

## ***Interactive comment on “Uniform grain-size distribution in the active layer of a shallow, gravel-bed, braided river (the Urumqi River, China) and implications for paleo-hydrology” by L. Guerit et al.***

### **Anonymous Referee #1**

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Guerit et al. present a field study from the Urumqi River, China in which they compare different methods of grain-size analysis including (i) horizontal surface counts over the whole river width, (ii) vertical surface counts on an outcropping trench wall and (iii) volumetric counts (sieving) of a 1 m deep trench excavated within the dry channel-bed. As they found no differences in sub-sample grain-size distributions in vertical nor horizontal direction within the trench, they propose that the grain-size distribution is uniform within the active layer, which might be a typical phenomenon for non-armoured gravel-bed, braided rivers. Second, they found no difference between the volumetric

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grain-size analysis and the vertical surface counts within the same trench. They conclude that the surface point count method, which was originally developed by Wolman for horizontal surface granulometry analyses in active rivers, can also be applied to vertical outcrops.

Temporal variations in grain-size distribution are used to reconstruct paleo environmental conditions including climate and tectonics. As such, it is important to investigate the differences between methods that are commonly applied to characterize grain-size distributions. As this study performs a very systematic comparison of three of those methods in a natural, gravel-bed, braided river, and we generally lack those systematic method validations, the study is a valuable contribution to the community. The three presented methods to measure grain-size distributions in the field are commonly applied in other studies. The methods are well explained and carefully performed in the field. The paper is clearly written and I recommend to publish the manuscript in ESurf. However, I have some comments regarding the statistical analyses, the presentation of the data, the structure of the manuscript and the extend of the discussion.

The aim of the manuscript is to compare grain-size distributions. As such, statistical tests to investigate if distributions are different from each other or indistinguishable, are mandatory. One example is on page 5 line 28, where the authors state that the grain-size distribution in the surface layer is indistinguishable from the layers below. This statement needs to be supported with a statistical test. Another example is on page 7 line 1, where the authors report that above a threshold of 10000 kg the D50 and D90 are equivalent to the whole trench. I think that the identification of such a threshold should be based on statistical analyses. Calculating a moving mean and the according standard deviations and test when means become indistinguishable is one option. In addition, the measured grain-size distributions are only presented as cumulative density functions (CDF) in fig. 6. When plotted as CDF, differences in distributions are hard to detect by eye. For better comparison of the distributions, the probability density functions (PDF) and quantile or percentile plots should be added.

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I think the structure of the manuscript is lacking a clear separation between the Methods, Results and Discussion sections. The Method section should be a clear description of the applied techniques, but should not contain any references to measured data. The Results section should be a neutral description of the data without any interpretation of it. I advise the authors to carefully check the manuscript and clearly separate method description, results description and interpretation. Below, I have listed a few points where the mixing was obvious to me:

- p. 3 lines 11-15: To me, these sentences belong to the Results, Discussion and Conclusion section.
- p. 4 lines 4 – 16: This paragraph mixes the methodological descriptions and results. The description and reference to Fig. 5 is part of the results section.
- p. 5 line 10: Same here, the reference to Fig. 5b belongs to the Results.
- p. 6 line 6-8: This is more than just the description of the result, and should be moved to the discussion.
- p. 6 lines 16-21: From my perspective, this entire paragraph belongs to the discussion section.
- p. 7 lines 2-3: The last sentence of this paragraph is discussion and not a description of the results.
- p. 7 lines: 8 – 11: These sentences belong to the discussion.

