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Interactive comment on "A conceptual model of cyclical glacier flow in overdeepenings" *by* J. B. Turrin and R. R. Forster

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General comments

This manuscript presents velocity time series from several Alaskan glaciers using feature tracking methods. The velocity of these glaciers is shown to 'pulse' over periods of several years and a conceptual model is presented to explain this, based on till deformation and water flux through overdeepened regions of the bed. The satellite-derived dataset is certainly interesting and it is useful to discuss causes of dynamic behavior not necessarily linked to climatic changes. However, the authors jump somewhat beyond the means of their data and so I would suggest they take more of a speculative rather than conclusive tone regarding their conceptual model. I also have various concerns that I list below:

1. I find the conceptual model proposed in this manuscript to be interesting; one which

warrants further investigation in overdeepened regions. However, a problem with the manuscript is that theories such as till deformation in overdeepenings, pulsing behavior, causes of surging and overdeepening hydrology are stated as if they are widely accepted and fully understood, whereas there are, in fact, multiple theories for these processes. The manuscript should therefore be rephrased to take account of this. I have raised various examples of this within the specific comments below but the introduction should also be rephrased accordingly.

The conceptual model is also presented as if the data fully prove the model. However, the lack of bed data to demonstrate overdeepenings, lack of surface elevation change data and the lack of in situ data to show the presence and deformation of till mean that this model should be presented as a hypothesis rather than conclusive findings. Additionally, emphasis should be placed on future work to collect data to test this model.

Another examples of overstating a hypothesis is when you say that overdeepenings have similar characteristics to where pulses have been identified (flat surface slopes etc.), so therefore pulses are a result of overdeepenings (Pg. 4472, lines 24-25). To argue this, much more information on basal characteristics would be needed at these sites, and therefore the language should be toned down so it represents a hypothesis rather than a 'fact'. Also, not all overdeepenings show pulsing behavior. If your conceptual model is correct there must be some reason why it occurs in some regions and not others – this should be discussed. Similarly, not all pulsing behavior is related to overdeepenings. In Turin et al (2014) it is mentioned that Trapridge experienced pulses during surging as did Black Rapids glacier. In addition there is the alternative theory posed in Turin et al (2014) that pulses are due to a temperate till layer with low permeability just above a threshold value. These points should be mentioned in the current manuscript.

2. It is not clear where the pulse is initiating as it is a) not obvious from your data that velocity changes are related to overdeepened regions and, b) as you state, the ice velocity appears to increase for most of the glacier trunk simultaneously. This is

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important when you are discussing causal factors for pulse initiation. It is also not clear from the figures that the region you have picked to show velocity change or where the bedrock lithology changes is necessarily where the pulse initiated. For example, for Eldridge glacier at 35 km, the velocity profile that is picked does show the pulsing behavior described. But up-glacier of this at around 20km it jumps about all over the place (year-by-year it seems). This behavior therefore requires more discussion and perhaps justification of why the velocity profiles were picked at those specific locations. In particular, why not look at velocity where the lithology changes (where the grey vertical bars are) since this is supposed to be a controlling factor?

3. In other areas of the manuscript you jump beyond the bounds of the data. For example, on page 4470, line 25 you claimed that the fastest velocities and pulsing behavior occur where the surface slope is in the range of 1-2 degrees. This is misleading because the entire region that you have velocity measurements for is generally within the 1-2 degree surface slope range, so because you have no data outside of this range it is difficult to argue any control. The exception to this is the Copper and Sanford records but at these glaciers the 'shallow slope' control does not seem to hold. You mention later that you have data from other glaciers that don't show pulsing behavior. Perhaps the better way to look at this would be to see whether these other glaciers are generally steeper along their trunks. If this is the case a table or figure should be included to illustrate the surface slope difference between pulsing and non-pulsing glaciers.

4. Because a discussion on the results for all the glaciers is lacking within the manuscript, your statements are sometimes misleading. For example (Pg 4470 Line 9), the velocity peak of Nizina is argued to occur in the region of the bedrock lithology transition, which is presented as a likely causal factor for the velocity maximum. However, the remainder of the data sets (aside from Capp glacier) do not show this. Similarly it is mentioned that the increase in surface slope at Nizina Glacier coincides with the velocity maximum; this is not the case for any of your other data sets. The main results from these other glaciers should therefore also be presented and contrasts in

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behavior discussed.

5. The conceptual model suggests that a major control on water flow out of the overdeepening is the surface slope. Although you plot the surface slope from one time slice along the flowline this will change over time. Do you have data that can back up your theory of steepening and thinning ice during the pulses over the overdeepenings? I think this would greatly strengthen your argument and your conceptual model. When were the surface elevation profiles plotted from? It would be helpful to note this in the manuscript or figure captions.

Specific comments:

Pg. 4464, Lines 8 and 22. Remove 'may' since you know this from your data.

