Response to Reviewer 2 comments on manuscript: egusphere-2022-165 "Use of fluorescent sand to assess plot-scale hydrological connectivity and sediment transport on young moraines in the Swiss Alps"

COMMENTS FROM REVIEWER 2 AND RESPONSES OF THE AUTHORS.

This study employs night-time sprinkling experiments, dyed water and fluorescent sand distributed on steep moraine slopes to investigate runoff generation and sediment transport at the plot scale. Drone-based orthophotos, digital surface models and field measurements are used to acquire and analyse surface characteristics, for example slope, vegetation and rock cover, roughness and soil properties. While the movement of fluorescent sand particles was monitored, overland flow and sediment yield from the erosion plots were measured at the downslope end of the erosion plots.

Overall, the paper is well written, and the described method (the innovativeness of which I cannot properly judge) appears as an interesting means to detect and quantify surface runoff, sediment mobilisation and transport (distances) across such steep slopes. The paper also contains interesting insights, for example regarding the influence of stone cover on hydrological connectivity (and consequently also sediment transport), which generally makes it a good contribution for both the hydrological and geomorphological community. I found references both to methodological and scientific papers to be ample, and the aims clearly stated.

We thank the reviewer for these kind words and seeing the value of this method and our results.

While generally the conclusions appear to be supported by the results, I think that the "connectivity" part of the objectives could be better elaborated and/or discussed. While "connectivity" is in the title of this study, I feel that the applicability of the fluorescent sand method to investigate connectivity at a more relevant (i.e. larger) spatial scale, for example hillslope-channel coupling should be discussed more explicitly. This is related to the question as to how much connectivity on such small spatial scales matters; if surface runoff re-infiltrates downslope of the plot, then the connectivity observed ON the plot would have limited relevance regarding hillslope-channel coupling. I do acknowledge though that the authors are aware of this, as the title judiciously contains the restriction "plot scale", but considering the relevance of connectivity at (an even slightly) larger spatial scales (i.e. the hillslope scale) for catchment-scale sediment dynamics, it would be really interesting to learn what the authors think about the potential application to sediment transport on and delivery from hillslopes or hillslope sections larger than their plots. For example how large do they think is the area that can be monitored by their method?

We indeed only focused on the evaluation of connectivity at the plot scale and will make this much clearer in the beginning of the manuscript (i.e., in the introduction and method section). We will also expand the discussion (near L524) to describe how connectivity may increase (due to more water flow) or decrease (due to more infiltration or vegetation) further downslope. We nevertheless think that it is important to show runoff generation and connectivity mechanisms on such small spatial scales as well, as these patterns will determine the response at larger hillslope scales.

We think that the method itself can be used at larger scales as well. As mentioned on L91-93 fluorescent sand has been used in geomorphological studies on sediment transport on beaches (e.g., Ingle, 1966). However, with the current set-up, the method cannot really be used at larger scales (L482) because the resolution of the pictures would be too low (and the camera would need to be too high to be practical). However, there are of course other ways of imaging larger hillslopes, e.g., using multiple cameras or taking pictures of small parts of a larger area. For example, a camera on a rail could take multiple pictures of the hillslope. We will expand this discussion in the revised manuscript.

The relation of shutter speed, ISO setting and motion blur on resulting images is not properly described as "high shutter speed" means shorter exposure times and hence smaller risk of motion blur.

We agree and thank the reviewer for spotting this mistake. We meant "slow shutter speed" instead of "high shutter speed" and will fix this in the revised version of the manuscript.

I would question whether hillslopes whose average (!) gradient differs by as much as 15° are in fact "comparable". Slope is a factor, and of course it is very difficult to find every combination of inclination, vegetation cover etc on a lateral moraine of limited size. Furthermore, the plots have a different relative position – though generally consisting of the same parent material, footslopes could have different properties than top- or midslope locations due to decades of soil redistribution, for example. So maybe the "comparable" needs to be supported by other evidence or be tuned down a bit by acknowledging the variability that makes strong, unambigious (cor)relations with single controlling factors somewhat unlikely. By the way, the correlations should be reported with the correlation coefficients AND the p value, not only with the latter that only shows the significance, but not the strength (and hence something like relevance) of the correlation.

