Response to comments from Reviewer 1 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"

We thank the reviewer for the very useful comments, which will help to improve the manuscript. We provide the comments in full below and our response in blue font.

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.

> We thank the reviewer for the summary of the main findings and agreeing with us that the topic of the manuscript is within the scope of HESS. We agree that fluorescent tracers have been used in previous studies and that overland flow and connectivity have been studied in the lab and field studies before. However, the novelty of this study is in the combination of the different methods that were used.
>
> Rainfall simulation is a frequently used method to study overland flow generation and sediment transport. The unique part of this study is that we used this method to study overland flow and sediment transport on young moraines. We are not aware of any other study that has used rainfall simulation on relatively large runoff plots to study sediment transport on young moraines during extreme events. This is an important research topic because the ongoing glacier retreat exposes new

bare areas. It is important to know erosion rates from these sites to understand water quality in Alpine areas, and in particular for hydropower reservoir management.

We also agree that dyes and colored sand have been used to trace water and sediment before. We reference some of these studies in the introduction, but as far as we are aware, colored sand has not been used on natural hillslopes to study sediment transport. We think that the evaluation of the method is useful and that this method has potential for other studies. Therefore, we think that this manuscript is useful for researchers who would like to use this method.

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.

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

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).

We agree that some details on the experimental set-up and data analyses should have been included in the manuscript and will use these comments to improve the manuscript. Unfortunately, we can't do any further experiments because the plots are no longer in place. Please see responses to the detailed comments below on how we plan to improve the manuscript based on these comments.

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.

We agree with the reviewer that there could have been water flow but no sediment transport on some parts of the hillslope (although the blue dye tracer experiments suggests that this was not really the case). Indeed, we assume that sediment transport indicates that there was water flow, as shown by also the comparison with the blue dye in Figure 10 in the manuscript - but note our discussion on splash erosion as well. We did not intend to also infer the opposite, namely that a lack of sediment movement is an indication of a lack of water movement. We will explicitly state this in the revised version of the manuscript. We also see that some of the writing in this regard was not clear enough and will carefully read through the manuscript to ensure that we don't accidentally give the impression that no sediment transport implies no water movement.

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.

As described above, we fully agree with the reviewer that there can be water movement (and thus connectivity for water movement) even if there is no sand movement. However, we focused here on the connectivity of the pathways along which sediment moves down the slope. Indeed, we don't expect to be able to use the sand to trace sediment over very long distances because it will get stuck behind rocks and/or vegetation. We will more explicitly state this in the revised version of the manuscript.

We agree that in some situations – e.g., when one is interested in tracing the water, it is very useful to use neutrally buoyant particles. However, we were interested in the sediment movement on the hillslopes. For this, it is less appropriate to use neutrally buoyant particles because they would move further than the sediment. Furthermore, a big advantage of the fluorescent sand is its low cost compared to fluorescent spheres.

We will change the title for the manuscript, e.g.: "Use of fluorescent sand to assess sediment transport on young moraines in the Swiss Alps".

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 experiment with the blue dye was indeed done to show where the water flowed and to "prove" that the sediment moved with the water. We agree that it would be very interesting to do more experiments with blue dye and to look at the water flow pathways, to understand how wide the flow pathways are or how far the water flowed down the hillslope before it reinfiltrates. However, the aim of the project was two-fold: 1) to test if colored sand particles can be used as an indicator of water driven sediment transport and 2) to understand the differences in the amount of overland flow and sediment yield for the different plots. Thus, we looked at the total amount of overland flow and sediment at the bottom at the plots. To be able to relate these fluxes to processes, we looked at the travel distances of the colored sand. Unfortunately, we cannot do any other experiments as the project has finished and the plots are no longer in place.

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.

We apologize for not citing the Martínez-Carreras et al. (2010) study and will reference it in the introduction and discussion sections. We will also better highlight the advantages and disadvantages of using a camera in the introduction section. In particular we will more clearly state in the discussion that other methods are available but that the method tested here is particularly useful if one cannot be in the field at the time of the rainfall event, that the colored sand is relatively cheap, and that the method doesn't rely on more elaborate analyses of samples afterwards.

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.

> We will update the references in this section to ensure that they focus on the expected effects of climate change in Alpine areas. We will also carefully check the reference list to ensure that all cited papers are included.

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).

> We agree that continuous photo collection is extremely useful. We certainly did not mean to imply that continuous photo collection is a disadvantage. What we meant to say is that with this method, it is not necessary to have continuous photo collection and that this method can thus be used in locations where continuous photo collection is not possible (either because of the remote or exposed location, or because one doesn't know when the rainfall event will occur) or when measurements need to be taken at multiple plots and it is too expensive to equip each plot with a camera. We will carefully read through the manuscript and revise the text to make this point clearer.

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.

> Thank you for these very useful comments, please see detailed responses below.

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.

> We will revise the abstract accordingly.

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.

> We thought that it would be useful to have a general introduction on overland flow but if the editor and other reviewers agree with this comment, we can certainly shorten this section.
>
> As mentioned above, we will update the literature cited for the effects of climate change on rainfall intensity.

