Response to reviewer 1 comments of the manuscript: egusphere-2023-899 “Assessment of plot scale sediment transport on young moraines in the Swiss Alps using a fluorescent sand tracer”.

Comments of reviewer 1 are shown in black; the answers of the authors are shown in blue

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The paper is well written and structured. Most of the methodological framework is described in a well reproducible manner, and I found no flaw in the analyses.

We thank the reviewer for his kind statement and the acknowledgement of our work.

However, I have made several comments and would be interested in the response. There’s one major concern with respect to the content. The paper has a very strong methodological focus, but considerable effort was made to conduct the experiments on a remote and steep place (moraine hillslopes). This is more than just a natural laboratory that offers a range of different surface characteristics – it is a system that changes with time passing since deglaciation. Both the analyses of different surface characteristics and the findings regarding overland flow and surface dynamics should be interpreted more in light of the processes that effect changes on moraines after deglacation. I would suggest that this be written in an additional discussion section.

We thank the reviewer for this useful feedback and agree that the manuscript has both a methodological focus (discussed in section 5.1) and provides actual data and insights on sediment transport in a remote and steep place (discussed in section 5.2). This study was part of the larger Hillscape project (www.hillscape.ch), which focused on the evolution of hillslope characteristics and processes after deglaciation. We refer to these processes at the end of the introduction, where we provide our hypotheses. Thus, the reviewer is right in the assessment that the remote location was specifically selected to determine the effects of landscape evolution on hydrological processes.

We describe the effects of the different hillslope characteristics on sediment transport in the second discussion section. To address the reviewer’s comment, we have added more discussion on how the system changes after deglaciation and thus why the surface characteristics are different in this section. We also point to (our) other papers where these processes are described in more depth (L. 564-568 and L. 588-590). These studies include older moraines as well. It is harder to describe the evolution for just the two moraines (and only five plots) of this study.

In all, the paper is of potentially high interest for the community in geomorphology and soil (erosion) science. The strengths and weaknesses of the method are discussed at breadth and depth, and further developments are suggested to counter the weaknesses. If the scientific part regarding the proglacial characteristics and dynamics is strengthened a bit further, the paper can be accepted in my opinion.

> We thank the reviewer for pointing out the value of the study for the scientific community and appreciating the discussion of the strengths and weaknesses of the approach. We also think/hope that geomorphologists, soil scientists and hydrologists will be interested in the outcomes of the study and the presented method.

Comments in the PDF:

> We thank the reviewer for the comments in the PDF. We have copied the main ones and respond to them here below. We have implemented all other editorial suggestions and minor comments in the manuscript.

Manuscript, line 100 ff.: If the selection of the study area (and the plots on the different moraine slopes) is not only in order to have a variability of surface conditions, we need a fourth question that is related to the study area. Something like: Are the differences in surface (and soil?) conditions on moraine sections of different age reflected in measured sediment transfer and connectivity? This questions would then be related to the hypotheses mentioned below (e.g. lines 104ff).

> We are a bit confused about this comment. Indeed, the third research question asks how the surface characteristics of the different plots are related to the observed differences in sediment yield and sediment transport distances. The surface characteristics of the different plots already reflect the differences in surface conditions of the two moraines. Note that by having only two plots on one moraine and three on the other and having selected the plots to be as different as possible, it is not really possible to do any (statistical) analyses on the differences between the two moraines.
>
> However, we agree that the manuscript could be improved by providing more information on how landscape evolution affects the hillslope surface conditions and how this may affect sediment transfer processes. We added additional text and references on this in L. 564-568 and L. 588-590.

Manuscript, line 159 ff.: I still doubt whether this is feasible, given that the vegetation cover on the older moraine is 80-95%, and some of the photos show that shrubs and grass are taller than the claimed 5 cm on some plots. Throughout the paper, you do not show flow accumulation nor refer to these datasets for explanation or interpretation of your findings. Therefore consider whether the DSM is really needed here. If you consider it important, than what you do with it should be visible in the paper.

