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Interactive comment on "Alluvial channel response to environmental perturbations: Fill-terrace formation and sediment-signal disruption" *by* Stefanie Tofelde et al.

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Review of Tofelde et al. (2019, E Surf D): Alluvial channel response to environmental perturbations: Fill-terrace formation and sediment-signal disruption

Dear editor,

I had the pleasure to carefully read the interesting new manuscript of Tofelde and colleagues. In it, the authors present a set of seven flume experiments documenting the transient response of an alluvial channel to changes in sediment and water fluxes, and in base level. Their observations illustrate the transient behaviour of transport-limited streams that was previously described in various scattered theoretical, experimental, and field-study articles. It is a useful and timely contribution. However, the text is written in a manner that often suggests that the results of the flume experiments are novel findings while they mostly confirm and support previous work. As such the manuscript is almost a review in disguise. Nevertheless, the experiments led the authors to some valuable novel observations, in particular the lagged response of incision to a drop in input sediment flux. It is particularly valuable in that regard because we precisely lack documentation of transient responses as equations describing fluvial geometry mostly deal with equilibrium conditions. I suggest that the manuscript be accepted after moderate revision.

The figures are of quality and support the text well. The text is also detailed enough such that the reader can grasp all they need to understand the experimental runs.

Below I propose three types of comments to the authors, 1) general comment about fill terraces; 2) a suggestion for the structure of the text; 3) line-by-line comments on science and bibliography.

1. Fill vs. Cut-in-fill terraces: The authors introduce the object “fill-terrace” on page 2 and thereafter it is inferred that all terraces recorded in their flume experiments are such. I would object to this use of the term. A fill terrace, as described on page 2, is a morphologic datum recording the culmination of sediment aggradation immediately preceding a phase of incision and thus abandonment (Howard, 1959; Bull, 1991; Pazzaglia, 2013). In several experimental runs, it seems that the entire active floodplain is being eroded before it narrows its width and starts entrenching, thus abandoning terraces. In that situation, these terraces are not “fill-terraces” but cut-in-fill as they record a moment during the incisional phase and not the culmination of alluvial aggradation. The title of the article needs to be accordingly modified. Then, the difficulty resides in reliably identifying if a given “top” terrace (top as in being the highest from the last incision episode) is indeed a fill terrace. To me it is very interesting that the authors identify cases where barely any fill terraces are abandoned. And that instead two large

cut-in-fill terraces replace the fill terraces one would commonly expect. It appears to capture the moment when vertical incision is promoted over lateral erosion leading to fast autogenic entrenchment of the channel (Malatesta et al., 2017; Bufe et al., 2018) but the two experiments with a drop in Qs suggest that this inflexion point does not always occur at a similar moment. Finally on that point, the rationale behind picking the terraces TA and TB should be fleshed out because at least in the case of the DQsin run, they capture cut-in-fill terraces. More about that with the comment on p. 12 l. 13.

2. Structure of the manuscript

I think that a weakness of the current manuscript structure is that it is difficult to understand what the novel advances are and what the narrative of the work is. That is especially true for readers who are familiar with the existing, extensive, body work on alluvial geometry dating starting with Gilbert and Murphy (1914). The results are presented as if they almost provided a first-time observation of such alluvial dynamics. However, most of the observations from the flume experiments have already been observed, predicted, or discussed in previous bodies of work. What is novel is the documentation of the transient response itself. The manuscript could be somewhat modified to make this clearer and better highlight the contribution of the authors to this larger body of work. In that spirit, I would suggest to move elements of the discussion to the review section “2 Formation of fluvial fill terraces” so as to clearly establish what is acquired knowledge and to underline the gap that the authors want to fill here. In particular, section 2.1 could be augmented with large parts of sections “5.1 Channel response to perturbations and conditions of terrace formation” and “5.3 Differences in terrace surface slope”. By explicitly introducing the theoretical framework used to describe the relationships between alluvial slope and fluxes of sediment and water (Qs and Qw), the authors would build a better launchpad for their study in my opinion. The Meyer-Peter Müller (MPM) equation revised by Wong and Parker (2006) or more recent derivations of slope as a function of Qs and Qw (e.g. by Malatesta and Lamb, 2017 GSAB, or Wickert and Schildgen, 2019) can help establish clearly what is known so far,

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and what is not. The latter being a good understanding of the transient behaviour from one equilibrium configuration to the next. I believe that this modification to the structure of the manuscript would help the reader better navigate the coexistence of the review and experimental aspects of the paper.