The authors clearly state in the Introduction that they test two hypotheses, namely the investigation of granulometric uniformity within the active layer and the application of surface point counts developed for horizontal layers on vertical layers. And both of these hypotheses are discussed later. However, the authors perform three different grain-size analyses. Currently, to verify or falsify their hypotheses, they only discuss two of them in detail, which are the volumetric analysis and the surface analysis on a vertical section in the trench. I think the paper would benefit from expanding the dis-

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cussion about the reach-scale surface counts. As the authors state in their manuscript, vertical surface analyses are applied in paleo-studies. But these measurements are often compared to modern channel measurements, in which case a vertical surface count is compared to a horizontal surface count. In their study, the authors show that horizontal reach-scale surface count results in a coarser distribution than the vertical surface count from within the trench (Fig. 6d). Why is that? And what implication does this observation have for field studies that compare vertical with horizontal (or paleo and modern) grain-size distributions? I think it would be a missed opportunity to not extend the discussion (and maybe add a third hypothesis accordingly). However, if the authors decide to not include it, the third method (horizontal clast counts) can be removed from the paper.

An important point of the paper is that the investigated gravel-bed river has no armour layer and thus, any conclusion drawn from the findings are restricted to non-armoured channels. This restriction is mentioned in some parts of the paper, but not consistently throughout. From my point of view, this restriction needs to be mentioned in the abstract and potentially even in the title of the manuscript. Further clarifications of this restriction needs to be added to the sentence page 6 lines 9 – 11 and in the Conclusion (page 8 lines 27 - 31), which are currently phrased too generalized.

The abstract is currently fairly short. As an abstract serves as a stand-alone summary of a paper, the abstract could be extended by clearly listing the two hypotheses, the results and the according conclusions.

An important difference between this method-testing study and an applied study is that the analyses in this study are performed on a modern and active channel-bed. In paleo-studies, the vertical grain size measurements are applied to deposits that are thousands, sometimes millions of years old. I think it would be useful to mention within section 5.2 (page 8 lines 1 - 19), that the grain-size distributions in sedimentary deposits can also be altered after their deposition/ abandonment. Desert pavements, for example, can form in arid or semi-arid environments. Aeolian processes form a

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coarse gravel layer of interlocked clasts at the surface, underlain by a layer of very fine material [e.g. McFadden et al., 1998]. Processes like this should be taken into account when applying the vertical sampling strategy to paleo-deposits. Other examples of post-depositional alterations include soil-production or bioturbation.

McFadden, L. D., E. V McDonald, S. G. Wells, K. Anderson, J. Quade, and S. L. Forman (1998), The vesicular layer and carbonate collars of desert soils and pavements: formation, age and relation to climate change, *Geomorphology*, 24, 101–145.

As this study compares different approaches and analyses, and aims to improve the reliability of characterizing grain-size distributions in the field, it would benefit from including the raw data of the field measurements as a supplementary file. That allows the re-analysis of the data for future studies.

The following points are minor comments only:

p. 2 line 8: “. . .at a reach scale. . .” I think this sentence needs some further explanation, maybe include rough dimensions or explain the term ‘reach’.

p. 2 line 20: Please clarify in the second part of the sentence that the thickness of the active layer corresponds to the maximum elevation difference within a cross section and not in the downstream direction.

p. 3 line 6: D’Arcy et al. did not sample a vertical section, but the grain size distribution on the surface of an alluvial fan. Same accounts for p. 8 line 7.

p. 4 lines 26-27: List all sieve sizes used for the analysis, not only the minimum and maximum, since the size step can potentially affect the resolution of the datasets.

p. 5 line 27: Remove the extra comma and space after 0.2 m.

p. 6 line 9: Maybe clarify in the title that you compare the volumetric analysis to the horizontal surface counting and not the vertical surface counting.

p. 7 line 6: The vertical counts are not only shown in fig. 6d, but in all four graphs of

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fig. 6.

p. 8 line 22: For clarification the authors could add ‘horizontal and vertical’ surface counts and volumetric samplings, . . .

p. 8 lines 20: As it is written now, the following paragraph is more a summary than a conclusion, so I suggest to adjust the title of this paragraph.

Fig. 4. It would help the reader to add length information to the pictures a, c and d.

Fig 5. Although stated in the figure caption, the figure does not really show uncertainties, but rather variability. How is the inherent variability defined? It would help to explain this at least in the figure caption. The combination of red and green colors (fig. 5b) is invisible for everybody suffering from red-green blindness.

Fig. 7 (caption). ‘dashed’ line instead of ‘dotted’.

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