> We thank the reviewer for this useful comment. Indeed, we only used the information on the flow accumulation in a qualitative way and this is not a major part of the analyses

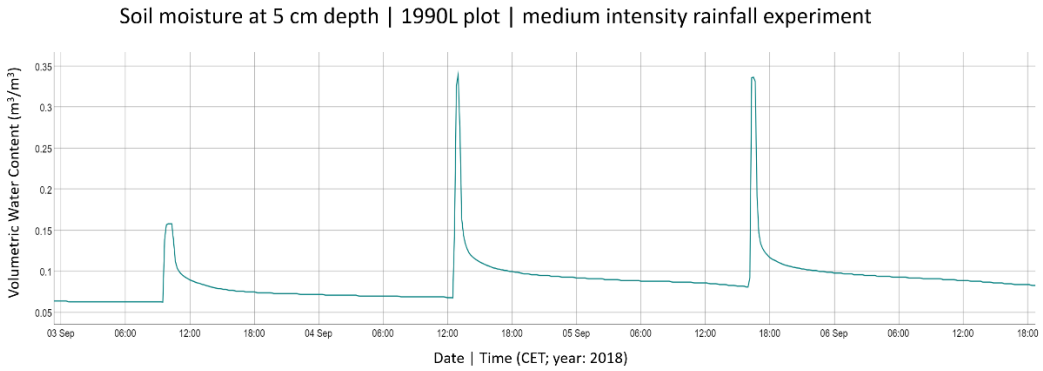
or study. We show it in Figure 10c (blue background shading) and describe the visible comparison between the sand movement and flow accumulation estimated from the DSM on L. 466-468.

Even-though 80-95% of the plots on the older moraine were covered with vegetation, most of it was short grassland which did not considerably impair a correct generation of the surface model and thus did not skew the results of the flow accumulation. The 1-2 larger shrubs on the plots also did not influence the suitability of the DSM for the flow routing algorithm.

Because the DSM is not an essential part of the paper, we have decided to not start the methods section with the description of the DSM and have moved it to section 3.4.

Manuscript, line 233-34: Assumption? Literature? Measurement?

This is largely based on the soil moisture measurements that we made in the plots (see for example the figure below.



Data: Maier, F. and van Meerveld, I.: 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, 2021.) and the fact that subsurface flow from the plots generally ended within two hours after the end of rainfall (Maier et al., 2021). It also agrees with other literature that show that drainage in coarse material is very fast and the majority of gravity drainage towards field capacity happens in one day. We have now added this clarification in the text.

Manuscript, line 324 ff.: Looking at this, a recommendation could be made (in the discussion) to use long nails with a fluorescent tip that are anchored deeply in the soil as reference points for the coordinate system.

We thank the reviewer for this advice. Indeed, this would be one of our recommendations. We added a sentence on this on L. 554 ff.

Manuscript, line. 347: But isn't that equivalent to the maximum distance (line 343) - well it's computed column-wise and not for the whole line, but still it is a maximum distance. I was a bit confused upon reading this - maybe you could re-write the first sentence of this paragraph. By the way, an alternative to taking the maximum distance would be to select a high percentile (e.g. P95) that is probably less sensitive to extreme values.

We used the described sand distance method as an improvement of just the overall maximum sand distance by having a look at the maximum sand distance in each pixel column and using all of these values combined as an indication or integrative measure for the maximum distance measure. This is also the reason that we used the root of the mean squared maximum distance for each column. We did not use another measure because for most pixels in a column there are no sand particles. So it would be a bit random to e.g. take 95 % of the maximum distances in each column as in most cases no sand will be found there. An alternative would be to calculate for each column the distances from the starting line to each pixel with sand. From all these values within a column one could then take e.g., the 95 % percentile and calculate then the "root mean squared distances" over all 95 % percentiles. However, we think that often even the 95 % percentile can lead to a negative distance as some of the sand disappeared below vegetation and rocks during the experiments and only a few particles/aggregates moved further down the plots (and thus, the sand ribbon on the plot gets "thinner", leading to negative values where the sand didn't move downslope).

In response to this comment, we have revised this sentence (L. 337ff.) as it is indeed confusing to say that we don't use the maximum distance and then still use a measure that puts an emphasis on the particles that traveled furthest.

Manuscript, line 445: This is hard to understand as with "negative travel distance" (along the slope) the reader might associate an upslope movement... Except for occasional ejection of sand particles in the uphill direction by rainsplash, this is hard to imagine.

