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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.

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Anonymous Referee #3

We thank referee 3 for the very helpful comments, which we have considered. Please find below an explanation of how we have addressed the open questions.

Reviewer: This manuscript addresses the question of whether bed sorting reflects transport rates of the bed material using a compilation of grain size measurements from (mostly?) gauged stream reaches and Monte Carlo simulations of flow competence.

The dataset is useful and warrants eventual publication. However, the analysis does a poor job of testing the hypothesis, given what we know about thresholds for motion in gravel- bedded rivers.

Our response: We have focused the introduction and better specified the hypothesis to be tested. The thresholds for motion of coarse-grained sediment particles are set by assignments of values to the Shields variable φ. As the reviewer indicates, there is a large body of literature on this topic. However, most analysis concur on the notion that a range of φ-values between 0.03 and 0.06 adequately constrains the threshold conditions for sediment entrainment along streams where energy gradients are between 0.001 and 0.03 (please see compilation by Lamb et al., 2008), which is the case in our setting. We explained this point more carefully in a new section where the Monte Carlo simulation is described. We additionally add a new section where we justify the assignments of uncertainties to the variables.

Reviewer; Moreover, the framing of the argument and the interpretation needs some improvement to help the reader understand the purpose and findings of the study.

Our response: This has been done. The introduction has been sharpened and more clearly focused.

Reviewer: Below, I list a number of concerns related to the analysis presented in this manuscript, followed by suggestions to improve the presentation. Finally, I list some comments by line number, some of which echo or substantiate my main concerns/suggestions.

Reviewer: Concerns with analysis: Transport probabilities are estimated via a Monte Carlo ap- proach, where "mean annual" shear stress is compared against the threshold stress for motion (i.e., is tau>tau_c?) for 10,0000 simulations for each site. Some of the variability between simulations results from measured standard deviations in mean annual discharge. However, any real transport variability that results from variability in water discharge is contaminated by synthetic variability in the other parameters (width, slope, critical shear stress, and grain size). The standard deviation of these parameters (except for tau_c) is set to 20% for all sites, without any justification.

Our response: This point has also been made by reviewer 1. We have added a new chapter where we discuss the uncertainties, and how we have estimated them. We present additional material in the new Supplement.

Reviewer: The authors should justify their use of a mean-annual discharge. Why not use bank- full or channel-forming discharges, which typically occur every 1.5 or 2 years? Or, if the goal is actually to quantify the fraction of time that the threshold shear stress is exceeded, wouldn't it make the most sense to just estimate a threshold discharge for each site and count the number of flow measurements that exceed that discharge?

Our response: Reviewer 2 rose a similar concern, but from a slightly different prospective. We have addressed this point in the following way (please see also see response to reviewer 2): we ran a test, where we explored how the transport probability changes if we take a series of models for various runoff quantiles and calculate the resulting probability of sediment mobilization accordingly. We then multiplied the probability of occurrence of each quantiles (listed by the Swiss authorities and calculated for the Peruvian streams based on daily records) with the corresponding transport probability and sum the values. This integration provides an estimate of the 'real' probability of transport and accounts for the non-negligible temporal variability of water discharge (up to 3 order of magnitude for some Peruvian rivers). The analysis shows that this new estimate is positively and linearly correlated with the probability of transport estimated with the mean annual water discharge Qmean. In addition, these correlations are very similar between the Swiss (slope: 0.74±0.02; intercept: 0.05±0.01) and the Peruvian streams (slope: 0.73±0.19; intercept: 0.03±0.14). This means that the positive relationships between grain size sorting and sediment transport probability remains for both settings. We present the results of this test in the Supplement S3. Nevertheless, because Qmean is commonly selected in a large number of studies mainly because these data can be readily extracted even for very remote areas (e.g., calculation via TRMM data), we decided to keep the results of the model run based on Qmean in the main text and to illustrate the alternative solutions in the Supplement S3.

Reviewer: To estimate bed shear stress from discharge measurements, the study relies on a Manning's-n flow resistance calculation, in which Manning's n does not depend on any roughness length scale (e.g., D50 or D84). While this approach has been shown to compare favourably to measured flow velocities in some cases (Jarrett, 1984; Ferguson, 2007), it strikes me as an odd choice for this study, which is focused on the relationship between bed sediment size and transport conditions. Would including sediment size in flow resistance calculations affect the results?

