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Earth Surface Dynamics Discussions

Interactive comment on "Stabilising Large Grains in Aggrading Steep Channels" *by* William H. Booker and Brett C. Eaton

William H. Booker and Brett C. Eaton

william.booker@alumni.ubc.ca

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The general comments submitted by Referee 2 are addressed below. We would like to thank the reviewer for their comments, especially those regarding the methods, as we think they will greatly improve the paper and its arguments.

General Comments

Reviewer: Reorganization of introduction - I think the introduction reads fairly well, but that further motivation could be provided by discussing the predictions of Lane's balance at the beginning of the article. One could use the idea that Lane's balance

would predict the same slope for a give D50, regardless of the rest of the GSD as a null hypothesis, then reference the known importance of large grains in degrading systems and the lack of complementary work on aggrading systems in order to more directly motivate this work. I think this reorganization could help to streamline the logical progression of the manuscript.

Author: This feedback agrees with those made by the first referee, with a reorganisation of the information displayed in the introduction necessary to improve the communication of this importance. As a result, the structure is being re-written to relocate Lane (1955) and introduce Church's (2006) conceptualisation of it. We also agree that this will improve the flow of logic within this article.

Reviewer: (1) How where the discharges determined? Are they specified to span the range of partial transport to full bed mobilization? It would also be useful to provide the calculated/estimated shear (or Shields) stresses related to each of these discharges of both flows. I'm aware that this may require some assumptions in relation to the sidewall correction, but given that most of the literature on this topic is presented in terms of Shields stress, it would be useful to also provide this estimate, especially for the discussion of relative transport capacity.

Author: Discharges were determined based on initial conditions used during trial experiments, as well as their ease of calibration in setting up these experiments. The discharges were not calculated to correspond to any given shear stress value; as the deposit slope was set by the sediment transport dynamics, we were unable to predict the corresponding slopes. Without controlling the slope of the deposit, as is traditionally done in such experiments, we could therefore not relate discharge to shear stress during experimental design.

Reviewer: It took me until halfway through the results to recognize that the multiple measures of slope presented were from different time steps following the onset of sediment transport out of the flume. How long were the experiments run after this point and

how were the experiments determined to be over? Was an equilibrium slope/transport rate reached or were adjustments still occurring when the experiment ended? If equilibrium was reached, how was it determined?

Author: In these experiments, equilibrium was not a concept explicitly used in the determination of any experimental condition as sediment output had to be dried and weighed, which was used to feedback this information during the experiment. Therefore, we are careful to avoid usage of the term "equilibrium" in discussion of the dynamics involved here. As a result, the length of each experiment was solely determined by the volume of input material available for sediment feed. In addition, during the experiments themselves we used the morphodynamics as in-situ justification; we believed that they had not substantially changed between when sediment was output and the end of the experiment. Therefore, we believed that the system behaviour was not in flux when the sediment supply ran out.

Reviewer: (3) For the slope-derivation, I think more information should be provided regarding the randomForests model, how it works, and the degree of user-specification it requires. How many images are input in order to determine the slope? How are the sub-classes determined? Are there uncertainties associated with these slope measurements based on the method or number of sample images input? A citation here providing the relevant background information could also help. The authors later report the mean slope and standard deviation for each experiment, but it is unclear if this is from multiple time slices (if so, how many?), multiple locations in the flume, or related to some uncertainty in the slope estimation. Organization-wise, I don't necessarily think this needs its own section in the methods. Alternatively, I might suggest splitting the methods section into (1) Experimental set-up, (2) Measurements, and (3) Slope derivation.

Author: We greatly appreciate this advice, as it is very helpful in improving the layout of the relevant sections, and will be used to clarify the methods employed within the paper for the reader.

Reviewer: (4) I find GS1 and GS2 not to be very informative variable names. I would suggest changing them to GSnarrow and GSbroad or something more information so it is easier for the reader to keep track of throughout the paper. Even H and L are a bit confusing to keep track of, but less so.

Author: We will take this into consideration when we are restructuring the paper for clarity.

