

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

### **Anonymous Referee #3**

Received and published: 1 February 2019

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

January 2019

Dear Editors of Earth Surface Dynamics, Thank you for the opportunity to review for your journal. I have read the manuscript prepared by Tofelde et al., and here I will first comment on my impression of the overall quality and character of the paper, then offer my opinion on acceptance of the manuscript, and finally offer some minor line-by-line suggestions of an editorial nature.

First, I enjoyed reading this well written manuscript. I appreciate that the authors

crafted an accessible background literature review (from the perspective of a non-experimentalist). In their manuscript, Tofelde et al., develop interesting and timely scientific questions and knowledge gaps—what are the responses of alluvial fill terraces to modulation of base level, and changes in upstream water discharge and sediment supply ( $Q_w$ ,  $Q_s$  respectively)—which they then address using seven experiments. I echo the sentiment of Reviewer 2 that this paper has the ring of a review paper, yet that is not a problem for me, and I actually appreciated the good explanations of current knowledge (theoretical, field, and experimental). I thought the amount of review in the introduction was appropriate to bring a non experimentalist/expert up to speed on the current thinking of how terrace incision-aggradation functions with respect to changes in upstream or downstream (base level) boundary conditions. I thought the figures are well made and that the captions are effective as well.

The results of the seven experiments performed by the authors show there are distinct responses in the slope of pre-perturbation and post-perturbation alluvial surface elevations that are dependent upon the type of forcing mechanism, and the authors document interesting transient behavior of fill terrace, channel elevations/width, and  $Q_s$  out of the experimental system with time. In experiments with increased  $Q_w$  or  $Q_s$ , gradients in the new equilibrium channels decrease significantly compared to the pre upstream perturbation channel gradients. This is a somewhat intuitive, yet interesting result, and one that presumably has the potential to be tested in the sedimentary/geomorphic record. I thought that the rationale for the experiments and the results are thought provoking to those interested in not only morphologic response of alluvial fill terraces to external forcing, but also the implications of their response to external forcing in terms of chemical signatures preserved (or not) in sediment/sedimentary systems (end of Section 5).

The experimental design did not include simulations of increased  $Q_w + Q_s$ , or decreased  $Q_w + Q_s$ , as conceivably might occur/be expected in a natural sedimentary system undergoing upstream changes in boundary conditions. Thus its possible the

[Printer-friendly version](#)[Discussion paper](#)

results of these experiments (pure perturbations in  $Q_w$  or  $Q_s$ ) may be difficult to invert from sedimentary records or be more pronounced in experiments than nature. I don't consider this a shortcoming of the manuscript, its just an observation, and perhaps the authors could include a statement about this in the discussion? Other reviewers have suggested ideas to help improve the communication of what results are novel by the restructuring of the literature review and parts of the discussion (e.g. Malatesta's comment #2). I concur that the authors should consider improving the way in which they communicate how to interpret these experimental results in the context of existing theoretical and experimental knowledge.

Recommendation: I recommend that this manuscript ultimately be accepted for publication after the authors implement minor revisions.

Line-by-line comments: P1 L9 suggest "...tectonic histories" rather than "...tectonic conditions"?

P2 L20-21 You may want to specify that (at least for Schaller et al 2004) the methods used to interpret paleo discharge were in part based on cosmogenic nuclide concentrations, not simply the age of terrace formation. Interpretations from those concentrations are in turn subject to assumptions of the systematics of cosmogenic nuclides and sedimentary dynamics.

P3 L10 The following sentence needs to be rewritten: "To our knowledge, there are no experimental studies that systematically compare how fill terraces formed through various mechanisms may differ from one another, or investigate the impacts of terrace formation on downstream sediment discharge."

P5 L33 The end of the second Section (2 Formation of fluvial fill terraces) seems abrupt; would it help to provide one or two statements that help summarize and transition into Section 3 here?

P8 L2 Suggest "channel incision" rather than "river incision"?

Printer-friendly version

Discussion paper



P9 L23-26 “When comparing terrace slopes to the active channel slopes (blue lines) at the end of each run, terrace slopes are steeper in all experiments in which upstream conditions ( $Q_w$ ,  $Q_{s,in}$ ) were changed 25 (Fig. 6 A-D). In contrast, the slopes of the terraces and the active channel in the BLF experiment are similar to each other (Fig. 6E).” This is a really interesting relationship, and one I would not have expected (though I don’t often think about these kinds of experiments), but that does seem intuitive. Is this pre-perturbation terrace slope and upstream-downstream boundary condition relationship something that is seen in other experimental studies? In nature? I see your discussion includes some mention of this explicitly, and introduces the active tectonic aspect that unfortunately complicates interpretations and adds non uniqueness to potential interpretations of terrace slope history. Can you predict/offer guidelines for which kind of natural systems your experimental results would be best applied?

P10 L22 add a space after “. . .Fig 5).”

P15 L30-31 Perhaps cite the figure # again for clarity, for which grey vs. yellow circles relate to this sentence.

P16 L6-8 The last sentence of Section 5 suggests chemical signals may be propagated more efficiently through systems during phases of aggradation, rather than phases of incision when mixing of older stored sediment might overprint the chemical signature of “fresh” hillslope derived sediment. This is interesting. . . Your statement makes sense, however would it also be fair to say that the chemical signature would be a function of the ratio of the “fresh” to recycled sediment (and obviously the erosion rate upstream)? And that those ratios could vary greatly given different system scales (I’m thinking about the ratio of upstream derived  $Q_s$  vs excavated volume)? Perhaps this is a tangential idea more suitable for its own paper?!

---

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-84>, 2018.

Printer-friendly version

Discussion paper