3. Science and bibliography comments

p. 1 l. 9-10: This is a pretty strong statement. I would argue that published work provide a pretty good understanding of the impacts of such forcing on terrace formation and sediment dynamics. What is lacking âĂŤ and provided by the authors here âĂŤ is rigorous observations of the transient response.

p. 2 l. 27-30: Malatesta, Prancevic and Avouac (2017, JGR) explicitly target lateral feedbacks with a numerical model.

p. 2 l. 31: Limaye and Lamb (2016, JGR) could also be mentioned here as an example of an excellent bedrock model.

p. 3 l. 8-10: I strongly encourage the authors to have a look at the 2003 Geology paper by Bonnet and Crave. Therein the authors investigate the impact of climatic (Qw) vs. tectonic forcing (base level) on an experimental landscape. While not targeting terraces in particular, it is one of the most insightful papers I've read on the subject. I strongly encourage the authors to read through it and incorporate some thoughts in their work.

p. 3 l. 20: "upstream" [and along stream] (to take into account extra Qs from local incision)

p. 4 l. 3: If incision supplies sediment to Qsin along stream, then Qsin is not the input sediment flux. It might be useful to separate Qsin, Qsc (sediment transport capacity at any point along stream), and Qsout.

p. 5 l. 10-13: I understand and appreciate the distinction here, and it is quite useful to separate the two. But is it a new refined definition? It seemed to me that fill-cut terraces are commonly considered both "complex response" and "autogenic" at the same time

(Schumm's work and Pazzaglia's review paper). If you indeed propose this new,useful, distinction here, I would encourage you to take ownership of it.

p. 5 l. 28: There is a new paper by Johnson and Finnegan that is in revision at Geology on "Tributary Channel Transience Triggered by Bedrock River Meander Cutoffs." I don't know when it will come out. But regardless, it might interest you for the future

p. 6 l. 5: As the reference codes of the experiments are going to be used thereafter, I would suggest to make a reference to Table 1 here.

p. 6 l. 16: what is the vertical resolution?

p. 6 l. 29: It could be helpful to mention that water is tainted blue in the photos.

p. 6 l. 32: why can it be considered unaffected?

p. 7 l. 30: I would argue that change in channel width is not required to form fill terraces. What needs to be reduced is the breadth of the active floodplain (in which the channel, of potentially fixed width, migrates left and right).

p. 9 l. 4: The nature of terraces TA and TB could be mentioned here to simplify the reading of the paragraph.

p.9 l. 21-22: Is there a threshold for what constitutes a pair? Is there a way to define that objectively, or at least in a consistently arbitrary way?

p. 10 l. 6-7: Not sure I understand the rationale behind the ratio of vertical and horizontal erosion. A terrace of width W is preserved for a time T with a river lateral erosion Eh such that T=W/Eh. Preservation is independent from the vertical incision rate. However, deep incision will result in higher walls that are costlier to erode.

p. 10 l. 15-19: Field studies such as Tofelde et al. (2018), Malatesta et al. (2017, Basin Research), or, and especially, Dzurisin (1975). More on the latter below.

p. 11 l. 4-5: a comment only valid if the theoretical framework for alluvial rivers is not beefed up above: I suggest to state that +Qs leads to +S in order to preserve eq. 1 under constant Qw, just as to explain the rationale between Qs and S which is not directly derived from Eq. 1 and 2.

p. 11 l. 8: This dynamic is described and discussed by Malatesta et al. (2017, JGR). It is also worth noting two earlier flume experiments by Schumm et al. [1987, chapter 6] and Meyer et al. [1995] describe the evolution of a channel profile after it reaches a new equilibrium post-incision (see description of that work in section 5.1 in Malatesta et al. 2017, JGR).