We agree that the plots are only 'somewhat comparable' and differ in slope. In fact, we chose the plots in such a way that they cover as much of the variability within the two moraines as possible (L149). This is also the reason that we report the results per plot and not only average values per moraine. Regardless, we will tone down the statements about the comparability of the plots and state in the methods more clearly that we do not expect the correlations between individual variables to be very strong due to the variability in the plots in slope, etc. This is the reason why we also tried multiple linear regression.

As for the variation in soil properties, we agree that there are some differences between the plots but need to highlight here that the soils are very young, i.e., the soils of the 1990 moraine, where the plots differ in topographic position, have been ice-free for less than three decades. The topographic position for these plots mainly has an effect on the amount of flow that is generated during events (i.e., the footslope plots were wetter and generated more overland flow; L395-398). The two plots on the 1860 moraine are both located at a midslope position.

As for the correlations, we showed the correlation coefficients in the figures but we will include the Spearman rank correlations in the text as well.

A major concern for me is the fact that a digital model derived via SfM is always a digital surface model (DSM) in the presence of vegetation, not an elevation (=bare ground) model. The authors chose to measure slope and roughness in the field (although slope could have been measured on the DSM by measuring points on the slope, their distance and elevation difference) and correctly justify this with vegetation being contained in the DSM. On the other hand, they start the paragraph with the statement that the "DTM was used … to determine the potential direction of OF". Where vegetation is present, it would be impossible to correctly identify potential flowpaths, and moreover I imagine that the vegetation covers the surface on which the sand particles travel, which might render them invisible, so that the modelled potential flowpaths are not realistic, and can moreover not be compared to the real-world flow paths if/where the latter are not visible.

Indeed we derived the surface topography from drone images (SfM) and this includes the vegetation (i.e., DSM instead of DTM). However, the vegetation on these young moraines is very short and sparse and the influence of vegetation on the derived topography is therefore relatively small. While water flow is influenced by vegetation, we think that the overall flow routing and flow accumulation on the moraines was only somewhat affected by the vegetation because the slopes are very steep and the vegetation is very short (see for example Figure 10c). Above all, we used the DSM only in a qualitative way to relate the observed patterns of the sand (and the blue dye) to topographic derived flow pathways. We also used them to derive contour lines for visual representation of the topography (e.g., Figures 8a, 10c). In other words, we use the topography here only for visual comparisons. Slope and microtopography were derived in the field (i.e., measurements below the vegetation).

We will add a comment to the text that the flow accumulation is influenced by the vegetation and replace DTM with DSM.

Lines 162-169: I did not understand why high-resolution images taken by a different drone were taken for visualisation purposes ("background") and for determination of rock cover, while you used a different drone (with apparently poorer resolution as I understood the text) to produce the DSM with "coarse" (4 cm is not too coarse anyway) resolution. The authors could explain better why they used which system, and what are "georeferenced photos" – on a steep slope, I can hardly imagine how georeferencing (without orthorectification) could produce images as a suitable mapping basis.

We could not use the "high resolution" drone pictures to derive the DSM because we used the simple drone (Phantom 4 pro) only to take pictures of the plots (thus no SfM). The earlier (somewhat coarser) drone flights were used to create a DSM for all the moraines.

The photos were georeferenced and orthorectified. We will clarify that in the text.

I have added similar, and some more comments (also typo corrections etc) in the annotated version of the manuscript.

We very much appreciate the comments and edits to the manuscript and will make these changes in the revised manuscript.

