

# ***Interactive comment on “Armor breakup and reformation in a degradational laboratory experiment: detailed measurements of spatial and temporal changes of the bed surface texture” by C. Orru et al.***

## **Anonymous Referee #1**

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## SUMMARY

This paper examines how the texture of a mixed sand-gravel channel bed responds to changes in flow discharge. It is qualitatively interesting to see the formation of static armor during high flow followed by breakup of this armor and reformation of a coarser mobile armor during subsequent high flow. Furthermore, application of new techniques for repeated longitudinal profiles of grain size and bed elevation offers new quantitative insight into armoring processes.

While some of the observations are interesting, I think they are insufficient to merit

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publication. Though it was very long (20 h), only one experiment was run. Therefore, it is impossible to determine the reproducibility of these results or the dependence of the armor formation and breakup processes on the specific initial bed configuration, pattern of flow changes, and trimodal bed texture. Additional experiments to vary at least one of the experimental variables (e.g., magnitude of low and high flow) would provide much more insight into the controlling factors of bed armoring.

Furthermore, the manuscript is poorly organized and needs to be fundamentally re-structured. The abstract and conclusion are nearly identical and do not establish the motivation and implications for this work. The introduction is very disorganized and does not follow a logical progression in presenting information about past work. For example, the opening sentence of the paragraph at line 18 on page 1 seems unrelated to the remaining content in the paragraph. Some information in the methods (section 2) really belongs in the results (section 3); e.g., section 2.3. In the results, there are several assertions made without proper explanation. For example, on page 4, line 14-15, an “imbricated structure” is mentioned, but there is never any discussion of when the imbrication developed or what morphological features suggest this interpretation. Related to this, there are several interpretations in the results (section 3), which really belong in the discussion (section 4); e.g., section 3.2, lines 9-10. Finally, all figures should be mentioned in the manuscript (currently Fig. 2 is missing), and the figure numbering should correspond to the order of mention (currently Fig. 10 is mentioned before Fig. 6).

Given these concerns, I therefore recommend rejection for this manuscript. I encourage the authors to run additional experiments, then more clearly present their results for future publication of this work. Further minor comments are listed below.

## MINOR COMMENTS

1. In the abstract, the phrasing, “trimodal mixture composed of sand and gravel,” implies that there are three components, but only two are mentioned here. It would be

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better just to list the three sizes (1, 6, and 10 mm) here.

2. Page 3, line 13-17. Why do you impose a stepwise fining pattern in the initial bed? How does this reveal the dynamics of static and mobile armor formation better than a uniformly graded bed? Please explain in the manuscript. Also, the description of patch lengths is confusing. You should refer here to Fig. 2, which is never mentioned in the text.

3. Section 2.2 on grain size techniques is insufficient to understand these techniques. Unless you have a strong reason to delve into the details, I suggest summarizing this to one or two lines then referring to Orru et al (submitted 2015) for further information. I also suggest removing Figures 3 and 4 for this reason.

4. In describing the armor breakup and reformation, it is very difficult to see these bed changes from image to image in Figure 7. Could you apply some kind of image differencing technique to make the changes more apparent? The quantitative information in Figs. 8 and 9 is much more useful. Finally, unless you can justify them with more quantitative information, I would suggest removing the assertions on page 4, line 27-29, about textural changes between grain size analyses.

5. The methods used for characterizing the bedload transport rate from the front propagation are confusing (page 5 and Fig. 11). Where is the front being measured, in terms of the streamwise coordinate? Is there really no observed transport here prior to the step increase in discharge (as indicated in Fig. 11), or are you just assuming this?

6. At page 5, line 26, it is not clear whether the mentioned bed load transport rate is referring to the propagating front bed load transport or some other measurement. It is curious that a bed load trap is mentioned on page 2, lines 32-33, but then never again mentioned in the paper.

7. Finally, I am a bit concerned that no mention of the shear stress or Shields parameter is made here, despite the fact that this is usually considered an important variable in

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studies of the evolution of mixed sediment surfaces and armoring (e.g., Wilcock and Crowe, 2003).

## REFERENCES

Wilcock, P.R., Crowe, J.C., 2003. Surface-based transport model for mixed-size sediment. *J Hydraul Eng* 129, 120–128. doi:10.1061/(ASCE)0733-9429(2003)129:2(120)

Interactive comment on *Earth Surf. Dynam. Discuss.*, doi:10.5194/esurf-2016-1, 2016.

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