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Interactive comment on "Fluvial response to changes in the magnitude and frequency of sediment supply in a 1D model" *by* Tobias Müller and Marwan Hassan

Anonymous Referee #2

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Summary. This manuscript details the use of a numerical model to understand how a bed load sediment transport system within a flume will adjust to variations in the frequency and magnitude of sediment supply. The model is calibrated by tuning parameters to match slope and grain size data produced from previous flume experiments with variable sediment supply. Post model validation the manuscript explores a wide range of grain size distribution width and sediment supply frequencies and identifies two key timescales (the fluvial evacuation time, and the timescale of sediment armor) necessary to explain the results. The manuscript concludes with a short analysis of these ideas applied to a field setting.

General Comments. In general I am supportive of the authors methodology and approach for tackling the issue of sediment supply pulses within a bed load system and believe that the manuscript could be well suited for publication within Earth Surface Dynamics. The number of model simulations exploring a range of sediment supply frequencies and grain size distribution width variants provides a very robust analysis of the problem put forth in the manuscript. The culmination of the partial data collapse using the derived fluvial evacuation and armoring timescales provide the key results and major synthesis of the large number of model runs. These are very informative results as to the problem of sediment supply variations on reach scale bed dynamics (slope and grain size). My primary concerns with the current iteration of the manuscript is that it is particularly challenging to follow as currently structured and reads like two manuscripts stitched together. Below I offer some suggestions as to how I believe that these results could be restructured to enhance the clarity of the manuscript and would recommend publication if the authors can substantially tighten the structure and narrative.

The simplest way to restructure the manuscript, and I make these as suggestions only as I want to respect the authors ability to structure their manuscript as they see, would be along the lines of the following. Split the results into model calibration and testing via extension of the flume results which would require moving the model validation section to the beginning of the results section. I would suggest renaming the initial section of the results model calibration as well. Move the current analysis section (section 3) to the discussion along with figures 6c and 6d. I recommend this because currently the primary results of the analysis section are not strongly utilized until the end of the results and seem to have been developed as a way to understand the results. This way the results can be presented as model calibration (figure 3), flume results extension (Figure 4), model extension via exploring sediment input frequencies and grain size (figures 5-6a & b). The initial discussion can then talk about limitations of the model and flume results before the important synthesis results. This should allow for the reader to fully understand the results before they are used to develop the synthesis timescales (currently introduced in section 3) that tie everything together in 6 c&d.

In my reading the other issue to tighten up is the link between the field, flume, and model. The manuscript currently uses prior flume results to 'validate' the model behavior and then uses this connection to place the model results into context with field systems. This to me feels like a broken chain of logic though, as the connection between the flume and field is never established therefore the connection between the model and the field is never properly established or verified. Depending on how one views flume results as either small scale field analogues or separate systems with similar physics (my personal view) this connection of model applicability to the field is tenuous. This is why I comment below in the 'specific comments' section on adding a more robust discussion of the comparison between the flume and model results. Currently the flume results are used to calibrate the model rather then validate it, as several model parameters are tuned to match (by eye?) the flume results. This discussion would give the reader a better idea about how these results can be interpreted and where and at what scales we might expect the results to transfer well. This will help with the discussion when applying the understanding of the model results to the field as the connection of the field to flume and flume to model will already be solidified. A short example, transport in the flume is a stochastic process and the model only represents one representation of this probabilistic system (I believe that Wilcock and Crowe represents the mean behavior in a best fit sense). In many ways the model only seems to need to represent the mean behavior to get the large scale behavior of the flume right, but this is a neat result. Apologies for the long windedness.

Additionally, I would encourage a very brief discussion of (1) how higher or lower shear velocities would impact the results in figure 6, particularly through the modulation of the fluvial evacuation time and mobility of the grain size mixture, and (2) how these results would change for a partially mobile mixture. This discussion would add value to the conclusions of manuscript as it is insight that the authors likely have into this problem that is challenging to parse at the moment.

Rewrite - Include a model validation section and put figure 3 in that section, then the current analysis discussion can be placed there as well. Currently all discussion of figure 3 prior to its appearance shows up on pg. 8 while figure 3 is on pg 12 with scant discussion of the results there. Or move all of the results discussion of figure 3 to the current results section.

Minor Comments. In the model are all sediment types mobile for the given flow?

Specific Comments. pg 2 ln 1 - Do you mean 'concentrated activity'?

pg 2 ln 2-4 - It is not clear that this statement is necessarily true. At this point it would seem that the number of papers are fairly equally split between large fine-grained lowland rivers and coarse-grained rivers in or near the mountains. I would suggest either making it more specific by qualifying the mountain stream (i.e. steep gradient) or providing a definition of what the authors consider a mountain stream? As an example, for the purpose of this manuscript is a stream bounded by talus slopes the same type of mountain river as one meandering in the bottom of a mountain valley?

pg 2 ln 22-31 - There a quite a few articles (Cui et al., 2003 a b; Cui and Parker, 2005; Lisle et al., 1997; and especially Lisle et al., 2001) some of which are cited above in the intro that deserve a bit more discussion in the opening paragraphs. I think a subset of these papers could be worked into the current manuscript within this paragraph. The results they present are not always for sediment supply, but many similar concepts of adjustment time to a perturbation in a fluvial system are the same and would help place this paper in context (i.e. the cycled hydrographs of Wong and Parker 2006, and the followup by Parker, Hassan, and Wilcock [Gravel Bed Rivers book chapter] where they have a similar parameter to the fluvial evacuation time).

pg 2 ln 23 - Lisle et al. (2001) report the 'dominance of dispersion' in the instances of larger sediment supply events. There is likely an interchange between magnitude of supply input and in-situ reworking, diffusive transport, and total reorganization of the system. Something to think about.

