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Interactive comment on "Brief Communication: Twelve-year cyclic surging episode at Donjek Glacier in Yukon, Canada" *by* T. Abe et al.

Anonymous Referee #2

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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,

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don't necessarily support the 12-year periodicity unless it has evolved over the past century (which is no doubt possible!).

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.

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.

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). P5945 L3 – I am not clear what 'acquisition time' refers to in this context. Do the authors mean seasonality? 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. P5946 L16 – The sentence 'We calculated the terminus area changes. . .' is somewhat confusing. P5946 L24 – Why is the colorbar shown in

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linear scale on one panel but logarithmic on the other? 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. 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. 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. P5947 L29 – As above, the authors provide no specifics about the 64 images used. 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. P5948 L6 – The authors state that there are 'a few time lags'. Can they be a little more specific? 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. 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 argument. 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,

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I would suggest it is perhaps a longitudinally fluctuating width. I would like to see a plot of width vs length here. 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. 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. P5949 L12 – The sentences describing the findings of Eisen et al (2001) read a little bit like an undergraduate textbook. 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 re-write. 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. 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. 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?

Interactive comment on The Cryosphere Discuss., 9, 5943, 2015.