Our response: We did explored whether equation (2) could be solved using the Darcy-Weisbach friction factor f instead of Manning's n. According to Ferguson (2007), the friction factor f varies considerably between shallow- and deep-water flows and depends on grain size D relative to water depth d, and thus on the relative roughness. Ferguson (2007) developed a single equation to compute f, referred to as the Variable Power Equation (VPE), that considers roughness-layer and skin friction effects. Calculations where the VPE was employed indeed revealed that roughness-layer effects have an impact on flow regimes where D84/d>0.2 (Schlunegger and Garefalakis, 2018), which is likely to be the case in our settings. However, similar to Litty et al. (2016), we are faced with the problem that we have not sufficient constraints to analytically solve equation (2) with the VPE. We therefore selected Mannings's n instead, which allows to solve equation (2) analytically. We have addressed this point in the revised manuscript. Reviewer: Finally, I'm concerned that the analysis underestimates variability in the critical Shields stress and ignores the covariability between critical and bankfull Shields stresses. The critical Shields stress for bed motion varies by more than an order of magnitude between sites (e.g., Phillips and Jerolmack, 2019). Moreover, that study (Phillips and Jerolmack, 2019) makes a compelling case that the best predictor of critical Shields stress is the bankfull Shields stress. This suggests that the inferred differences in excess shear stress between sites may not even exist. This is impossible to test without measurements of bedload transport (which I assume is outside the scope of this study), but this potential issue with the approach should be acknowledged.

Our response: Yes indeed. In our case we cannot really constrain the bankfull runoff conditions, but we consider the range of Shields stresses (please see compilation by Lamb et al., 2008) that are appropriate for most channel gradients between 0.001 and 0.02 as is the case in our setting. We discuss this point and justify our selection in the revised manuscript, and we also refer to the Philips and Jerolmack (2019) and Turowski et al. (2011) in this context and discuss the outcomes.

Reviewer: Suggestions to improve clarity: Causality needs to be consistent throughout the paper. The title of the paper states that sorting influences transport (this causality is repeated in the conclusions, lines 174 to 177). However, the mechanistic argument stated in the intro (lines 28 to 32) is that lower bed mobility leads to enhanced armoring (winnowing of fines), which is expressed as differences in bed sorting. In reality there is likely a feedback between the two and causality in both directions, but the paper, in its title, intro, and conclusion, should at least be internally consistent.

Our response: We rephrased the introduction to better explain the focus of our paper. Feedback mechanisms between sediment supply, sorting and transport probability are discussed towards the end of the paper.

Reviewer: The mobility of bars (which are bedforms) is used as the main motivation of the paper in the introduction and is mentioned several times in the short abstract, but this paper does nothing to discuss the mobility of bedforms. Please revise these sections to match the actual subject matter of the paper, which seems to be the extent to which the transport of bed material sorts that bed material.

Our response: We have clarified this point. We actually refer to the mobility of individual clasts on gravel bedforms. This has been clarified.

Reviewer: Line comments 21 – 23. I don't think this reference suggests that the mobility of bars affects channel form.

Our response: The introduction has been changed. Please see responses above.

Reviewer: 39. It's unclear to me what mean-annual discharge is and why it's used. Is it the average discharge for the entire record? Or the mean of the maximum annual discharge? I assume that it's the mean of the entire record, in which case there is nothing "annual" about this measure.

Our response: In Switzerland, the mean annual water discharge corresponds to the arithmetic mean of the means of the annual flows, measured over several decades (c. 20 years). We present the data in the Supplement S2. In Peru, the case is more difficult because the gauging stations for many streams are situated upstream of the grain size measurement sites. Reber et al. (2017) have discussed of how the gauging records have been scaled to the sites where the grain size data by Litty et al. (2017) were collected. In this work, we present hydrological data for those 7 sites where the gauging stations are close to the grain size surveys (Supplement S2). We additionally performed sensitivity tests to explore the consequence on sediment transport probability for the case where runoff quantiles are used (Supplement S3). The patterns and conclusions remain the same.

Reviewer: 45 – 46. So you avoided braided channels, but the intro suggests comparing braided and single-threaded channels is the main motivation of the study.

