EGUsphere, referee comment RC1
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Comment on egusphere-2022-165

Anonymous Referee #1

Referee comment on "Use of fluorescent sand to assess plot-scale hydrological connectivity and sediment transport on young moraines in the Swiss Alps" by Fabian Maier et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-165-RC1, 2022

This work assesses the feasibility of using glow-in-the-dark colored sand to study sediment transport in five 4 m × 6 m plots on two Swiss moraines. To this end, sprinkling experiments are executed on the plots, overland flow (OF) is measured with a Bernoulli tube at the downslope end of the plots, and high-resolution photographs are taken before and after sprinkling experiments. Such measurements are used to determine OF characteristics as well as the maximum spatial extent traveled by the sand.

These are the main results:

- If the soil particles of the plot match the dimensions of the colored sand, this approach is successful at showing the redistribution of sediments onto the soil surface. This can be executed even if no OF is measured at the downslope end of the plot and without any pictures/videos being taken during the sprinkling experiments.
- Areas of the plot surface where the colored sand was redistributed tend to be consistent with some of the areas where OF was observed through dye staining experiments.
- Total OF and sediment yield could be predicted by the combination of the plot rock cover, vegetation cover, and rainfall intensity. However, such parameters were not strongly related to the changes in the spatial extent traveled by sand.
- Microtopography and vegetation cover can explain sand redistribution on the plots.

The paper proposes and investigates an experimental method to study the changes of the Earth surface where unconsolidated sediments are uncovered, such as in glacial moraines. This is within the scope of HESS to study the "spatial characteristics of the global water

resources and related budgets, in all compartments of the Earth system" as well as "the role of physical processes in the cycling of continental water in all its phases and at all scales".

While the paper presents novel data, I am not particularly convinced it presents novel concepts, ideas, or tools. The idea of using sediment color to quantify sediment sources and transport has a long tradition in Hydrology, and has been addressed with multiple techniques spanning from diffuse reflectance spectrometry to fluorescent tracers. Additionally, OF timing and surface hydrological connectivity have already been investigated with liquid and particle tracers as well as a vast array of sensing techniques, including image analysis.

I agree with the Authors that the main conclusion of the article is the convenience of using colored sand to partially (since some of the sand disappeared) show the redistribution of sediments in small scale plots without taking pictures/videos continuously during the experiments. This piece of information may be useful for conducting research in environments like those illustrated in the paper.

Additional conclusions entail the facts that sand redistribution most likely occurred along OF paths, and vegetation cover was largely responsible for the observed response in total OF and sediment yield. These conclusions support well-known results available in the literature.

Regarding the scientific methods and assumptions as well as result traceability, I think that further parameters should have been considered and should be included in the paper to better understand how the experiments were executed (see later).

In my opinion, one major issue is that results are not sufficient to support some of the interpretations and conclusions. I do not agree with the Authors that colored sand illustrated OF pathways. OF pathways are routes taken by water and may not necessarily all coincide with paths taken by heavy sand particles. I agree that sediments redistributed where OF occurred, but water may have also followed a lot more paths that were not necessarily taken by sand. OF pathways can be studied by using neutrally buoyant (which do not sink) particles of more varied granulometry than those used by the Authors. Similarly, I do not think that much can be concluded on the surface hydrological connectivity of the plots based on the reported results. For example, even if a detailed description is not provided, it looks like sand was deployed on top of the vegetation in the 1860 plot. After the sprinkling experiments, such sand disappears since it is probably masked by the vegetation top cover. On the other hand, water did not necessarily follow the same path (that is, it did not remain trapped below the vegetation) and, therefore, nothing can be said on the relative OF paths in that area. In parallel, I suggest the title of the article is opportunely edited to include the sole assessment of sediment transport.

To properly assess the consistency of sediment and OF pathways, the Authors should

include an experimental validation section where the brilliant blue dye tracer solution is deployed during all sprinkling experiments at locations consistent with the colored sand. By continuously (and automatically) capturing dye pathways, reliable information could be inferred on the hydrological connectivity and overall OF pathways in the plots. For instance, data on the average dimensions and length of the rills/OF pathways would be particularly useful to fully understand the mechanism of sediment transport. A more careful analysis of this aspect would highly enrich the work.