Pg. 4464, Line 13. I suggest you rephrase this to "and where the surface slopes are very shallow, 1-2o,. . ."

Pg. 4464, Line 15. It is not immediately clear why lateral constriction and shallow surface slopes mean there are overdeepenings. This should be clarified.

Pg. 4464, Line 23. It seems a stretch to define that all pulses are due to deformation of subglacial till as there is little direct evidence of this. This is also important when saying that surging glacier accelerations are due to till deformation (pg 4465, line 1) which is not universally recognized.

Pg. 4465, Lines 4-16. This theory of pulsing is presented as a widely accepted and universal mechanism. This should be rephrased so that it is clear this is a hypothesis and little direct (in situ) evidence is available to support it. I don't see a problem using this theory as a basis for the rest of the paper but it should be made clear this is just one possibility for pulsing. For example in Turin et al (2014) it was also mentioned glacial lake drainage could cause pulsing.

Pg. 4465, Lines 23-27. Again, it is too strong to say surges happen because of reorganization of the basal drainage system. This is not universally accepted and contradicts

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what you say above about till deformation causing surging (line 1).

Pg. 4467, Line 9. I suggest you specify early on that '0 km' is from the glacier head so that phrases like "between 5-10km" are clear.

Pg. 4467, Line 16. How do you know the Rohn ice completely ablates by 28km? I would think that this is unlikely.

Pg. 4467, Line 18. Missing word.

Pg. 4470, Line 7. It would be good to point out where the glacier convergence occurs in Figure 5 with another vertical line (and the same for glaciers that have a tributary in the supplementary data).

Pg. 4470, Line 20. Some plots or a table of the outputs from the nine glaciers should be included in the main manuscript rather than just the supplementary material if you are using their characteristics to build your conceptual model.

Pg. 4471, Line 1. As you mention on page 4470, line 12, this behavior with steeper surface slopes coinciding with maximum velocity also occurs for Nizina glacier. Since it is unclear when these surface profiles (in what stage of the pulse) were measured it is not really possible to use the static measurement of surface slope as a driving characteristic for pulsing since, according to your conceptual model, steepening and shallowing of the surface slope is an important aspect for the whole pulsing cycle.

Pg. 4471, Lines 3-7. I am confused here. The changes in bedrock lithology marked on your figures don't seem to correspond well with where the pulse occurs, as is stated in the text.

Pg. 4472, Line 6-20. It should be noted that this is one theory for overdeepening formation but is not universal, as discussed in detail in Cook and Swift (2012).

Pg. 4473, Line 17. The basal hydrology system for overdeepened regions is not universally known to be distributed. I suggest you rephrase this to say ". . .beneath overdeep-

enings has been previously shown to be in a distributed. . .". Similarly on line 24 it should say ". . .is likely a result of constriction. . ."

Pg. 4474, Line 7. I suggest you remove "or not".

Pg. 4474, Lines 9-15. These supercooling thresholds are based on water flowing at overburden pressure. If water is at lower pressure it can freeze on shallower slopes, at higher pressure it can flow up steeper slopes as shown in Dow et al (2014). This could be an important process for changing the drainage characteristics and therefore till deformation rates within the overdeepenings. Similarly, it would be good to discuss the possibility of drainage reorganization i.e. high pressure water in the overdeepening may drive most water flux around the overdeepening margin preventing evacuation of sediment.

Pg. 4474, Lines 17-20. Where are these slopes calculated for? Which surface slope are these based on e.g. before rather than during pulsing (since the surface slope will change as the velocity changes)?

Pg. 4475, Lines 2-24. It should be noted that this is a hypothesis rather than proven mechanism. I would have thought the supercooling freeze-on restricting the flux of water would prevent till evacuation rather than changing the flux speed by increasing the hydraulic gradient. Why will the till accumulate until the rate of transport equals rate of erosion?

Pg. 4475, Line 15. More explanation of till deformation is needed here (with accompanying references). Till does not all deform when water pressure reaches overburden. Instead it depends on the cohesion and strength of the till along with the water pressure. This is another reason why your conceptual model is not universal since it would strongly depend on the type and strength of till; this should be discussed.

Pg. 4477, Line 8. Why is it distributed in the overdeepening and channelized on the adverse slope? I would have expected if channels were formed on the adverse slope

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there would also be channels within the overdeepening.

Pg. 4480, Line 12. Remove “as one progresses” for clarity.

Pg. 4480, Line 22. Or if they don’t have any till?

Pg. 4483, Lines 4-6. Rephrase for clarity.

Pg. 4483, Line 17. Replace “says” with “suggests”

Figure 1: This would be clearer if you zoomed into the regions so we could see where the glaciers are located

Figure 6 caption: “. . .physical mechanisms which might cause. . .”

Interactive comment on The Cryosphere Discuss., 8, 4463, 2014.