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Dear Editor, I have read the new manuscript by Savi and colleagues, Interactions between channel sand tributary alluvial fans: channel adjustments and sediment-signal propagation. The authors present the results of six flume experiments where they modelled the dynamics of a tributary stream building a fan onto a trunk channel (both transported-limited with uniform grain size and a discharge ratio 2/3). They tracked the evolution of sediment flux (Qs) and topography after changing water discharge (Qw) or input Qs in either channels. The authors build a classification framework with four cases mapping the types of interaction between tributary alluvial fans and trunk channels and their likely Qs signature. The article is well written and the experiments are exhaustively described. While this fluvial configuration is quite particular, it will be a very useful resource for anyone work-ing on similar or related features. The manuscript merits publication in e-surf after some amendments. I have comments related to: 1) the structure or nature of the manuscript as review/experimental paper; 2) potential confusion in parts of the description (text and figure) of the experiments; and 3) technical aspects of the discussion. I start by general comments on the manuscript and then move to focused remarks before a short list of miscellaneous details.

> *We are thankful to the reviewer for the constructive comments. Our answers and the changes made to the text are reported as in-line comments.*

Review/experimental paper

The manuscript tries to strike a balance between review paper and niche flume work which I find uneasy to read. The introduction and the background take up the first 8 pages of the manuscript (more than a quarter of the text). They are well-written and offer a quasi exhaustive, if sometimes repetitive, review of the literature. Besides repeated teasers of the flume work to come, the reader could forget it's an experimental paper until the methods section on page 9. Only then the nitty gritty flume work begins. In my opinion, the readers who are interested in a contribution on such a fairly niche setting will be well versed in most of the concepts detailed in the first pages. One or two refresher paragraphs on the graded stream and the relationships between Qw, Qs, and slope should be enough. Below some examples based from the text.

> *Following the reviewers' comments we have strongly reduced section 2 ('Background') leaving only few background information that may help the reader to better appreciate the results of our study.*

Section 2

The whole section is a review that I would estimate unnecessary or at least that could be trimmed generously. Only the paragraphs l. 168-172 and l. 224-232 are really important here because they introduce and contextualize the vocabulary used to describe the experiments.

> *We have moved some of the important lines with the vocabulary in the introductions and strongly reduced the whole section. The following passages, mentioned by the reviewer, have been changed or deleted.*

l. 142-153: this paragraph reads like an introduction and repeats many elements of it. It could be advantageously cut to avoid redundancy.

l. 175-178: this has already been stated and doesn't need to be repeated again.

l. 206-208: reads like an introduction.

l. 239-241: same

If the review should stay, I believe it would be then appropriate to balance the paper and tie up the discussion with reference to the reviewed field sites. It would be particularly strengthening for the framework proposed. For example what would all the one channel studies e.g. Simpson Castelltort be missing by ignoring tributary feedbacks?

Complex feedbacks as motivation for study

The potentially important role of tributary feedbacks for buffering or accentuation of environmental signals (l. 63-66, l. 131-132) appears particularly important to me. I would suggest to emphasize it further, and especially to highlight the broader impact to the entire sedimentary system. Maybe you could build a case of how the effects of tributaries could strengthen or weaken the dynamics described by Simpson and Castelltort. That article is well known and I think that it would make your work even more approachable to the reader.

Thanks for pointing this out. We have added few lines in the introduction and discussion which point to the importance of these feedbacks and interactions for the whole sedimentary system, in connection with the work and results of Simpson and Castelltort (lines 64-65 and 860-865 in the manuscript version with changes).

Motivation for the flume setup.

Somewhere in the text, maybe in a new section 2, the target landscape of the experiments should be spelled out. The flume seems to be representing the following fluvial landscape: two transport-limited streams (one twice as large as the other) with the same grain size join in a broad alluvial valley/floodplain of unlithified/uncemented sediments. The tributary builds an alluvial fan in the trunk channel. For the case of junctions between alluvial streams of the same order of magnitude Qw and same grainsize I would not expect the growth of an alluvial fan. The cases I have in mind where a tributary alluvial fan disturbs a main trunk are higher upstream. Paradigmatic would be the Illgraben Fan growing in Rhône Valley and constraining its river flow. In this case and the many others I can remember, there is an important grain size difference. I think I simply don't have the right references. I suspect that many readers may share the same experience as me. It would therefore be useful to discuss some field sites where the flume setup would apply. Preferably some that were studied for that dynamic.