The manuscript did not always give proper credit to related work: several investigations on experimental studies for sediment redistribution are not included among the references (see for instance the work by Martínez-Carreras et al., 2010). Likewise, the objectives of several cited works are not properly stated. Here a few examples.

In the Introduction, it is stated that climate change is also expected to increase the frequency, intensity, and amount of heavy precipitation. This is a very general statement and cited works are not related to glacier covers nor to environments like those studied in the paper. Moreover, reference Maruffi et al., 2022 is not even reported in the Bibliography.

In several instances, the Authors refer to the continuous acquisitions of photos/videos as an experimental disadvantage. However, continuous pictures could provide reliable information on the mechanisms underlying the formation of OF pathways, which, instead, cannot be properly justified in this work (see the conclusions on rain splash detachment and transport that sound rather arbitrary).

The methodology should also undergo an extensive revision based on the specific comments reported below. Presentation quality is generally good apart from minor points highlighted in the following.

Specific comments:

Abstract: I think this part should be re-written by better stating the actual results of the paper and reducing deductions on the hydrological connectivity that cannot be properly supported.

Introduction: the first introductory part on climate change as well as Hortonian and saturation-excess overland flow is too general and should be more cautiously related to the literature.

Regarding the research questions, I think those should be reformulated since fluorescent sand particles cannot be properly used to trace water.

Sprinkling experiments: I think a major flaw exists in the selection of the average rainfall intensities adopted in the experiments. In Table 2, rainfall intensities are not consistent at all among the plots. How can we compare results for the 1860L plot – HI experiment (81 mm/hr) to the 1990L plot – HI experiment (48 mm/hr)? Conversely, the experiment on the 1860L plot – MI (43 mm/hr) replicates a rainfall like that on the 1990H plot – HI. Rainfall durations are also inconsistent. Thus, given such diverse "meteorological forcing", it is unclear what we can infer from these experiments since plot characteristics, cover, and "complexity" are also diverse.

Image pre-processing and analysis: It is not explained how image spatial resolution was adjusted to make each pixel refer to an area of 1 mm2.

A main assumption of the image analysis method is that "the main part of the sand ribbon did not move during an experiment". I think this is a rather strong assumption and should at least be supported with photographic material (i.e., a time-lapse during the experiment).

The following methodological details should be considered:

- The time between sand deployment onto the plot surface and the beginning of the sprinkling experiment should be included.
- Likewise, average wind values should be integrated since the wind may have influenced the redistribution of sand particles.
- Further details on how the sand was deployed should also be stated. The presence of vegetation implies that some of the sand particles remained on the top of the vegetation cover while some other reached the soil surface. Such "depth" effects cannot be observed in images but may have most likely influenced the sand redistribution.
- The order at which experiments were executed also plays a role in the results. Sand particles in the final (HI) experiments may have moved through already existing rills. So, are these experiments (and the related results) truly independent on each other?

Discussion: Lines 461-468. I do not think this comment is relevant since some of the cited works aimed at identifying OF pathways/timing. This work instead can only deduce OF paths from sediment redistribution.

Some details on the sand particles should be included as well:

- Were the particles charged? The occurrence of aggregates suggests so and this may have influenced their motion and redistribution onto the soil surface.
- Did photoluminescent material lost from the sand particles coat/bind to other soil particles? This may have created "fake" colored particles.
- Did the sand particles exhibit photo-quenching effects or changes in their color following exposure to sunlight/soil pH? These are typical photoluminescence effects that may have also influenced results. A preliminary laboratory characterization of the sand material should have been executed before experiments outdoors.

Code and data availability

The reported statement is not in agreement with HESS policy (https://www.hydrology-and-earth-system-sciences.net/policies/data_policy.html)

Minor points

Line 289: "plot containing" is unclear

Line 228: moraine is misspelled

In conclusion, I support consideration of this work upon a major revision of its objectives, better pondering of the existing literature, and inclusion of validation experiments that may help in broadening the overall scope of the work. Further, justification of the methodological flaws (see the comments on the sprinkling experiments and the Discussion) should be provided by supporting laboratory tests on the characterization of the sand material.