Reviewer: Organization of the results section - I found this section to be a bit muddy, with parts ′ of the motivation, methods, and discussion being mixed in. While I am okay with some intermingling of these sections, in this cas, I found it to make this particular section a bit difficult to follow. Below I've made some suggestions to streamline this section. (1) Move Lane's balance discussion to introduction. See above. (2) Move sediment transport efficiency calculation to methods. I would suggest adding this following the slope derivation. If Lane's balance has already been presented in the introduction, it would naturally follow to calculate the sediment transport efficiency. Introduction of this calculation in the methods would allow the authors to more cleanly step through the results. Again, some information of the number of samples used to make these calculations would be helpful (table 5).

Author: These comments support the changes we have already made and incorporated into the technically corrected version of this paper, agreeing with the suggestions already made by handling associate editor.

Reviewer: (3) This is a style thing, but I would suggest avoiding things like "Panel A of Figure 3 shows.." and instead simply say "There is a significant difference between equilibrium slopes as a function of the supplied grain size distribution (Figure 3A)." I think this would help with readability.

Author: We agree that the manner of text in this piece can be improved, and we will update the relevant sections with clearer language.

Reviewer: (4) Much of the information in the tables is not fully presented in the paper. I would recommend more explicitly discussing these results in the main text. Lots of the results are presented in a fairly vague way (e.g. - ". . .both systems retaining a higher proportion of sediment" even though the authors have quantified these effects more directly. I would suggest rephrasing to provide these values directly in the text (e.g. – ". . . in response to a doubling of sediment supply, both systems retained a higher proportion of sediment, XX% for the narrow GSD and XX% for the broad GSD." This in-text quantification would also help to clarify the main differences between the experiments.

Author: We will employ these changes in order to make explicit the differences between the two grain size distributions, introducing these into the text will make the points easier to get across.

Reviewer: Argument for large grains – While I find the argument that the transition between partial transport and full mobilization of the GSD drives the observed differences in slopes observed in the experiments reasonable, I am not entirely convinced that the data presented really show this. I agree given the results that D50 is a poor metric for predicting behavior in aggradation systems, but I think more could be done to support the argument of the importance of large grains. Do the authors have any observations from the experiments to be show this? For example, was the sediment exiting the flume sieved to determine the GSD of the transported sediment compared to the supplied sediment? Can the photos/videos of the bed be used to determine if there is significant sorting that arises during the experiments that may support this idea? I imagine that the videos could be used to track the mobility (or immobility) of the largest grains (or the bed surface as a whole) in the flume to better evaluate this idea.

Author: The addition of this kind of data will likely improve the strength of our arguments. The most demonstrative data for this would likely be images taken from the videos, which we will be integrating in the results discussion to make clearer to the reader the differences in bar dynamics.

Reviewer: The portion of the discussion where shear stress calculations are made is quite confusing. It is unclear what inputs are being used and what information is being drawn from the calculation. Specifically, this sentence is quite unclear "Equation 4 produces a shear stress 44.4% greater for entrainment of the D84 than the entrainment of the median in GSD1 than in GSD2." I assume the authors are solving for τ_{ri} with reference to the D84 of both GSDs, but the reference stress value and the actual calculated values should be made explicit to better support this point. Additionally here, a comparison to the estimated shear (shields) stresses applied in the experiments (see previous comment) would help to bolster this point.

Author: Thank you for this comment, we will update the wording to better clarify this section. We will also add a table to include these values for the reader's ease. In addition, more work by Alain Recking (Recking, A., 2013. Simple Method for Calculating Reach-Averaged Bed-Load Transport. Journal of Hydraulic Engineering 139, 70–75. https://doi.org/10.1061/(ASCE)HY.1943-7900.0000653) will be used to support this point in conjunction with the aforementioned shear stress calculation.

Reviewer: Discussion of bar formation and effects – Currently, I think this point of the discussion appears as an afterthought. While I agree that this might not be the main result of the paper, the authors describe the differences in bar presence and morphology between GS1 and GS2 experiments in order to support their conclusions regarding the role of large grains. If this is a main point to bolster the argument related to the importance of large grains, mapping of these bar formations and quantifying their differences between runs should be included in the methods/results sections of the manuscript. This discussion would be better supported with photos or measurements in the text to more clearly illustrate the argument made

Author: This point has been echoed in other comments, and is definitely an improvement to be made to the results section. We are constructing a segment on the bars observed within these experiments, as currently the only reference available is located in supplementary materials. To that end, photos of the bed surface and of the morpho-

dynamics will be included to display these processes and differences visually.

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