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

> We agree and will reformulate the research questions. As mentioned above we will rewrite the text to avoid the impression that a lack of sediment movement indicates a lack of water flow.

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.

> We agree that the rainfall intensities were different for the different plots and that especially the high intensity experiments on the 1990L and 1990H plots were not as intense as for the other plots. This was unavoidable for the field simulations because of wind drift and differences in water pressure. We will explicitly acknowledge this difference in intensity in the text and add the intensities to figures 6 and 9 in the manuscript. This way, it will be clearer that the intensities differ and that the results for different intensities can be compared for each plot but not across moraines. We mainly use the comparisons of low, medium and high intensity results for an individual plot to show the effect of rainfall intensity and will make sure that all statements that compare plots come with a discussion on how that result may be affected by the difference in rainfall intensity. We will normalize the amount of overland flow shown in Figure 6 in the manuscript by the amount of rainfall (see new figure below) so that the results are more comparable. For the other results, we will also mention how the results are different when they are normalized by rainfall intensity or amount.

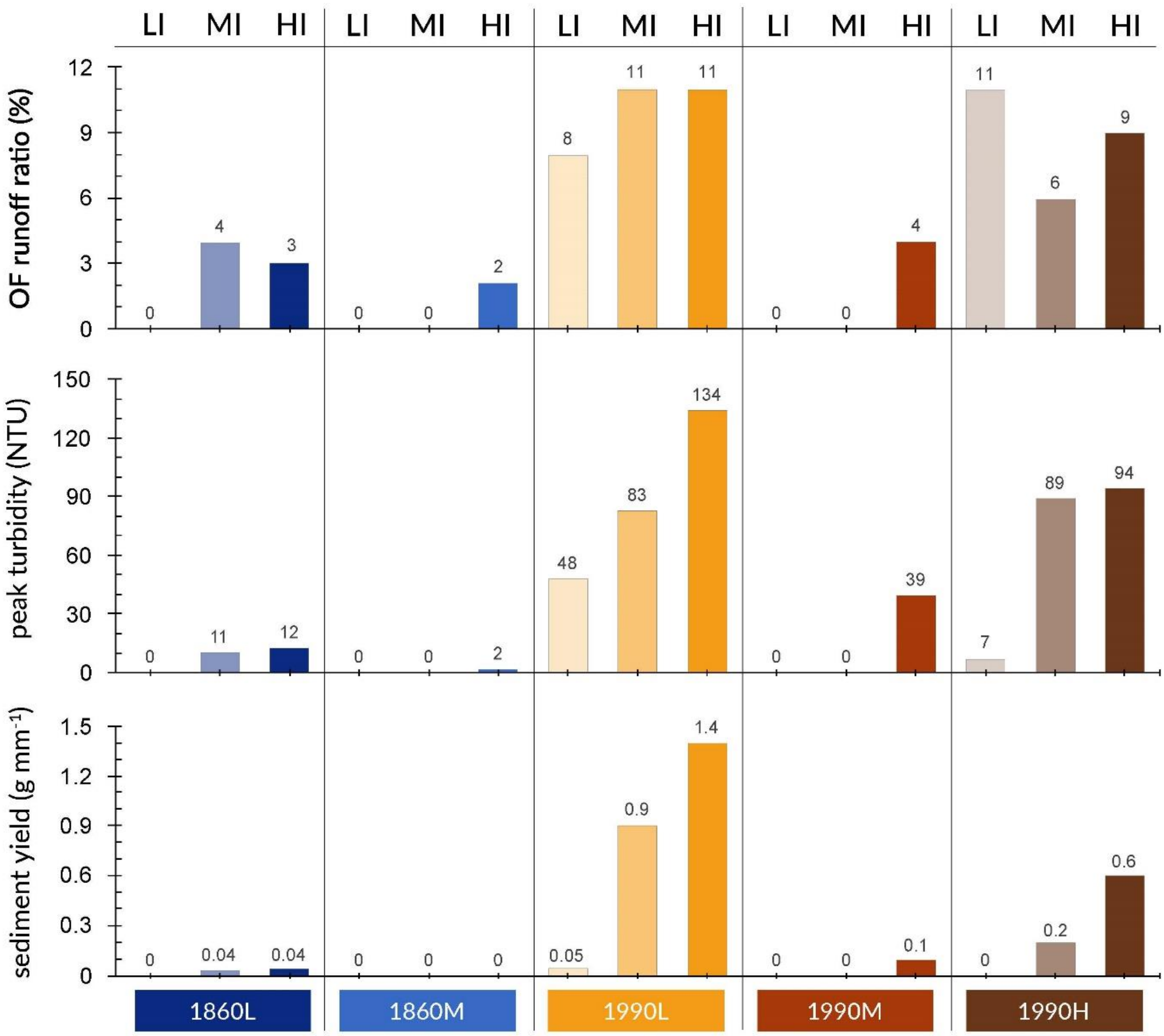

Figure 1: Revised version of Figure 6: Bar charts of the runoff ratio of OF (%), peak turbidity (NTU), and the sediment yield (g per mm of rainfall) for each sprinkling experiment (LI, MI and HI represent low, mid and high intensity experiments, respectively) on each plot (1860L, 1860M, 1990L, 1990M, and 1990H). The absence of a bar indicates the lack of measurable OF (and thus also turbidity and sediment yield). The number above the bar denotes the actual value for each experiment. By normalizing the OF amount and the sediment yield by the rainfall amount, results are more comparable.