p. 12 l. 1: What exactly is the degree of reworking of terrace material? The amount of vertical incision?

p. 12 l. 5: I am a little hung up on paired/unpaired and the threshold it implies. Wouldn't it be more informative to simply write that the terraces are abandoned successively?

p. 12 l. 13: Runs DQsin and IQsin_DQsin both lead to entrenchment when sediment flux drops. So, why does the same forcing cause very different terrace creation, or at least be considered as two different systems? To me, it seems that the different terrace record of the two runs could be explained as reflecting the inherent variability in the abandonment of cut-in-fill terraces. See point about fill terraces written at the begining of the review. It should be however noted that, in the experiment DQsin, there are two slivers of what was probably the original floodplain datum. As such, these slivers should be TA and TB for comparison with IQsin_DQsin.

p. 12 l. 17: this feedback has also been extensively discussed and explored by Malatesta et al. (2017, JGR).

p. 12 l. 19-21: yes, but the two effects mitigate each other. If the incision rate is slow, the later terrace will also not have been lowered that much such that the geometrical difference remains about the same.

p. 13 l. 11-12: The formulation used here suggests that the authors have observed and established ("we found that") this relationship for the first time, along the 2018 Wickert & Schildgen paper. Yet, the fact that terraces have a steeper gradient than the stream's for Qs or Qw forcing is not a new observation or theoretical construct, it is built-in in theory since early fluvial geomorphology work (Mackin, 1948; Meyer-Peter & Müller, 1948; Léopold & Maddock, 1957; Hooke, 1968; Schumm, 1973; Leopold and Bull, 1979; Wells and Harvey, 1987; Harvey et al., 1999; DeLong et al., 2008; Rohais et al., 2012). Recently Malatesta & Lamb (2018) used a derivation of MPM to constrain alluvial slope as an explicit function of Qs and Qw. This passage is one that inspires my earlier suggestion to provide a more complete overview of current knowledge, in particular in terms of theories of transport and geometry.

p. 13 l. 14: I would also point to the absolutely remarkable site of the Gower Gulch alluvial fan in Death Valley. There, a man-made diversion instantaneously changed the hydrology of the catchment leading to sudden incision of the alluvial channel. Details are found in the work of - Troxel, B.W. (1974, ManÂ■made diversion of Furnace Creek Wash, Zabriskie Point, Death Valley, California: California Geology, v. 27, p. 219–223), - Dzurisin (1975,Channel responses to artificial stream capture, Death Valley, California: Geology, v. 3, p. 309–312, doi:10.1130/0091Â■7613(1975)3<309 :CR-TASC>2.0.CO;2.), - Snyder & Kammer (2009), - Malatesta & Lamb (2017). [you will find the two 70's papers on Gower Gulch attached hereby]

p. 13 l.30 - p. 14 l. 9: I am not sure that I follow the argument here. When terrace treads are used to quantify tectonic deformation, the gradient of the terrace does not matter as it is always detrended to retrieve local deformation (e.g. from an anticline, Lavé Avouac, 2000). As long as the tread is straight, tectonic deformation can be well-constrained.

p. 14 l. 12-15: this context could be introduced much earlier in the manuscript to better motivate the study.

p. 15 l. 7: It can be noted that this illustrates predictions of laws like MPM whereby no geometric change at the downstream end of the reach demands that the sediment flux transport capacity does not change either

p. 16 l. 6-7: Wouldn't chemical signals be best transferred during phases of bypass? Or is recycling more important in such phase than during aggradation?

p. 16 l. 18-19: I understand that these are observations from the runs, but I think it would be advisable to add that these "findings" validate existing theories. Though grammatically correct, the word suggests an unwarranted degree of novelty to my ears (non-native english hearing ears, mind you) . That is well known and demonstrated already. The same comment is also valid for point 5 of the conclusion.

I appreciate that some of my suggestions represent a considerable amount of rewriting and thank the authors for considering them. I hope that they will find this review helpful.

Kind regards, Luca Malatesta

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2018-84/esurf-2018-84-RC1-supplement.zip

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