Reply on RC2

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Author comment on "Episodic sediment supply to alluvial fans: implications for fan incision and morphometry" by Anya S. Leenman and Brett C. Eaton, EGUsphere, https://doi.org/10.5194/egusphere-2022-267-AC2, 2022

Thank you Dr Piliouras for your helpful suggestions and feedback. We have copied your comments here for reference (in *italic* text) and respond to each point individually.

Comments:

The introduction would benefit from a brief discussion of what we know about adjustment timescales relative to event timescales in alluvial fans. I know this is touched on in the discussion, but this is a classic concept in geomorphology and is worth highlighting. You are looking to determine if changes in sediment supply over time will have any impact on the system, so this will clearly be related to the timescale of the change relative to the timescale of system adjustment or an avulsion timescale. This would set you up to discuss your results in the context of timescales later on, which I also suggest placing more emphasis on in the discussion, as it would be helpful to explain a lot of your results.

A useful suggestion. Added a paragraph to this end in the introduction (lines 30-40). Have also discussed timescales more explicitly in the discussion (lines 368-372).

Why were there 3 (or 4?) experiments done for Run 1 but only 2 for Runs 7-9? Further, why did you do repeats but then only present results from one experiment? It would be beneficial to show the results averaged from all of the experiments, not just one.

There were 3 exps for Run 1 (CON) and only 1 for Runs 7-9 (OSC); have clarified this (line 127-129). The main reason for this is that I ran out of time to conduct additional repeats due to lab renovations and then Covid-19. Future replicates of these experiments would be useful and interesting and I'd love to run them when I get another fan-simulator up and running. The student currently using the UBC fan simulator is not conducting additional repeats of these experiments as she is taking her PhD in a different direction. I have not added this lengthy explanation to the paper (it seems a little superfluous) but can do so if the editors advise.

R.e. Showing the results from all experiments: have added data from all repeats of Run /OSC1 to the relevant figures (except Figure 10, where doing so would inhibit comparison between runs).

You have selected particular distances at which to measure things in your data analysis (e.g., 0.25 m down-fan for fan head entrenchment). Please provide a justification for

selecting these distances.

Added detail to the methods to explain our choices (lines 161 and 167).

Figure 5, and all figures of a similar nature, would benefit from including text on the figures that lists the mean or median value for each experiment. On Figure 5, for example, we can see the difference in how slope changes over time, but what is the time-averaged slope for Run 1 compared to Runs 7-9? Please include this informataion on each figure. If you're also going to talk about variance, as you do later, put that number on the figure along with the mean/median.

Mean and standard deviation added to all plots where relevant.

Figure 8: What is the physical reasoning for the spike in Fn after sediment supply is turned off? Is it related to excavation of the feeder channel? Some discussion of this would be appreciated, as it is not necessarily expected or intuitive.

It's because the channel is actively reshaping itself from multi-thread to single-thread (often a new, central single-thread channel), so mobility is temporarily very high. We had implied this but have now explained the physical mechanism more clearly (now lines 248-250).

Looking at Figure 5 and Table 1, I suspected that if the channels become more entrenched with the longer periods, those entrenched channels would have less or slower lateral migration. There may also be a larger difference between the channel gradient and fan gradient for fans with longer periods of oscillation (Run 9) compared to those with shorter periods (Run 7). Combining this with Figure 8, by eye, it looks like there is overall faster migration in Run 7 compared to Run 9, possibly due to this entrenchment. What does this tell us? There could be drastically different adjustment times, avulsion times, and overall system dynamics if the channel gradients are very different from fan gradients.

The average migration rate is actually very similar for runs 7 and 8, and slightly lower for Run 9. What really stands out is that the lowest migration rates are right at the end of the zero-supply period. As you say, this acts to enhance the efficiency of down-cutting because flow is concentrated in one place at the fan-head. Have added text to this effect in the discussion of trenching (lines 365-367).