We agree that -at first - this is confusing. However, we also think that the ability to have a negative distance is useful, exactly because it allows us to quantify the disappearance of the sand or, as the reviewer points out upslope rain splash. To avoid confusion, we now explicitly mention what a negative value for the transport distance means early on in the manuscript, on L.356-359 and L.572ff, and in the caption of Figure 9 (L.1103ff.).

Manuscript, line 588: I repeat an earlier comment: I have not understood how delta D can be negative. Needs to be explained earlier, and perhaps also here.

We agree with the reviewer that the calculation of negative travel distances needs to be explained more carefully. Thus, we changed this paragraph (L.337 ff.), and repeat this explanation in the caption of Figure 9 (L.1103ff.).

Manuscript, line 595: After two comprehensive sections dealing with the discussion of the method and of the results regarding OF, sediment transfer/connectivity and sediment yield, I think that one thing is a bit "underexposed" in your manuscript: You selected moraine slopes, which means you had to work very hard to get the equipment out there and perform the experiments. In the discussion, it almost seems you "just" used the moraine as a natural laboratory that has similar slope angles and features different characteristics such as rocks and vegetation. I think that your observations and findings, both with respect to differences in surface characteristics and process dynamics (OF, SY, connectivity) are worth more. They should be more/better discussed with respect to the changes that a deglaciated moraine experiences. What are the processes that change the characteristics (and hence also the dynamics) on a moraine slope in the first decade(s) or century after deglaciation?

We thank the reviewer for these nice words. We are aware of the uniqueness of the dataset of this study and appreciate that the hard work behind it is valued. The study was part of the Hillscape project, which focused on the landscape evolution and changes in vegetation, soil, and hydrological processes) after glacier retreat. We have published on these aspects elsewhere but also agree that the manuscript could be improved by providing more information on how landscape evolution affects the hillslope surface conditions and how this may affect sediment transfer processes. We added additional text on this in L. 564-568 and L. 588-590 (see comment above).

Manuscript, line 1045-1046: Then consider using symbols where overplotting is clearly visible!

We thank the reviewer for the comment. However, we think that in this case the symbols can stay the way they are as all of the overplotting symbols represent experiments without OF (and thus with a value of 0). In addition, we made all these symbols empty to visualize the overplotting.

Response to reviewer 2 comments of the manuscript: egusphere-2023-899 "Assessment of plot scale sediment transport on young moraines in the Swiss Alps using a fluorescent sand tracer".

Comments of reviewer 2 are shown in black; the answers of the authors are shown in blue.

The paper is generally well structured and will likely be of interest to those interested in soil erosion, deglaciation of alpine environments, and sediment transport. However, as mentioned by another reviewer, the paper would benefit from a more in-depth discussion of these experiments in context of landscape change following deglaciation.

We thank the reviewer for pointing out the value of the study for the scientific community. We agree that the manuscript could profit form a more in-depth discussion of landscape evolution after deglaciation and to better put the characteristics of the plots into context. To address the reviewer's comment, we have added more discussion on how these systems change after deglaciation, and the processes that cause the surface characteristics of the moraines and plots to be different. We also point to other papers (that also include older moraines) where these processes are described in more

depth (L. 564-568 and L. 588-590). We also refer to these processes at the end of the introduction, where we provide our hypotheses.

The only other substantial comment that I have is that the methods for calculating sand movement need to be described more clearly. Additionally, these methods seem to be limited by their sensitivity to the "loss" of grains – resulting in negative values that obscure whatever real transport there was. I would recommend either more clearly explaining this in the manuscript and why it is a better method for calculating transport than other metrics, or using a different metric altogether.

We thank the reviewer for this comment. We agree that the calculations for the sand movement and the advantages of the chosen method compared to other metrics could be explained in more detailed in the manuscript. Thus, we added additional information on the method in section 3.3.4.

We consider the described method an improvement over calculating the overall maximum sand distance because it looks at the maximum sand distance in each pixel column and use all of these values combined as a distance measure. To skew the overall average to the longer distances (and avoid non-meaningful results because the distance is zero for many most columns), we used the root of the mean squared maximum distance for each column. Because for most pixels in a column there are no sand particles, measures such as the 95 % of the maximum distances in each column are also not very informative.

