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Comment on egusphere-2022-53

Anonymous Referee #1

Referee comment on "A comparison of 1D and 2D bedload transport functions under high excess shear stress conditions in laterally constrained gravel-bed rivers: a laboratory study" by David L. Adams and Brett C. Eaton, EGUsphere, https://doi.org/10.5194/egusphere-2022-53-RC1, 2022

The paper "A comparison of 1D and 2D bedload transport functions under high excess shear stress conditions in laterally-constrained gravel-bed rivers: a laboratory study" submitted by Adams and Eaton provides an analysis of flume measurements of bedload transport. A poorly graded sediment mixture figuring a gravel bed river reach was submitted to relatively long-lasting experiments, each lasting 16+ hours. The channel was laterally confined and the slope and discharge were sufficiently high to generate relatively intense sediment transport (only relatively since the transport stage Tau/Tau_c < 2). The authors also perform accurate measurement of the bed topography of the drained channels. These data were used in a 2D numerical model to compute flow depths and shear stresses across the whole flume. These data, i.e., solid discharges and modelled flow depths, are then used to shed light on one of the most basic way to model bedload, i.e., using threshold equation of the Meyer-Peter & Mueller type. By using either spatial average or detailed estimations of the shear stresses, the authors recall how different it is to compute things in 1D and 2D, especially threshold shear stress for incipient motion.

The paper is of interest and fits well, in my opinion, in the scope of the journal, i.e., Earth Surface Dynamics. The approach is rigorous and the paper well written. In my opinion, it is an interesting contribution to explain and highlight the implication of non-linearity of sediment transport equation and effects of using threshold equations. It will interest readers curious in better understanding what the difference between 1D and 2D approaches are. It is also of interest for experienced modellers as an experimental proof of concepts that are known by the community but to my opinion, often neglected.

Although threshold equations for bedload transport are widely used in the community, and thus deserve to be critically appraised to demonstrate their limitations and how they should be calibrated, I think the paper would gain in interest if the authors would provide also a discussion section on the alternative way of using non-threshold approaches (e.g., the Recking, 2013b equation). To my opinion, the whole community studying sediment transport in gravel bed river is still suffering a deep influence of the pioneering works performed in the lab with near-uniform sediment mixtures and narrow flumes (not allowing the geomorphology to develop). In such cases, threshold equations and the concept of threshold of incipient motion make sense to my experience. In natural sites where bedload sediment sizes cover one to two orders of magnitudes and where geomorphology is fully developed, is-it still acceptable to use concepts as threshold for motion and threshold transport equation? I think it does for some targeted tasks, especially for numerical modelling (1D and 2D), thus the interest of this paper. But conceptually non-threshold approaches make more sense to my opinion. This has been quite extensively discussed in the paper of Recking (2013a). Regarding the past publications by the first author, e.g., the paper in *Progress in Physical Geography,* and his demonstrated capacity to shed a new critical light on the topic of geomorphology, hydraulics and sediment transport, I suggest to try adding a section having a broader perspective than to critically assess the way threshold for motion should be approached: what the concept is useful for and what are its limitations.

I also suggest trying injecting a bit more of process description in the results section: try going beyond the graph analysis to tell us a story about the channel, the geomorphology and the flows.

Last comment: I find it surprising that although Prof. Eaton wrote very good papers demonstrating that using D50 to assess bedload transport in gravel bed rivers is often irrelevant and that D84 should be preferred, this paper still focus on Tau_c50. Please explain why you did not perform the whole analysis on Tau_c84. Maybe, it is one more argument to shift from threshold equations to go for non-threshold?

I suggest a few additions in the next comments:

-L97: I am not sure to understand the ", and every 15 minutes,". Does it means that the mesh buckets were weighted and reintroduced on the conveyor belt every 15 minutes whatever the state of the flume (i.e., drained or not)? Please clarify.

-L112: why did you use the final DEM and not another one within the three last hours? If this is arbitrary, please just say this.