pg 2-3 ln33-3 - Please provide a working definition of an equilibrium channel. It is not clear from the text what constitutes a transient form or an equilibrium form in a river channel or what level of adjustment is still considered to be no longer equilibrium. (note - there is a definition much later in the paper, but that could be placed here) Building on this comment - the narrative (the writing itself is fine and clear) present from page 2 lines 32-35 through page 3 lines 1-15 appears very speculative and rather than illustrating the proposed importance of event frequency the message it conveys (to me) is that a lot of stuff can happen and it appears very complicated. I understand the authors interest in painting the landscape of the literature about adjusting and systems disturbed from equilibrium but these examples as presented are very hypothetical and lack precision. These paragraphs would carry more weight if the mechanistic studies cited earlier on page 2 lines 10-21 were worked into them to demonstrate what has been tested or explored. As an example the discussion of relaxation times and reaction times has always been vague in the literature as what are the variables at play here or is it just all the variables for a system with at least three or four (commonly measured) degrees of freedom (grain size and distribution, width, slope, flow regime). There is a lot of information on river adjustment to perturbations within those works and the sorting literature that provides firmer footing for the current study (especially because the current manuscript models a flume and those are flume studies by and large while the cited examples here pertain to field studies for the most part).

pg 3 ln 20 - Editorial suggestion 'must not be' to 'is not' or 'might not be'.

pg 4 ln 14-19 - Please split this into several sentences as it is hard to follow as is. As a general rule of thumb sentences over 30-40 words become unwieldy (this is $\sim$80 words long).

pg 4 ln 28 - first 'water depth' followed by Q should be discharge. Assuming all of the units here are SI?

pg 7 ln 11 - 'depending' to dependent?

sec. 2.2 Model Validation - How was sediment flux measured in the experiments and at what frequency? I did not see this mentioned in the section.

pg 8 ln 18 - Is a value of t*rm not available from the flume experiments? It seems odd that a factor of 2 increase is required. This should be explained as it suggests that we might be missing a parameter in the equations that treating the threshold as a fitting parameter hides (sidewall corrections?).

pg 8 ln 19-20 - Overall the model provides a good approximation of the flume data, however there appear to be additional consistent differences between the model and the flume that may not be just sampling related and it would be nice for the authors to comment on this as it directly relates to the relaxation and response time of the system. See comments to figure 3 below for specific differences.

pg 9 ln 10 - This definition would be helpful within the introduction. If the other authors cited use different definitions, than those can be recast as how they differ from the definition used by the authors which is clear to me.

pg 11 ln 1 - This sentence is not clear, please consider rewriting it.

pg 10 ln 16-17 - pg 11 ln 1-20 - The writing here is fine, but this reads like discussion prior to seeing any of the results. Suggest moving this to a different section, either before the results in a theory section or after the results. It feels out of place in the analysis section.

pg 12 ln 14-18 - This is mostly duplicate text from earlier sections.

Figure 3 - More discussion on where and how the model matches and does not match the flume results seems warranted. In particular, could the authors comment on why the model response and relaxation in channel slope outpaces the flume? Other areas of interest are from hour 180 onwards the flume results indicate almost no change in slope despite the model displaying fairly dynamic changes. There is some case of sparsity of the flume measurements (because they are time consuming, no fault

there) that inhibits a direct comparison and a good example of this is during the four pulse phase and maybe the initial one pulse phase. However, the model response to the two pulses (hrs 160-200) is substantially more dynamic and to match this via an undersampling argument the model results would need to shifted vertically, but that would produce a stronger mismatch with the preceding experimental pulses. I don't think that the model does a poor job, quite the opposite, but the authors should explore the source of the discrepancies as they seem to indicate a more dynamic flume system in terms of response to pulses, in that there may time sensitive parameters that the model assumes constant or that there is a time sensitive parameter in the flume that we might be missing (or error/variability in flume measurements). I think expanding on this will help in understanding the aspects the reader should interpret as having greater or less precision for the following model results. Additionally, can you comment on why the model predicts a nearly constant D90, while the flume shows a slow decline in (c), and the potential origins of the model lag in transport rate in (d).

pg 14 ln 16-23 - This information is better suited for figure captions.

Figure 6 - (a) what is the numbering scheme for the Tpp axis? (b) can this be put into hours so as to match 6a? In 6a the distributions seem strongly skewed, in that the mean may not be the best statistic to represent the distributions especially from about 80 hours onward. (c) is the xlabel supposed to be Tfe instead of Tfc?

pg 19 ln 16 - lower = shorter?

pg 20 ln 2-4 - This might be a bit of a stretch as the total difference in slope ranges (6a) from 0.8 to 1.2 from an equilibrium profile and the armor ratio compared to equilibrium only differs by a factor of 1.2 for these simulations. These are pretty small differences to pick up in the field with parameters that sometimes have at least factor 2 to order of magnitude variability. To be fair this section is a reasonable idea to pursue but it feels underdeveloped here to the point that it may not yet be practical given field data sparcity and error.

pg 20 ln 5-17 - This section and Table 4 feels like a non-sequitur and seems underdeveloped compared to the rest of the paper. I recommend removing it and extending these ideas into a different manuscript. Substantiated with more data from different field sites these ideas could be expanded into a short format paper.

Data availability - Nothing mentioned that I could see. No mention of model code or data from the model runs that were presented in the figures. Lab data used for validation from Elgueta-Astaburuaga and Hassan (2017) is available upon request from M. Hassan. I do not know Earth Surface Dynamics data availability policies, however as it currently stands the data used in this paper is not available and the flume data used to calibrate the model is only marginally available.

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