Figure 9: Can these be separated out into separate plots? it's really quite difficult to see anything. Or at least have a row of erosion, a row of deposition, and then a row where they're combined like this

The idea was to show the relative magnitudes of erosion and deposition, particularly at the end of Run 9/OSC40. Nevertheless, I concur that the data are difficult to see (especially the mean deposition, as Dr Clarke also notes). Have remade as two rows of plots, following your suggestion (colours now match previous plots as well). The original plot is still retained in the SM if readers want to compare by eye.

It would be helpful to show fraction area abandoned alongside fraction area newly inundated to observe the change in channel network structure as it reorganizes.

The channel network structure can best be gleaned from Figure 6 (the number of channel threads) and Figure A3 (same plot, farther up-fan). Figure A4 is also useful, as it shows the sector of the fan that is spanned by active flow (implying diverging or channelised flow with higher and lower values, respectively).

Adding the fraction abandoned certainly would help elucidate the timing of mobility peaks.

However, I'm not sure the extra data would be as helpful as that already included in Figures 7 and 8. I don't want to overwhelm the reader with information, but could definitely add the fraction abandoned (perhaps as a separate panel for Figure 8) if the editors advise.

Regarding fan head trenching (discussion lines ~319-326), how do you reconcile the fact that previous studies did get a trench with constant Qs? You have replicates, so is it true across all replicates that you did not get trenching in Runs 1 and 7, only 8 and 9? Later, your explanation for why there is a trench in 8-9 but not 7 doesn't quite get there. You suggest a decrease in the number of channels during 0-S that concentrates flow and erosion in a few channels, leading to downcutting. But you have periods of low sed supply in all runs, so it must be related to some response or adjustment timescale, which in Run 7 is likely longer than the period of oscillation, no?

The lack of trench development in Run 1/CON relates to the highly dynamic nature of the experimental channels – they simply didn't stay in place long enough to incise a trench! In a previous paper we hypothesise that this reflects the generally high sediment concentration and wide GSD in our experiments, both of which facilitated channel sedimentation. Have added a citation where relevant (lines 355-356).

It is true across all repeats that no persistent trench developed for Run 1/CON. See for instance Figure 6, which now contains data for all three repeats of Run 1/CON. There were no additional replicates for Runs 7-9/OSC (this fact has also been clarified now).

Yes, the lack of trench in Run 7 and presence in Runs 8-9 is absolutely related to an adjustment timescale, and to the duration of the zero-supply period relative to that adjustment timescale. This was implicit in lines 327-334 (old version) but as you say, the explanation didn't quite get there. Have added text to fully explain what's going on here, and to reflect more upon what this tells us about disturbance vs adjustment timescales in our experiment (lines 362-368).

Technical comments:

Table 1 should include the standard deviation and error.

Standard deviation added to table; error added to caption.

L151: 7 mm is not sand. Rephrase to just say "approximately the size of the largest grains."

Done.

You note that video speed differs because photos were collected at different intervals. This can be corrected when the videos are made by adjusting the frame rate. Please consider adjusting this so readers can see the difference in relative rates of channel mobility.

This difference only affects the comparison between Run 1/CON and the other runs. We feel that the most interesting comparisons are between videos of the OSC runs, which are all at the same speed. However, we're happy to remake the video for Run 1/CON with a slower frame-rate to match the OSC videos if the editors agree that it would be helpful.

*L163: "for their self-organised adjustment **the** input conditions or changes thereof" change 'the' to 'to'*

Added 'to'.

It would be helpful to remind the reader in the figure captions and/or text if these various metrics you're plotting are at a transect or fan-averaged.

Done.

L314: The experiments don't say anything about amplitude, only period. You have the one prelim experiment in supplemental, but I'd focus the discussion here on duration for what you can actually show.

We've kept this sentence as we felt that the reference to our first low-amplitude experiment was useful to highlight how dampened variations had a lesser effect. This is the only place where we comment on amplitude; the rest of our discussion considers duration.