We have added the description of the represented landscape in the method section (3.1).

We understand the point raised by the reviewer and it is true that this setting may be peculiar of some specific region, as it may be the case of some catchments in the arid regions of north-eastern Argentina. There, thanks to several clast count measurements, we have evidence of jointly rivers draining alluvial material and carrying similar grain sizes (e.g. the Yacorite river joining the main Rio Grande in the Jujuy province of north-eastern Argentina). The tributary shows remnants of a paleo alluvial fan, suggesting that sometime in the past the Qs or Qw discharge of the tributary where different from those of today. However, the rivers have not been studied for the purposes analyzed in this *paper. Additionally, in most cases when an alluvial fan builds up in a main channel, the grain size distribution of this latter system is expected to change, as the channel slope adjusts to the incoming material brought by the tributary. It is clear that our examples represent a simplification of what may happen in natural settings, where the parameters that enters into play are many more than those used in the experiments. This is indeed a limitation inherent of our flume study. We have added a paragraph (5.4) on experiment limitations where we discuss, among others, also this aspect of the experiments and hope to accomplish to the point raised by the reviewer.*

Representativity of each model run

There misses a discussion of the relevance each individual run for the scenario explored. As detailed at length, alluvial systems have rich dynamics with a lot of stochastic processes. How confident are the authors that each run is a representative unique outcome of the scenario tested and not one of a wide range of possible evolutions? I fully understand that this is an inherent limitation of flume studies as each run represents tremendous work, but it would strengthen the framework if this limitation is directly addressed in a short paragraph.

We agree with the reviewer and we discuss this limitation in the new paragraph 5.4.

Line by line

• l. 121-130 The experimental work by Bonnet and Crave (Geology, 2003) on directionality of perturbations in landscapes would be particularly relevant for this paragraph.

We thank the reviewer for pointing this out. We have added a sentence to include the reference to the work of Bonnet and Crave.

• l. 254 It may be good to explicitly write that the level of the water sill is fixed.

Done.

• l. 269 I would suggest to point to Table 1 at the end of the first sentence already.

Done.

• l. 278-279 This seems a tall order to me. There is a lot of stochastic and non-linear processes in such a system. Wouldn't adding its parts yield more than their sum? Is there a reference for the feasibility of this?

Yes, true. We cannot be sure that other processes do not interact. We have removed the sentence.

• l. 333-335 This sounds more like the quantification of "straightness" rather than symmetry. The latter implies features within the floodplain to me. maybe add "axial" symmetry? this would make the link with the source-to-outlet straight line clearer.

Done.

• l. 367-369 For clarity's sake. V is then the volume of all sediments that were moved in the time interval, regardless whether they exited the section or not. It is the summed volume of all parcels of sediment mobilized during the interval, whether observed as new deposit or as new erosion. However, any sediment bypass would not count toward V regardless of its sediment throughput. I think that this is what I understand from the text.

Yes, this is correct.

• l. 381 "deposited"? as in incised and deposited.

Yes, changed.

• l. 385, l. 389-390: How long is the spin-up phase? Is it 300 minutes after which the changes are observed (Figure 4)? And the spin-up phase is the complete adjustment to boundary conditions, correct?

The spin-up phase represents the initial adjustments from the hand-made channel shape. Its timing changes from run to run and we have added a column to Table 1 where we stated, for each experiment, its total length and the spin-up time. After the spin-up phase the channels adjusted to the boundary conditions.

• l. 546 "mainly" how can the valley widen in other ways than bank erosion?

True, we have removed the word.

• l. 557 "once the tributary reached equilibrium": from a slope perspective? It would be useful to restate whether it was after incision or aggradation.

Yes, from a slope perspective. We have clarified it in the text. We are discussing here the T_NC1 experiment, so the system adjusts to the initial boundary conditions.

