus, we will re-write some sentences to make it clear.

On the issue of citations, I was surprised that the authors didn't cite more of the classic papers on surge behavior (as described by Referee #1). The authors make substantial inferences based on the apparent 12-year cyclicity but I note that their 1989 surge is based on a single datapoint, and the only datapoint prior to that is from 1986. I was also some surprised that the authors did not provide a list of the 64 images they used.

This is also due to the limited number of references, which is a rule of *Brief Communication*. However, we will re-write the introduction, adding the references on the previous papers on the surge at Donjek Glacier.

Regarding the 1989 surge, we added some velocity data in 1987, 1988, and 1990. Now we can unarguably claim that the surging occurred in 1989. Moreover, we will add the list of the image pairs in the supplementary.

I agree with many of the comments provided by Referee #1 and have tried to avoid duplicating them here. However, I want to point out that Referee #1 seems to be frustrated by the very broad statements made by the current authors, for example about waxing and waning of the terminus area, and links to 'global warming'. I absolutely agree, in that I got the feeling that the authors had made some interesting observations, but failed to put as much effort into coming up with plausible and well-thought out explanations for them. I have not pointed out small typographical or technical errors, but have made comments on passages that I found confusing. I provide below line-by-line suggestions and comments.

Thank you for the detailed comments, which would be helpful to make the manuscript much clearer.

Specific Comments P5944 L6 – On the regularity of the surge cycle at Donjek Glacier, have the authors read the literature about the velocity fluctuations at Black Rapids Glacier? In the upper parts of that glacier, 50% velocity oscillations have been observed on timescales of 12 years (see for example Heinrichs et al 1998; Nolan 2003, Annals of Glaciol.; USGS Open File; Truffer et al 2005, J. Glaciol.; Shugar et al 2012, JGR).

Thank you for your information. We have known about the oscillations documented by Truffer et al. (2005), and we have read some papers you suggested. However, they are not showing the active surging phase. Instead, Nolan (2003) shows that this oscillation

would be the manifestation of slowly propagating waves of till failure and till healing during the quiescent phase. The amplitude of the oscillations is quite different from that in our data.

P5945 L3 – I am not clear what 'acquisition time' refers to in this context. Do the authors mean seasonality?

What we mean here is SAR data acquisition can be done both daytime and nighttime.

P5945 L11 – Donjek River 'Valley System' is not a proper noun and so should not be upper case. Further, the Donjek Glacier is in southwest Yukon, not northern Yukon.

We are a bit puzzled with the comment, because Clarke and Holdsworth (2002) wrote "Donjek River Valley System". We will change "northern" to "southwest".

P5946 L16 – The sentence 'We calculated the terminus area changes. . .' is somewhat confusing.

We will re-write it as below.

"We calculated changes in area of the terminus lobe using a fixed reference line placed about 5 km from the terminus."

P5946 L24 – Why is the colorbar shown in linear scale on one panel but logarithmic on the other?