While a "negative distance" may at first sound illogical and confusing (and thus requires more clarification, which we now added on L.356-359 and L.572ff, and in the caption of Figure 9 (L.1103ff.), it is actually a useful feature when looking at sediment transport on surfaces where fine particles are transported into the soil or are lost below vegetation and rocks. These negative "distances" thus allow us to determine that only a few particles/aggregates moved further down the plots (and thus, the sand ribbon on the plot got "thinner", leading to negative values are created). It was this feature that allowed us to "quantify" the difference in the dominant sediment transport on the two moraines.

We now added additional clarifications to the metric on L.355ff. We discuss the reasons for not using the maximum distance on L.359-362.

In my opinion, the paper is novel and very interesting, and should be accepted given the two comments above can be addressed.

We thank the reviewer for these kind words and acknowledging the value of the study. We think that the two comments above have improved the clarity and value of the manuscript.

Comments in the PDF:

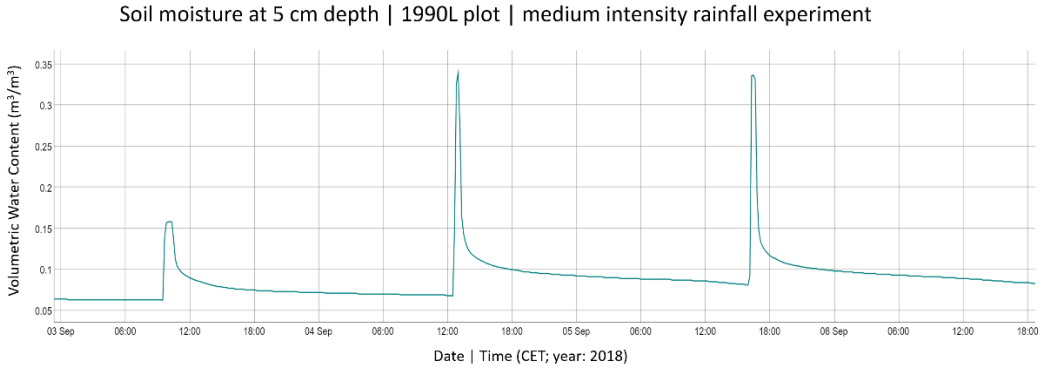
We thank the reviewer for the comments in the PDF. We have copied the main ones below and respond to them here. We have implemented all other editorial suggestions and minor comments in the manuscript.

Manuscript, Line 164 ff.: The DSM doesn't seem to add anything to the study. I would recommend removing it, because it is just distracting.

We thank the reviewer for the comment. However, we show the flow accumulation, which is based on the DSM in Figure 10c and thus need to explain it in the methods. We think that showing the accumulated area in this figure is valuable because it matches the observed flow pathways. We describe the visible comparison between the sand movement and the DSM on L.466-468. However, we agree that it is distracting to start the methods section with a non-essential part of the study. Therefore, we have decided to move the description of the DSM to a later section.

Manuscript, line 233-234: Citation? Or Measurement?

This is largely based on the soil moisture measurements that we made in the plots (see for example the figure below.



Data: Maier, F. and van Meerveld, I.: 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, 2021.) and the fact that subsurface flow from the plots generally ended within two hours after the end of rainfall (Maier et al., 2021). It also agrees with other literature that show that drainage in coarse material is very fast and the majority of gravity drainage towards field capacity happens in one day. We have now added this clarification in the text.

Manuscript, line 344-345: The measures that were used also didn't account for this... In fact, transport off the plots likely contributed to the confusing negative values.

It is both the transport off the plot (relatively minor as we didn't observe that many particles in the outflow) and the transport into the soil or below vegetation and rocks (more important) that contributes to the negative values. We now describe this more clearly on L. 356 ff. Note that this is also discussed in section 5.1 and 5.2.

Manuscript, line 347: I'm confused by how this differs from line 343 (maximum distance). It is unclear.

> The two are indeed related, although one focuses on an area and the other on a distance. One can image that depending on the pattern of how the sand moves across the plots, it may be more useful to look at the area or the distance. In fact, the correlation between ΔA and ΔD is 0.95 We now highlight on L.337-339 that the two measures are related.

Manuscript, line 431: Interesting. Why do you think that is the case? Is it because the turbidity increased?

> We think that splash erosion could have contributed to this result, which is discussed in section 5.2.

