

Earth Surf. Dynam. Discuss., referee comment RC2
<https://doi.org/10.5194/esurf-2021-69-RC2>, 2022
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Comment on esurf-2021-69

Anonymous Referee #2

Referee comment on "Effect of debris-flow sediment grain-size distribution on fan morphology" by Haruka Tsunetaka et al., Earth Surf. Dynam. Discuss.,
<https://doi.org/10.5194/esurf-2021-69-RC2>, 2022

Reviewer Report of Tsunetaka et al 2021;

The effect of debris-flow sediment grain size distribution on fan forming processes

In this manuscript the authors used a flume to analyse how the grain size distribution of a debris flow may impact the morphology of the resulting fan. The authors ran 2 sets of experiment runs in their flume, all parameters inside the experiments were kept constant except the grainsize distribution. One set of experiment runs used monogranular sediment while the other used multigranular. Both sets had the same average grain size so the authors could identify the impact of variations in grain size on the debris flow fan. They quantified these changes by measuring the surge height in the flume, the speed of the surge within the deposition area, the runout distance and a DEM of the final debris flow. The authors discovered that multigranular debris flows were more likely to produce alluviations in the debris flow resulting in asymmetrical fans. They postulate that these alluviations are the result of grain size segregation occurring within the flow where coarse sediment is forced to the front of the flow where it can produce an obstacle for any following surges.

I think this study is an interesting addition to the literature on debris flows. The experiments seem well thought-out and the results aim to fill a clear knowledge gap. However, there are several areas where I feel the manuscript needs to be improved before it is ready for publication. The manuscript is very short and as a result I feel that significant detail is missing, particularly from the description of the experiment design, results and discussion. I also found the figures poorly cited and discussed throughout. I have provided more specific comments below for the authors to read through. I hope the authors find my comments useful and I look forward to seeing the revised manuscript.

Detailed Comments:

Introduction

The introduction is too short and vague to be of use to the reader. Despite the research statement at the end of the section it is not completely clear how the authors see the study contributing to the literature. It is also not obvious from these paragraphs why the authors have chosen to focus on grain size distribution for this study rather than many of the other controlling factors highlighted here. Finally, this section would be greatly improved if there was better separation between discussing debris flow physics from debris flow fans. Currently it is very confusing whether the authors are referring to how a certain parameter might affect a debris flow or how it may affect the autogenic fan forming processes or the links between the two.

Line by line comments:

Lines 25 – 26: What specific climate and sediment dynamics information can be identified from debris flow fans? And how is it derived? How is the form (which is what is investigated here) important?

Lines 26 – 29: If debris flow fans are primarily formed by autogenic processes how can information on any external forcing be derived from them?

Lines 31-33: Shifting the focus of the paragraph from a geological perspective of fans to one about hazard is confusing to the reader particularly as neither focus is well covered.

Lines 34-36: It would be useful to the reader if the authors would elaborate on how these physical factors affect morphology and stratigraphy of the fans.

Lines 36 – 38: From this section the reader cannot tell how these changes will affect the debris flows. The authors do not define the property that is changing carefully nor do they describe the impact of these changes on debris flow behaviour. Without this information the readers can not make the link between debris flows and the resulting fan.

Lines 38-43: Again, the links between debris flow behaviour and resulting fan form are not well made. Further, the terms functional and structural changes are poorly defined and it is not obvious how these affect autogenic processes, which are also not defined.

Lines 44 – 48: This is a good succinct research statement however it is completely disconnected from the preceding 2 paragraphs. It does not mention why the authors have chosen to focus on grain size distributions nor how they expect them to change the debris flow fan.

Methods

The experiment design is reasonably well explained, however I struggled to understand what exactly was being measured. The authors have gone to great lengths to capture the vast amounts of data generated by the experiments; however, they do not discuss why they collected these particular datasets or what they plan to do with them. A better motivating statement within the introduction will help to improve this section.