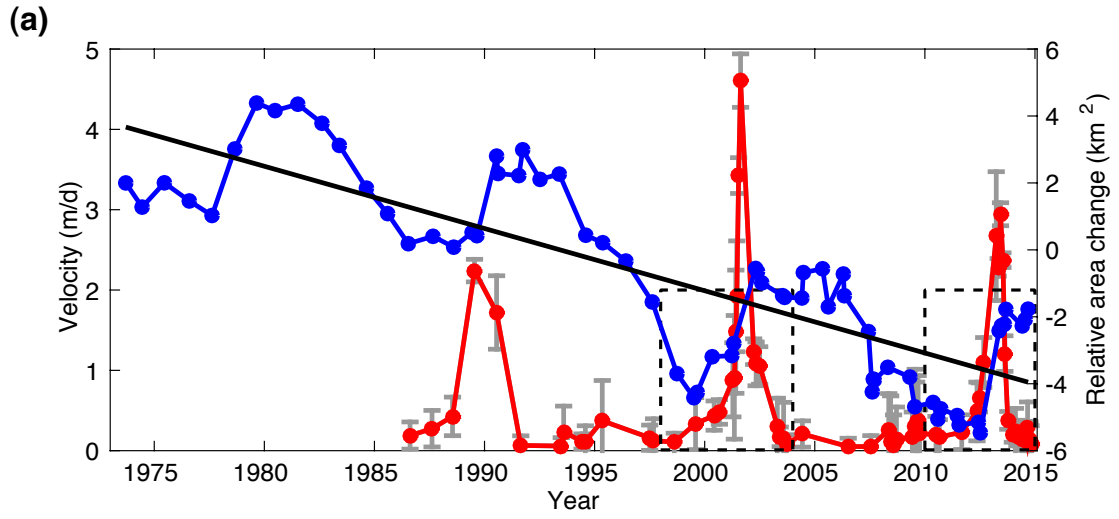
We considered that linear scale is clearer to show that the surging area is limited to the 20-km section from the terminus. On the contrary, logarithmic scale is clearer to show that upstream speed around 30-km section is faster than lower part in the quiescent phase and nearly constant over time. Thus, we use the linear scale for showing an example speed map associated with the 2001 surging episode and the logarithmic scale for the temporal evolutions. However, as also pointed out by Referee#1, we will set the scale as logarithmic.

P5947 L9 – Do the authors mean that there is a distinct slowdown in the 5 years prior to the surges/speedups they observe? This is interesting but there does not appear to be any effort to explain this velocity fluctuation.

No. What we would like to tell here is the velocity front (about ~0.5 m/d) propagated to the terminus prior to the surging. As also pointed out by Referee#1, we will add some sentences to make it clearer.

P5947 L17 – The authors describe the peaks in the red lines (Fig 2a) as representing surges. While I don't dispute this at all, I am left underwhelmed by the evidence for the 1989 surge, which is entirely composed of a single datapoint. Indeed, the blue line (changes in terminus area) pattern would suggest a surge, but the more direct evidence is not particularly convincing. Perhaps the authors ought to make more out of the blue line than they do.

As we mentioned above, we added some data in 1987, 1988, and 1990 (new figure is below). Now we can unarguably claim that the surging occurred in 1989. We do never claim, however, that the speed of 1989 surge is lower than 2001 and 2013, because there are fewer data points in late 1980s to early 1990s.



P5947 L22 – As mentioned above (and by Referee #1) the authors do not provide sufficient information about the scenes they used. Here, they describe a gradual increase in velocity from 'late 1998-1999' but provide no specifics.

We will add the detailed list of Landsat images and some sentences.

P5947 L29 – As above, the authors provide no specifics about the 64 images used.

We consider that it is not necessary because they are basically the same as those used in the velocity mapping.

P5948 L2 – Surely the authors can do better than simply presuming that the change in glacier terminus area is related to global warming. Or at the very least, cite some other papers from nearly glaciers (there are many to choose from) that back those claims up with data.

OK, we will cite some papers and re-write the sentences.

P5948 L6 – The authors state that there are 'a few time lags'. Can they be a little more specific?

We have considered the significant terminus advances occurred a bit after the speed-up, but we cannot quantify the time due to the coarse temporal resolution.

