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Interactive comment

# *Interactive comment on* "Controls on the hydraulic geometry of alluvial channels: bank stability to gravitational failure, the critical-flow hypothesis, and conservation of mass and energy" *by* Jon D. Pelletier

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## 1 Summary

The manuscript presents a combination of approaches to identify the controls on the hydraulic geometry of alluvial rivers. It includes hydraulic and geotechnical considerations which is, in my opinion, a good and relevant approach and likely to improve our understanding of bankfull geometry controls. The manuscript first presents a

geotechnical-based approach to compare the maximum stable height of a riverbank and compares the results with over 300 data points obtained from the Mississippi River Basin. It then analyzes data from the literature to establish correlations between bankfull width and depth as functions of bankfull discharge. Finally, the analysis presents two equations to predict along-channel slopes for sand-bedded and gravel-bedded rivers and compares them with observed values. In general, the manuscript shows good agreement between predicted and observed values. The author closes by recognizing some of the shortcomings of the approach and highlighting research needs to improve our understanding of bankfull geometry controls.

## 2 General comments:

After carefully reading the manuscript, I have the following comments, which I hope the author will find relevant and useful.

1. The use of the data by Dafalla (2013) seems misrepresented in this manuscript (Figure S1). Cohesion values reported by Dafalla (2013) correspond to pure sand, sand and clay mixtures with 5

In addition, Dafalla (2013) shows that for the same clay content (15

I really believe that the approach proposed in this manuscript has a lot of potential and others have included geotechnical considerations in models for stream restoration (e.g. CONCEPTS, see Langendoen et al. 2001; RVR Meander, see Motta et al. 2012). I would encourage the author to dig out some more references regarding cohesion estimates for soils that are more relevant for the Mississippi River Basin. For example, Masada (2009) presents a very extensive report on geotechnical parameters for the state of Ohio and includes different relations between sediment properties (cohesion for example) and soil composition (amount of silt, amount of clay, etc.). Their approach is specific to highway embankments but the results of their tests might be more general in terms of cohesion values in relation to clay contents.

2. The use of equation (2) might not be appropriate for riverbanks. The use of that equation as presented by Chen (1969) and Terzaghi et al. (1996; p. 271-272) is for soil embankments located above the water table. Several authors have used it in the past as discussed by ASCE (1988) but even there, the authors suggest that critical depth approaches are not accurate when the most common bank failure mechanisms for riverbanks are due to tension cracks that cause toppling or cantilever failures.

Assuming the equation is indeed an appropriate approach for riverbanks, I would encourage the author to explore the sensitivity of its input variables to other values. Chen (1969) shows a wide range of Ns values that depend on the internal friction angle of the material (which is sensitive to moisture content) and the actual slope of the bank. The smallest stable bank height would be given by the smallest possible safety parameter Ns so why not explore a range of Ns values. When the channel has low flow, the bank might be quite dry and its maximum stable height would be quite different from that obtained with a saturated bank (e.g. during the falling limb of a hydrograph where the river stage is getting lower but the bank remains saturated). It would be very useful to see these considerations in the analysis. The author discusses the issue briefly but more details regarding bank failure mechanisms and their prevalence might strengthen the manuscript.

3. Sensitivity analysis: Figures 2b and 2c present results for bank heights based on a synthetic dataset. If the author estimated clay contents using averaging windows for a soils dataset, why not extract second order statistics from it and use them directly instead of creating a synthetic dataset?

4. Use of the Mississippi River Basin data: The author clearly states why the MRB data are used. However, not knowing much about the many different locations along the basin, I have a few questions. (1) What percentage of the cross sections analyzed can be considered natural? (2) Did the author discard those locations where the navigable

channels are maintained by the US Army Corps of Engineers? (3) Of the many stations used, how many might be influenced by river control structures (dams, wing dams, chevrons, etc.) or road infrastructure (e.g. culverts, bridges)?

5. Figures 3 and 4: It is not at all clear why the author includes regression plots of the Dunne and Jerolmack (2018) dataset. Based on the abstract and introduction, it was unexpected that a different dataset appears in the manuscript and becomes the focus of the second half. I understand the use of the dataset for Figure 5, which is new but the content of Figures 3 and 4, is not. I would encourage the author to make it clear to the reader earlier that the DJ dataset is a substantial part of the analysis and to state explicitly the novelty of including figures 3 and 4.

6. Figure 5: I have a few specific questions about the analysis leading to Fig. 5. (1) What is the number (and percentage) of cases that report ripples/dunes over the entire Ohata (2017) dataset? (2) For those reporting ripples/dunes, what is the number and percentage of measurements obtained in the laboratory and in the field? (3) For those in the field, how many are for large rivers? Cisneros et al. (2020) show that traditional dune scaling equations overestimate the size of dunes in large rivers and propose the following relation between dune height (H) and water depth (h) – H  0.056h - 0.12h. (4) Are the only sources of roughness in the DJ data the ripples/dunes or gravel size? What about bars, meandering, vegetation?

7. As a final general comment, I was hoping to see more analysis on the Mississippi River Basin dataset and comparisons between it and the DJ dataset where possible. The manuscript seems to be split between two separate analyses but the abstract and introduction do not suggest that. I recommend the author to modify these initial sections as necessary and compare the MRB data with the DJ data where possible. What kind of relation does the author obtain between bankfull depth and bankfull discharge for the MRB under the geotechnical considerations? On the other hand, could clay contents (and cohesion) be estimated with a revised version of equation (4) for other rivers in the world where soil data is not readily available?

## 3 Specific comments:

I list a few specific comments here. Some relate to clarification, others to typos and the last one is a personal opinion, which the author is free to disregard.

1. How do the bankfull estimates found here for the MRB compare to those of Dong et al (2019). This reference appears in the introduction but is not mentioned in the discussion. 2. I did not understand the fourth criteria used to keep a USGS gaging station in the analysis of the MRB. 3. If the analysis discards rivers with depths smaller than 2m, why is the 0.5m to 1.5m soil depth the only section considered for the analysis. What about river sections with different bank layers? The author mentions that soil data below 1.5m is not reliable but how valid is it to assume a uniform soil profile for the entire channel depth? How sensitive is the proposed model to this assumption? 4. Line 150(and other locations) - Ohata et al 2017 (not 2019). 5. Line 151 – How did the author "cross-reference" the Ohata et al. dataset with the Dunne and Jerolmack dataset? 6. Line 214 – what is the equation of the curve (envelope) used to identify the conditions conducive to dune/ripple development? 7. Line 259 (and other locations) – Chen 1969 (not 1971) 8. Line 286 – Vegetation. What are the predominant types of vegetation along the MRB? How deep are their roots? Root length might set slump block thickness. Vegetation might be the most relevant factor in shallow channels (up to max root length) and geotechnical considerations might be more relevant in deeper channels where roots might not stabilize the full bank. 9. I think that box plots might look better than the grey areas used by the author to summarize results within certain bins in the different plots.

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