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ESurfD

Interactive comment

Interactive comment on “Short communication: Field data imply that the sorting (D_{96}/D_{50} ratios) of gravel bars in coarse-grained streams influences the probability of sediment transport” *by* Fritz Schlunegger et al.

Anonymous Referee #2

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Summary: This paper conducts Monte Carlo simulations to determine the likelihood of bed material mobility of D84 at mean annual flow (maf) in 35 gravel bedded rivers in Switzerland and Peru. The authors find that the probability of gravel mobility varies with the ratio of D96 to D50, such that D84 is less likely to be mobile at maf in channels with more uniform grain size distributions when compared to wide grain size distributions. This is an interesting finding, which should be shared. However, this paper needs substantial work before it is ready for publication.

Intro/general 1. I found the framing in the Abstract and Introduction confusing. Some

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Discussion paper

of this is due to imprecise wording: - The authors talk about the "mobility of gravel bars", but their work is actually focused on the mobility of individual grains of sediment. While bedform migration does require bedload transport, the work presented here never deals with morphologic change. I suggest the authors re-word. - The authors use the phrase "sediment flux" (ln 24) and "sediment discharge" (ln 12) where "sediment supply" would be a more appropriate choice. - In the title (and throughout the text), the authors suggest that grain "sorting" influences the probability of sediment transport. Isn't it just as possible that the causation runs the other direction? (Sorting reflects sediment transport conditions, as controlled by sediment supply?)

2. Many of the ideas presented in the hypothesis go unaddressed in the paper. As I read it, the hypothesis (which begins at Ln 27) is that high sediment supply channels tend to be braided, with high bed material mobility, while low sediment supply leads to single threaded channels with armored, well-sorted beds with lower bed material mobility. However, this manuscript presents no data on sediment supply or armoring. Furthermore, the Swiss channels are not (necessarily) naturally single-threaded channels. I suggest that the authors re-frame their hypothesis so that it is testable with the data presented in the manuscript.

3. The motivation for this study seems a little fuzzy (gravel mobility is important because of bar mobility?). Given that the main findings relate to the mobility of D84, the authors could consider using Mackenzie and Eaton (2017 and 2018) as motivation.

Methods: 4. Channel width was measured from aerial photographs, but I am not sure what width this refers to. Is this the full bank-to-bank channel width? Or the active width at mean flow? Given that mean flow tends to be much lower than the channel-filling flood, this difference has the potential to matter quite a bit for the results. Because flow depth is estimated using Qmaf, the width used in calculations should be Wmaf.

5. Some of the reported grain sizes are surprisingly small, especially when compared to the images in Litty and Schlunegger (2017). The authors state that "in cases where

more than half of the grain is buried, the neighboring grain was measured instead". This reviewer suspects that this method leads to a bias toward measuring small grains. Large grains are more likely to be buried. (And how do you know if it is "more than half buried"?). Do the authors have any field measurements of grain size to support their "photo sieving"?

6. Peruvian channels have VERY high standard deviation of Qmed. Are these calculated as the standard deviation of the individual years mean annual flow? How many years of Peruvian channel data are there in the dataset? The high stdev of Peruvian channels compared to Swiss channels suggests a very different flow regime. Is it possible that the differences the authors find between Swiss and Peruvian channels is a reflection of flow regime, rather than sediment supply (as they seem to imply)?

Results/Discussion: 7. I am surprised by the finding that D84 is mobile at mean annual flow in near 100% of the simulations in many of the Peruvian channels. That is an extremely mobile bed! Consider that other researchers have predicted D50 mobility of only ∼12% of the year (Torizzo and Pitlick, 2004), and that in some regions, bed mobility is exceeded only a few days a year (Pfeiffer and Finnegan, 2018). The authors should consider making comparisons to these previous findings in their discussion.

I have focused my comments on content, rather than prose. There were several grammatical errors (e.g. Lines 33, 91) and awkward sentences throughout the manuscript. I suggest that the authors give the next version a more thorough read before resubmitting.

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