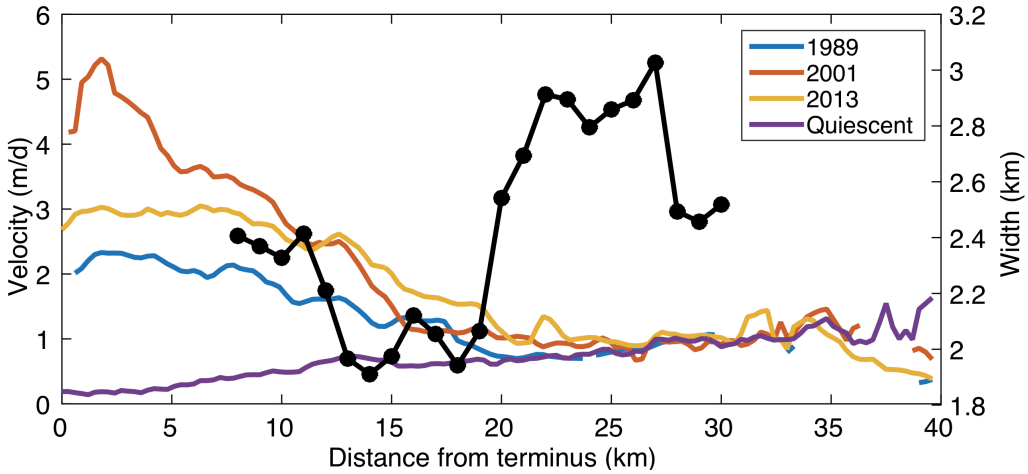
P5948 L8 – The authors state that they cannot derive the glacier speed prior to 1985. Why not? I conducted a 5-minute search on the Canadian Government's NEODF airphoto and satellite imagery search engine and found stereo imagery going back to the late 1940s (incl 1940s, 50s, 70s and 80s). While not the same resolution or timestep as the satellite imagery, these could certainly be used to augment the time series presented by the authors.

This is due to the low spatial resolution of Landsat 1-3 MSS data, which we have already mentioned in the paper. Although it may be useful to just mention the past surging episodes, the low and coarse resolution images do not augment the "time series" as equally as we have done in the manuscript. The aim of this paper is to inform that the recent three surging episodes occurred at Donjek Glacier with 12-year constant cycle and that they initiated in the narrower valley zone.

Also, thank you for letting us know the NEODF search engine. We tried it, but it turned out that there are only several images could cover the downstream of Donjek Glacier; the temporal resolution was inhomogeneous and coarse.

P5948 L13 - The authors describe the constriction in the glacier width here and elsewhere as being 'at least 35% narrower' [Italics are reviewer's emphasis]. I don't buy this argu- ment. A quick measurement in Google Earth shows it to be at most <30% narrower than upstream, and certainly not for the lower 20km (or whatever ~20km section the authors describe, which is unclear). In fact, there is a relatively short section of <8km that is <30% narrower than upstream, but the glacier then widens again. And in fact, is narrower farther upglacier too. Instead of a local constriction as the authors describe, I would suggest it is perhaps a longitudinally fluctuating width. I would like to see a plot of width vs length here.

We derived one figure showing a plot of width v.s. length in black below calculating every 1 km between 8 and 30 km from the terminus. The velocity profiles associated with the three surging and quiescent averaged between 2003 and 2011 are also shown. As you can see, the section between 18 and 22 km is significantly narrower than upstream, which is about 33% ('At least 35 % narrower' may be a little bit large…). Moreover, the section corresponds to the area where the surging episodes initiated.



P5948 L24 – The authors pin a lot on three surges, here saying: '. . .the recurrent [sic] interval seems to be fairly regular with few variabilities.' With their 1989 surge resting on a single datapoint, I find this statement to be a little too strong. Further, previously published reports of surges in 1961, 1969, 1974 (though a tributary only) and 1978 absolutely must be discussed.

As we mentioned above, we added some velocity data in 1987, 1988, and 1990. Now we can confidently claim that the surging occurred in 1989. Although we have mentioned the surging in 1974 and 1978, we have not stated the surges in 1961, and 1969. As we also discussed in the Reply to Referee#1, we will refer to Johnson (1972) and mention the surge in 1960s.

P5949 L8 – The authors again describe the constriction, and suggest that it 'may generate a steep surface slope around the narrowing zone'. This would be incredibly easy to determine in a GIS, yet they have not done it. A figure combining a plot of width vs length and elevation gradient would add some weight to their arguments, in my opinion.