-L122: I think it is necessary to justify this threshold of 2*D84 as limit for wetted area. From some basic computations, I understand it corresponds more or less to a Shields stress < 0.05, so it seems acceptable to me but the reader should not have to perform computation to judge whether this threshold is reasonable or not. Please provide your justification.

-Figure 2: what exactly are those error bars? The two mm accuracy of the gauge? But these values are mean values over the flume so how much was the standard deviation over the several measurements and how did you propagate uncertainties (epistemic, i.e., 2 mm) and aleatoric (i.e., standard deviation over every gauges)? Please clarify and elaborate on the variability of the measurements at the profiles.

-L146: the authors have sufficient data to compute a proxy of the sediment discharge variability to be later used an error bar in the graphs. I suggest computing for instance the standard deviation of the sediment transport of each run on the 3hr period and to add this information in Table 3 as well as in the graphs (e.g., Figure 9).

-L150-170: I suggest to provide full equations for the four ways sediment transport was computed, clearly differencing 2D and 1D and also model and d/S. Consider that for many readers, the uniform flow hypothesis is so deeply assimilated that it might not be clear that Tau is far from being everywhere and always equal to rho * g * <S> * <d>.

-L174: 52%-95% is a "small portion"??

-L175: I think it make sense to perform a computation of the unit discharge over only the "wetted area" but a quick control with the 2D modelling is important to my opinion: consider providing the information to the reader that only X-Y% of the water flux pass in the non-wetted area where d<2*D84. In other contexts, a large share of water is flowing within such shallow flows.

-L179: A short sentence linking this statistical result with a process interpretation would be appreciated. Am-I correct that it means that at reach-scale, indeed <Tau> ~ rho * g * <S> * <d>?

-L183-185: I am not sur to fully understand this statement. Could you try helping us to interpret it? Almost no cells comprised condition with Tau_c< tau(x) < <Tau>, and thus almost all wetted cells have Tau(x)>Tau_c? Is it a strong evidence of stress concentration?

-L198: consider adding something like "i.e., high shear stress and deep flows are close but not at exactly the same locations" to make sure the readers understand.

Consider also adding a comment on the effect of morphology on this scattering: would a plane bed morphology present a similar distribution and scattering for instance?

-L213: only 20 hours? More than that for the 16+ hrs of experiments at scale 1/25, no? A 20 hr flood is not very long so I suggest using a bigger number to convey a clearer message.

Figure 6: please add a legend of for the black dots to in the right panel.

-L227: I do not agree with this statement: you compute the mean flow depth or mean shear stress over a river reach using finely tuned 2D model results. An actual 1D approach is, to my experience, rather to measure a transversal profile (or a couple of them) and to compute the flow depth using a uniform flow hypothesis. It is somewhat more similar to the measurement performs at the gauges in the experiments. Knowing in details the flow depth distribution over the reach would be fantastic for people performing field analysis and 1D analysis but it requires to drain the river as you did (not easy to do at field scale).

Consider providing some comments on the differences between mean depth and local or multiple profile measurement or correcting your statement to make it more precise.

Figure 8: contour lines or density shading would be appreciated: in almost all of the interesting region of the plot there is some much overlapping between points that we do not know if a few or thousands of points are plotted.

-L248-251: This topic is also extensively discussed in Recking 2013a.

-L263-264: here again, I agree with the statement but the way it is written give me – a non-native speaker, I must confess - the impression that no other approach exist. Pity. What about non-threshold approaches?

-L323: Missing Journal in the reference

-L385: Missing Journal in the reference

References:

Recking A. 2013a. An analysis of nonlinearity effects on bed load transport prediction. Journal of Geophysical Research: Earth Surface **118** : 1–18. DOI: 10.1002/jgrf.20090

Recking A. 2013b. Simple method for calculating reach-averaged bed-load transport. Journal of Hydraulic Engineering **139** : 70–75. DOI: 10.1061/(ASCE)HY.1943-7900.0000653