Regarding the comments that were not already raised above, we will:

- L69: include the suggested references in the text.
- L151: add a short definition of vegetation complexity.
- L163: state what software (Pix4D mapper) was used to derive the DSM and that the DSM was made by WWL Umweltplanung und Geoinformatik GbR (https://www.wwl-web.de/).
- L364: write that the multiple linear model was used to predict the change is sand area and sand distance.
- L393: state that the lower frequency of OF for the 1860 moraine was not related to K_{sat} (which was in fact lower for the 1860 moraine than the 1990 moraine) but is instead related to the degree of rock cover.
- L447: better highlight in the results section that the blue dye and the sand were both transported to the bottom of the plot. This can be seen in Figure 10 (and for the sand also in figure 8). So, it does mean that the water and sediment would have been transported further downslope.
- L491: rewrite the sentence.
- L500 and 504: rewrite the sentence and state that the roughness and tortuosity index are high (not the microtopography). We will remove the word microtopography in these sentences.
- L524 and L550: more explicitly state that we look at connectivity at the plot scale and expand the discussion on the use of the method at larger scales (see also answers to comment above). We will refer to the Lane et al. papers (2004, 2009) when we discuss re-infiltration.

As for the comment on L169, the comparison of the DSM and inclinometer derived mean slopes suggests that these differences are small.

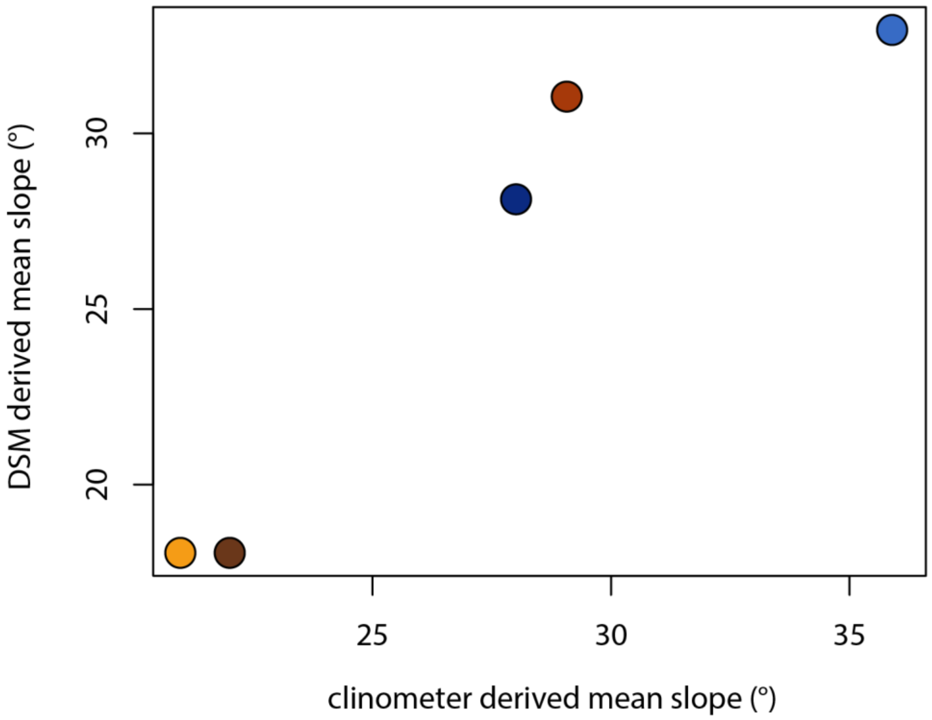

Figure 1: Difference between clinometer derived mean slope and DSM derived mean slope for the five plots. The different colors represent the different plots (same color code as in the paper). The differences in mean slope between the two methods never exceeds 3°.

As for the comment on L481, we cannot compare the flow accumulation (which is calculated for the entire moraine and thus based on the entire upslope area), with the amount of overland flow because we didn't sprinkled only the plots (and some area beside and below the plots) but not the entire upslope area of the plots.