Line by line comments:

Lines 50-60: The authors have not explained how the debris flow surge is generated. The paragraph could be separated to first describe how and where the surge is generated before discussing the depositional area.

Lines 61-67: How fans are produced in this study? Is the fan a result of a single surge triggered by the outlet of water? Or the result of multiple surges? Is the erodible layer rebuilt between surges? What is going to be measured as a result of these experiments?

Lines 69-74: Why is flow displacement being measured? Why is the flow height of the surge important to the authors?

Lines 79-80: What is being measured and how?

Line 86: It is not clear what is meant by “the SfM-MVS photogrammetry could not measure locations where granular flows descended”. Does this mean that the photogrammetry cannot measure the flow when it is moving?

Results

The results section also suffers from the same problems as the previous sections. What is being measured and compared between the different runs is not specifically stated and as

a result it is hard to understand some of the findings of the manuscript. Many of the result figures are poorly explained and some are cited out of order or not cited at all.

Line by line comments:

Line 95: What is the lower portion of the flume? How is this defined?

Line 96: Same with arrival point and upper position.

Line 97: What is a run? This refers back to the earlier point that it is not clear whether the experiment is single or multiple surges.

Line 101: Unclear how the arrival time is measured.

Lines 103-106: This should be in the discussion or introduction rather than in the results

Lines 110 – 113: It is unclear which panel of figure 3 is being referred to. Panel 3c is also not cited at all in this section.

Figures S2 and S3 seem important to the overall narrative of the manuscript and therefore the authors should consider including them as part of the main text.

Lines 119-121: State how the locations of the lobes differ, are they closer to the flume exit? Does the slope differ between the 2 locations?

Line 124: Why is the series of events being described in terms of time? Time is not likely to be a controlling factor in how the debris flow behaves. The slope over which it is traveling is much more likely to be the control (along with the grain size distribution).

Figures 5 – 9 are very poorly explained and hard to interpret. This is not helped by there being no explanation of what is meant by "Run" in the experiment.

Lines 217-129: A numerical metric would help to compare the shape of the mono vs multigranular flows. Perhaps the angle of deviation from directly straight or a ratio of the left vs right side length?

Line 135: The difference in shape at the 2.2m line could be due to the difference in runout. While in the monogranular flow the authors are measuring the apex of the flow height in the multigranular it after the apex. As the debris flows are producing a fan like shape you would expect the fan to be wider after the apex regardless of the granular structure.

Discussion

The discussion, similarly to the introduction, is lacking in detail and is too vague in some of its points to make an impact on the reader. Currently the discussion spends too much time focusing on areas the authors did not investigate (pore fluid seepage) and not enough time putting their results back into the context of the literature. To help the reader the authors should put their results, which are interesting and novel, front and centre and discuss the processes that they actually recorded before moving on to areas they do not have direct evidence for.

Line by line comments:

Lines 141 – 142: The term “processes” is too vague and the results do not mention stratigraphy at all so this seems like a strange sentence to start the discussion with.

Lines 142 – 147: This section is poorly linked to the previous opening statement of the discussions.

Lines 150 – 151: This would also apply if the flow was monogranular with coarse grains.

Lines 152 – 155: The authors previously mentioned that there was minimal difference between the thicknesses of the mono and multi-granular flows. This idea of the coarser grains forming an obstacle which diverts the tail of the flow should be expanded upon further with more descriptions if the authors believe it to be significant.

Lines 158 – 164: It is unclear what the authors are suggesting here. How can there be moisture differences in the bed of the depositional area? The depositional area is the same for all of the test runs? Unless they are discussing deposition on a previously deposited

fan? This is very underdeveloped.

Lines 165 – 169: This section is a strange ending to the manuscript. It focuses on two areas which the authors did not measure in their study; stratigraphy and moisture content of the fan.

Conclusions

The conclusions does not contain any references to the discussion and therefore it feels disconnected from the rest of the manuscript.