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.

We thank the reviewer for pointing out this lack of information and will add the missing information to the revised manuscript.

The color of the chain around the plot changed every 0.5 m, which facilitated the selection of the location of four points on the chain: top left corner, top right corner, a point on the bottom left, and a point of the bottom right of the images. We gave these points the known coordinates (measured in the field). For the resampling, we used the skimage.transform.resize function in python. This function uses a Gaussian filter (Gaussian smoothing) to obtain a lower resolution image from a higher resolution image. The original image had more pixels (in width and height) than the final image. Gaussian filters are commonly used in image processing to avoid aliasing artefacts.

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).

We agree that this is a big assumption. We do not have time-lapse images for the entire duration of the experiments. However, the comparison of the before and after field observations and photos (example for the 1990H plot below) show that the sand ribbons did not move relative to the big rocks. This can also be seen in Figure 3 in the manuscript: the majority of the sand line stayed in place and the ribbon kept the same shape because only some sand particles moved downwards

Figure 2. Daylight drone photo of the 1990H plot with the blue sand before (top) and after (bottom) the HI experiment. Note that on a daylight photo you cannot see (or hardly see) sand particles away from the sand line. However, these photos do show that the main part of the sand ribbon did not move 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.

We applied the sand in the evening before the experiment and then took the nighttime pre-experiment photo. We did the rainfall experiment the next morning. Thus, the time between sand deployment and experiment was 10 — 16 hours.

Likewise, average wind values should be integrated since the wind may have influenced the redistribution of sand particles.

> We do not have wind speed data, but wind speeds were never that high that they caused wind erosion. The sand particles have the same density as sand and are 0.3 - 0.5 mm diameter in size, so that they are less susceptible to wind erosion than neutrally buoyant 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 vegetation on the plots is very short and sparse. The sand ribbon was generally about 2 cm thick and mainly covered the small patches of vegetation – see photos in the supplementary material. We added the sand by hand. We will add more information on how we applied the sand in the revised version of the manuscript. We will then also explicitly state that some sand may have washed off the tallest vegetation on the 1860 moraines but that this transport would have been at most a few cm.

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?

> We agree that the experiments are not fully independent but don't think that this influences our results. While we observed sediment transport during the experiments, we did not observe any new rills develop during the experiments, nor any rills filling up with newly transported sediment. One exception may be the 1990H plot where we saw a small rill develop during the high intensity experiment (see also Figure 10 in the manuscript). We wanted to study the runoff and sediment response for natural hillslopes. We started with the low intensity experiments to ensure that they were not affected by any erosion that may have happened during the highest intensity experiments.

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.

> We agree. We wanted to refer to pathways of sediment movement. However, we do think that this gives us an indication of where (the majority of) the water flowed as well (but note that as stated above, we do not think that an absence of sediment means an absence of water transport). We will rewrite this section and better separate the studies on sediment movement and water movement.

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.

> No, the particles are not charged and consist of quartz. The aggregates that formed after the experiments were due to some of the glue dissolving during the experiment.
>
> The particles were charged in the sense that they were illuminated with the LED and UV lights to be able to take advantage of the afterglow effect and the fluorescent effect.

Did photoluminescent material lost from the sand particles coat/bind to other soil particles? This may have created "fake" colored particles.

> We did not observe this. Where we saw photoluminescent material, it was bound to the sand particles (but admittedly we cannot prove that).

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.

> We did not observe any changes in the color of the material. If there would have been small changes in the color, this would not have had an effect on the results because we determined the color range for each sand based on pictures taken before and after the experiments.

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)

> The overland flow and sediment yield data are now in a repository: Maier and van Meerveld (2021) HILLSCAPE Project - Data on moraine soil properties and on overland flow and subsurface flow characteristics. GFZ Data Services. https://doi.org/10.5880/fidgeo.2021.011. The images are included in the supplementary materials and can be uploaded to the repository as well. The code is based on the code of Weiler et al. (2001) and will be made available upon request.

Minor points:

Line 289: "plot containing" is unclear

> This is a left over from a previous version. We will remove it.

Line 228: moraine is misspelled

> We will fix this.

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.

> We thank the reviewer for the useful comments. We will use these comments to improve the manuscript. The comments have made it particularly clear that we have to write the sections on connectivity more clearly to ensure that they don't inadvertently suggest that no sand movement implies no water transport. Unfortunately, we can't do any new experiments because the plots are no longer in place and the project has ended.