We derived the one figure showing a plot of width v.s. length, as attached above. We could not derive the temporal changes in elevation gradient, because a series of DEMs are not available.

P5949 L12 – The sentences describing the findings of Eisen et al (2001) read a little bit like an undergraduate textbook.

We will modify the sentences as below.

"Eisen et al (2001) attributed the variability in the recurrence intervals to the variable annual mass balance".

P5950 L1 – The first paragraph on this page (starts on 5949) is rather confusing. It jumps around in space and theme, and is as a result hard to follow. I suggest a rewrite.

OK. We will re-write the paragraph to make it clearer to follow.

P5950 L13 – The authors speculate (their choice of words, L11) that ice locally thickens during quiescence, but they provide no evidence to back this claim up. Analysis of a series of DEMs would allow the authors to state with some confidence, rather than speculate, about ice thickness changes.

It is absolutely true that a series of DEMs would back up the claim, but we have no publicly available data to examine the elevation changes.

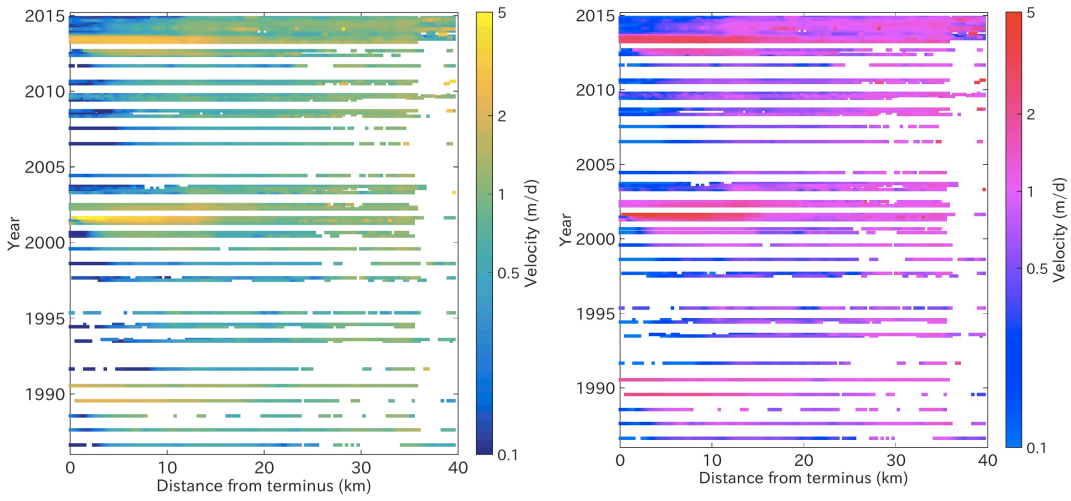
P5951 L2 – The authors 'propose to perform detailed observations of not only velocities but also geometric and hydrological changes for the next event' in or around 2025. While this is a nice goal, it reads as if for a grant proposal, not the closing sentence of a peer-reviewed paper.

We will re-write the sentence as below.

"The detailed observations of not only velocities but also geometric and hydrological changes are needed for the next event in order to reveal the generation mechanisms."

Figure 1 – I really do like the way the authors have portrayed the velocity time series in panel c. Except I don't understand why the color scale changes with respect to panel b. These should be the same. Also, there has been much online in recent years about how the 'jet' colorscale should be avoided because it draws the viewer's eye to things that are not necessarily 'real'. Have the authors tried plotting with a different colorbar to see what it looks like?

As pointed out by the Referee#1, we will set the color scale as logarithmic. Moreover, we generated two figures with different colorbars as below. What do you think? We consider that these colorbars are unclear to show the speed changes, and "jet" would be the best to plot.



Best regards,

Takahiro Abe, Masato Furuya, and Daiki Sakakibara