• l. 569-570 Is this change in sediment mobilisation that visible in Qs_out? Or is the lack of tributary Qs merely replaced by main channel Qs during transient phase?

Yes, the lack of Qs from the tributary is offset by the increased Qs in the mainstem from incision of the upper section. Therefore, the changes occurring in the tributary are not that visible in the Qs_out of the middle section. However, we do observe the delay in sediment transfer looking at the DoD figures (now moved to the supplementary material). There, we can observe that when the perturbation starts, sediment is initially deposited at the fan head and only with time is moved towards the main channel.

• l. 577-578 "blocked" what is the exact meaning of blocked? Does it mean that 100% of the upstream sediment flux is effectively blocked, or that the sediment flux is limited and part of it is deposited?

The second. We have added the word "partially" to clarify it.

• l. 592-593 What kind of deposits are we talking about here? The material buried underneath the floodplain or terrace deposits where available?

When possible, all of them. The more information available, the better incision and deposition histories can be reconstructed.

• l. 684 one "r" is missing in prograde.

Correct. Thanks.

• l. 702-704 The dynamic of that competition must be heavily influenced by the respective erodibility of fan and bank. I imagine that a balanced situation like this one is rare. Tributaries often carry coarser sediment than the floodplain of the main channel. Or conversely floodplain material can be significantly consolidated and much harder to erode than loose fan material. Not even mentioning bedrock-lined valleys. It might be worth discussing comparisons with field examples again here.

> *We guess that with "balanced situation" the reviewer refers to all settings where two rivers flow on an alluvial plain. Although our set-up may resemble this type of landscape, we do not actually described a "balanced situation". We observed that a perturbation in the system produced a response those prevailing effects depended on the relative "strength" of the two rivers and the competition between them. In this context, when the tributary is prevailing the main channel gets deflected more, whereas when the main channel is "stronger", it manages to have a more straight path. Of course it is a simplification. There are many aspects that cannot be taken into account when working with lab- experiments, as it may be the case of different erodibility between fan and main channel or the presence of vegetation. Although they can change the dynamics of the system and the mechanisms with which sediment is moved, we could not evaluate their impact with our experimental setting. This has also been added in the limitation section.*

• l. 780 how? where?

> *Data will be made available through the Sediment Experimentalists Network Project Space to the SEAD Internal Repository and will possibly be accessible by the end of February 2020.*

Figures

• Figure 4: This is a very important figure but it is unfortunately hardly readable. Most profiles overlap and any pattern of change is almost impossible to decipher.
Have the authors tried to subtract the elevation along the average slope of the first profile from all profiles? This detrended curve would allow to spread the plots in the vertical. Further, the colour scheme is most likely not colour-blind friendly and should be amended (see Crameri's scientific colour scales for example).

> *We see the point. We have changed the figure following the reviewer's suggestion (each profile now plots with a scatter in elevation and is shown against the first-profile's average slope profile). However, we also kept the original plots to not lose the information about the changes in elevation. We also changed the color scheme, and Figure 5 and 6 (now Figure S1 in the supplementary material) accordingly.*

• Figure 7: the small outlines of the fan shapes is a great idea!

> *Thanks!*

• Figure 12: typos in "decoupling". The figure would be much stronger if examples from the field were listed to anchor these cases in a familiar context. What about aggrading main channel? Where does this setting fall?

> *Thanks for the typo. We understand the point of the reviewer but, considering that this figure is already very rich and contains a lot of information, we would prefer to not add*

extra information on it. However we could add some examples if the reviewer strongly believes that it will be an added value for the manuscript. Nevertheless, we would like to point out that we are not aware of studies that have specifically analyzed the information reported in this paper, so that examples of field-cases would not really match the information reported here. Indeed, we explored the interactions between a tributary and a main channel and how this interplay may affect the transfer of sediment. This represented a knowledge-gap that may hinder important information for the reconstruction of climatic or tectonic histories of a certain region. Here, we provided a theoretical framework that may help filling this gap. It will be the readers who would need to see how our results may fit their own field site and up to which level they can use our framework for their analyses.

The case of aggrading main channels has not been tested in our experiments.

Good luck to the authors for the revisions,

Thanks!

Best wishes, Luca Malatesta