Our response: In the Peruvian streams, the single-threat reaches are only a few hundred meters long (compared to the several kilometres in Switzerland where channels are all confined by artificial banks), which is actually very short if the overall braided character over tens of kilometres is considered. We have clarified this point in the revised manuscript.

Reviewer: 66 – 68. It doesn't "account" for slope dependency by bracketing a certain range of values, and Lamb et al. (2008) show that the slope dependency extends to the lowest- sloping gravel bed rivers.

Our response: Yes indeed, it doesn't. However, this range includes most (admittedly not all) of the reported values for stream segments where energy gradients are between 0.001 and 0.02, as is the case in our setting (Lamb et al., 2008). We discuss this point in the revised manuscript.

Reviewer: 88. There is no roughness length scale in this roughness calculation.

Our response: Indeed, this variable is not considered because we don't have data as constraints. We mention this in the revised manuscript.

Reviewer: 106-108. How can you be sure that you're measuring the b-axis in a 2-D image? If assuming that the short axis is vertical, this should be stated.

Our response: We indeed infer that the shortest axis is vertical. We mention this in our revised manuscript.

Reviewer: 109-110. By only counting the large particles once, you systematically underestimate the areal coverage by coarse particles. The correct solution to this is to choose a grid with a spacing that's larger than the largest grain.

Our response: That's what we have done. In some very few cases where some grains were larger than the spacing of the grid, then we proceeded as described above. We have specified this point in the revised manuscript.

Reviewer: 119 – 124. Is there any justification for the 20% uncertainty?

Our response: We present a new chapter where we discuss and justify the selected uncertainties (see also response to the major comments by reviewer 1).

Reviewer: 126 – 127. Critical Shields stress at a site varies much more than this (see Turowski et al., 2011 for example).

Our response: Yes indeed. The selected φ-range between 0.03 and 0.06 includes most published data about the slope dependency of the critical φ where channel gradients were between 0.001 and 0.03 (which includes the energy gradients of our streams, Table 1), as a compilation by Lamb et al. (2008) has shown. We acknowledge that field-based data by Turowski et al. (2011) has shown that critical conditions for sediment entrainment cover a much larger spread than employed here. However, the four reaches that were analysed by these authors were at least five times steeper (energy gradients between 0.06 and 0.1) than the Swiss and Peruvian streams considered in this paper, with the consequence that sediment entrainment in these streams most likely requires the condition of φïĂ■values that are greater than 0.06 (Lamb et al., 2008). This is discussed in the revised manuscript.

Reviewer: 130-132. Maybe I missed it, but are these streams also gauged? Are the data similar in quality and duration to the Swiss data? It seems worthwhile to state this since the results are so different between the two study areas (e.g., Fig. 3).

Our response: Yes, these streams have been gauged on a daily basis. 7 gauging stations are close to the sites where grain size data has been collected. The other gauging stations are further upstream. Reber et al. (2017) used the records of the 7 aforementioned stations and TRMM data to scale the gauging data from the other streams to the sites where the grain size data has been collected. This has been explained and discussed in detail in Reber et al. (2017) and Litty et al. (2017). Therefore, we decided not to repeat their statements, but we present water discharge data from the 7 gauging stations in the Supplement S2.

Reviewer: 143. Is Qmed the mean or median? This is confusing.

Our response: It is Qmean. We have corrected this error.

Reviewer: 156. It seems very difficult to say that the relationships in Figure 3 are linear.

Our response: Following the request by reviewer 1, we present the normalized residuals against the sorting. This plot shows that the normalized residuals do not show any specific and significant patterns, and they are therefore independent on the sorting. This suggests that the inferred linear relationships between the probability of transport occurrence and the D96/D50 are statistically robust.

Reviewer: 157. I'm missing the justification for why the switch to D96 when D84 was stated to be the grain size of interest (line 69).

Our response: We could have selected the D84 as well, and the results would have been the same (see also Figure 2 that illustrates that the grain size distributions are self-similar). We decided to take the D96 instead of the D84 because the ratios to the D50 are larger, and differences in sorting patterns will become clearer.

Reviewer: 159. Bedload is not measured; D96/D50 is for the bed sediment.

Our response: Yes indeed, we measured the bed material. This has been corrected.

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