Manuscript, line 441: I don't understand this comment. Fig. 13 in Supp. Material shows that 1990H had a higher sediment yield than 1990M. Shouldn't that correlate with the fluorescent sand movement?

> We thank the reviewer for this valuable comment. Indeed, for some experiments we observed a discrepancy between OF amount (and thus sediment yield) and the actual movement of the fluorescent sand. The potential reasons are discussed in section 5.2 (L. 612 ff: e.g., the influence of splash erosion, which can move the sediment without actual OF or the generation of OF and sediment transport from areas close to the gutter (and thus without any movement of the fluorescent sand further on the plot).

Manuscript, line 444-445: These values being negative relates to sand loss from the surface (i.e., transport out of the plot or burial)? If this is the case, it seems to be obscuring what you are actually trying to measure (travel distances), and a different metric may be needed (or a better description as to why this one was used).

> We agree that -at first - this is confusing. However, we also think that the ability to have a negative distance is useful, exactly because it allows us to quantify the disappearance of the sand or, as the reviewer points out upslope rain splash. To avoid confusion, we now explicitly mention what a negative value for the transport distance means early on in the manuscript, on L.356-359 and L.572ff, and in the caption of Figure 9 (L.1103ff.).

Manuscript, line 451: It doesn't seem to match up all that well, and I'm not sure what the OF modeled from the DSM is adding.

> We thank the reviewer for the comment. We believe that the DSM analysis adds value to the study because it shows that the sand moved along the depressions in the surface and thus the expected flow path of the water (as also shown by the blue dye). However, we agree that it is only a very minor part of the analyses of this study (see also response to comment above).

Manuscript, line 524: So no movement was detected during the subsequent experiments? That's too bad.

We actually observed sediment movement during the subsequent experiments, but we think that this movement was inhibited by the gluing of the sand and the larger aggregates. Therefore, we could not compare the movement during the next experiments with the movement of the "fresh" sand during the first experiment. Therefore, we used another "fresh" sand ribbon (with a different color, e.g., Figure 8) for each experiment to overcome this limitation (as described in section 5.1).

Manuscript, line 540: I would like to see another discussion paragraph that discusses these results in light of landscape change following deglaciation in alpine environments. You could include a few other key studies that discuss the evolution of lateral moraines such as Curry et al. (2009): Curry, A. M., Sands, T. B., & Porter, P. R. (2009). Geotechnical controls on a steep lateral moraine undergoing paraglacial slope adjustment. Geological Society, London, Special Publications, 320(1), 181–197. https://doi.org/10.1144/SP320.12

We thank the reviewer for this useful comment. We agree that the manuscript would profit form a more in-depth discussion of landscape evolution after deglaciation and to put the characteristics of the plots more into this context. We limited this discussion initially because we had already written about this elsewhere (e.g., Musso et al., 2022; Greinwald et al., 2021b, Maier et al., 2020, 2022a, 2022b). To address this and the other reviewers' comment, we have now added more discussion on how the system changes after deglaciation and, thus, why the surface characteristics of the two moraines are different. We also point to other papers where these processes are described in more depth, including older moraines (L. 564-568 and L. 588-590). Moreover, we used the reference suggested by the reviewer to put the sediment transport patterns observed in this study into the context of the evolution of slope stability (L. 581).

Manuscript, line 568: Seems to be a fairly major limitation given the negative values in for delta D and delta A.

We refer to our previous responses regarding the negative values of ΔD and ΔA and what they mean. We agree that it is a disadvantage but also see the advantage of being able to quantify the disappearance of particles (and in this case, highlight the different type of sediment transport on the two moraines).

Manuscript, line 608: This could have been more clearly acknowledged and explained earlier. Are there other metrics that could be used that would not be so sensitive to a "loss" of grains?

We now state this earlier (on L.355ff.). We did a literature review on different methods that could be used to quantify the sand transport when we started the study and could not find any method that was not sensitive to either this loss of grains or very sensitive to the misclassification of particles or particles moving off the plot.

Manuscript, line 633: I think it would be nice to see those who contributed recognized by name if possible.

We thank the reviewer for the comment and agree, that the work of all students and interns involved was very valuable and should be honored. However, there were too many students involved in the different parts of the project to list all of them in the

paper. We are happy to refer you to the website of the project (www.hillscape.ch), which we also mention in the paper (L. 658), for more details on the overall project.