Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2019-23-AC1, 2019

Earth Surface Dynamics Discussions

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

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Received and published: 16 July 2019

The general comments from Reviewer 1 are presented below. We would like to thank the author of these comments for their substantial contributions to the reorganisation and restructuring of the paper. These comments were very useful in helping us improve the clarity of the message in this paper, and we believe will strengthen the arguments made within. First, general comments are presented and then followed by specific comments we feel appropriate to address at this time.

General Comments

Reviewer: This a personal opinion. The article's title could be changed to something more appropriate. When I received the article I thought that it was related to large chains in the sense of boulders or macro-roughness elements. Given that most steep channels do have boulders and other (actual) large grains, and those are neglected in this study, the title was misleading to me. Again, this is a personal opinion but please consider it if you think the same.

Author: This is an issue that had not occurred to us, but is useful to bear in mind. We had not considered the confusion that might arise from the overlap between our work and that in steep, jam-structure dependent streams.

Reviewer: The article structure does not convey the information in a fluid manner. The introduction has little information about aggrading systems and it seems to me that it gives more importance to degrading systems. Although, I understand that the idea was to make clear that we know more about degrading systems more information and references to what we really know about aggrading systems is required. There are virtually no references to any study that may have discussed aggrading systems.

Author: This feedback is vital for us; clarifying the issue and making sure the justification is present in a positive sense (i.e., what we actually know) is paramount for this study. As a result, we are working on re-structuring the introduction in order for the information to flow more smoothly. In addition, and in response to one of the specific comments, more background information and references will be added to ensure that the justification is presented in a clear and defensible manner. Furthermore, changes to the other sections will be made so the overall coherency of the article is maintained.

Reviewer: The article presents the study using Lane (1955) balance expression. Then, the assumptions of this expression are called into question and by doing so the hypothesis is formulated. The problem is that Lane 1955 did not consider a mixture of

sediment and therefore does not intended to explain the responses of different GSD, even when they have the same D50. Only later in the paper, in the discussion (page 10, line 4), this is explained. So, as a reader, I had problems trying to understand why this is not explained right away. The major concern about this is how the information flows in the article.

Author: Following from the previous comment, the section from the discussion will be relocated to the introduction to provide a more immediate justification for the study. Lane (1955) will still be introduced, but only as a tool to demonstrate the issues with using Church (2006) and the role of grain size distributions in bed material. We believe that this comment makes the mission statement clearer.

Reviewer: The hypothesis needs to be reformulated. I understand the idea of the study is to compare responses to different GSD and boundary conditions. This was well developed in the text. However, if I just take the hypothesis, it doesn't say that. "We hypothesise that, like degrading systems, the presence of the large grains will result in different transport regimes, as in MacKenzie and Eaton (2017), and thus different channel morphodynamics and depositional slope" It says that it is just the presence of large grains, what about boundary conditions? The article shows that is not just the presence of these large grains but discharge is a fundamental control.

Author: We agree that the hypothesis does not fully present the same ideas that this study addresses. Therefore, we will reformulate the hypothesis to better represent the information conveyed in the updated introduction, where we shall try to impress the importance of large grains upon the reader. In addition, the role of boundary conditions will be explicitly included.

Reviewer: A lot of information about bed structure, for example bars, is given by the end of the discussion. There is no data about this and only observations. This should be presented in a more formal way.

Author: This is a very important point that we will be integrating into the paper's re-

sults section. Whilst this data is qualitatively available in the video supplements, the addition of static images to this paper will greatly improve the present these ideas and observations through the discussion section.

Specific Comments

Specific comments were also provided, and we address the most relevant of the non-technical comments here.

Reviewer: 2) Abstract - line 4 - Is it correct to talk about "fan" if we are in a 1D system? The fan part is where the system spreads and here it does not occur.

Author: This system was designed to simplify a three-dimensional fan into a single slice that represents the system slope, like the experimental design of Guerit et al., 2014. So whilst the system is not fan-like, we believe it represents the fundamental interaction between surface organisation and slope in a manner that approximates self-formed deposits such as fans.

Reviewer: 3) Introduction - line 11 - There is one problem when we use the discharge as a variable to explain a certain response. If we double (or 3X, 4X, ...) the channel width while holding the discharge we may have different geomorphological responses. Therefore, is not actually the discharge, but some other characteristic (e.g., unit discharge) what is better for comparisons. This may be discussed somewhere.

Author: We used discharge as our system defining metric because it was the boundary condition we had control over in this case. Actual channel width varied in a manner that was not constant along the length of the flume, therefore specific discharge was avoided. Similarly water depths were unknown, so shear stress would not have been a useful metric.

Reviewer: 11) Methods - general - It would be really interesting to analyze the evolution

of the slope, that is, change of slope in time. I was wondering if the experiment came to a final equilibrium slope, or how do you decide to finish an experiment. Do we find the mean slope by the end of the experiment or by the middle of it. A simple plot would answer these interesting questions.

Author: Equilibrium slope was never explicitly reached, where the output matches the input rate of sediment; this value was approached but not used as a criterion in other studies (e.g., Eaton and Church , 2004). Instead, our experimental limit was set by the volume of sediment we could supply. We believed that the morphodynamics were similar enough over the course of the experiment that it was not undergoing a substantial flux. We will expand more upon the nature of the evolution of the deposits and also add a figure that shows the temporal pattern of the slopes presented in this study, if necessary.

Reviewer: 18) Discussion - Page 10 - Line 10 - It would be interesting to consider a little discussion about what may happen if we have the same D84 and different D50.

Author: We believe including this point, in the manner of the thought experiments of Parker and Toro-Escobar (2002), will complement the discussion section greatly. However, we will endeavour to limit our speculation as to the effects that these changes would have.

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