Response to Reviewer comments

Response to Reviewer 1

General Comments

Hartmann et al. present work on infiltration experiments across a moraine chronosequence in the Swiss alps, spanning almost 14,000 years. They performed infiltration experiments on four plots of different ages, with each plot being subdivided into three subplots where different precipitation intensities were applied.

The work is a heavily revised version of a previously submitted manuscript. I was one of the original reviewers back then and suggested a rejection. This submission was deemed different enough to be considered a new submission, and I would agree with this assessment. The way the authors reworked the current manuscript makes it much more enticing and sets it apart further from Hartmann 2020a and 2020b (in my opinion).

The authors would like to thank the reviewer for spending her/his time on reviewing our revised manuscript. We are pleased that the changes to the original manuscript were well received and appreciate the feedback and interesting comments.

In the introduction, the authors describe the need for identifying different flow patterns for potential integration into landscape evolution models. It would be interesting to revisit this idea in the discussion. What does the work suggest such an integration could look like, and more importantly, what would the practical differences be between the different flow types for such a model (especially considering that some of the differences between the plots appear minor, even though they were found to be significant)?

This is an interesting point, which we will include in the discussion of the revised manuscript.
First of all, we would like to state that we cannot derive any generally valid statements on the basis of the observations from this manuscript and Hartmann et al., 2020, since the observations are primarily only valid for the landscape development based on loose moraine material in siliceous and calcareous glacier forefields. Since our study made it clear that, due to the complexity of landscape development, time should not be regarded as the only primary evolutionary factor, it would be necessary to intensify correlation studies, such as those already vaguely tackled in Figure 11, in order to integrate a corresponding modelling approach in SLEMs. The aim of this study would be to identify the main influencing factors (e.g. soil properties, vegetation characteristics) on the formation of preferential flow paths and to create a kind of transfer function between properties and degree of preferentiality. The derived preferential flow frequency index (PFF) could be a helpful variable to quantify the extent of preferential water transport. The minor differences in the profiles between the plots would therefore not be of great importance.
How the final exact implementation of the description of the flow types in the model could look like largely depends on the desired complexity/accuracy of the target model (empirical/physically based).

I am also wondering if the amount of rocks has an effect on the flow type. If a large fraction of the profile is taken up by rocks, percolating water will be restricted to the space between the rocks. The authors do include flow types that take into account rocks in the profile. From what I understand, though, this applies mostly to homogeneous flow that happens around the rocks. What would happen if a larger rock led to an effective partitioning of an otherwise homogeneous wetting front? (That is, if the soil below the rock remained dry)

In the case of large rocks leading to partitioning / funneling of the wetting front, this funnel flow is automatically counted as finger-like flow within the image analysis (when the redirection of water leads to thinner flow paths). We will clarify in the revised manuscript that the flow type class finger flow refers to the water transport pattern and not specifically to the narrow sense of fingers created only by instabilities of the wetting front. Since we are primarily concerned with what the flow patterns look like and what they mean for water transport and not with a precise identification of the mechanisms behind the flow patterns, both flow types are classified in the same class of flow patterns. Both flow types lead to a fast and preferential vertical downward transport of the water by creating the same flow pattern.

Further, it appears that some profiles exhibited a significant portion of rocks in the upper soil layers. Overland flow was not measured, but it could be beneficial to talk a little more about the potential impact of less water infiltrating at these sites.
In the discussion, the authors mention very briefly that the edges of the plots were not analyzed. I might have missed this earlier, but does this only apply to the outer edges of the 1x1.5 m plots or also to the borders between plots 1 and 2 and plots 2 and 3? If so, how big of a buffer was included? I could imagine that interactions around the inner boundaries could have an impact, too.

To avoid a possible impact by interactions around the inner boundaries we excluded a soil space between neighboring subplots with a width of at least 6 cm from the analysis. We will include this information in the method section in the revised manuscript.

I think the revisions are a little more than just minor, but I am confident that the authors can address them.

Specific Comments

Page 4, Lines 9-11: What is the reasoning for having two plots of ~the same age?

The original reason for selecting the 110 year old moraine as the youngest and the 160 year old as the second youngest moraine was mainly the result of local conditions at the glacier forefield. The actual goal was to select age groups that were as identical as possible to the moraine ages selected in Hartmann et al., 2020a (30a, 160a, 3000a, 10000a). This was not entirely possible for the youngest moraine. The choice of the 110 year old moraine as the youngest moraine is the result of the local conditions, as no adequate moraine with an age of around 30 years could be identified that also ensured comparability in terms of elevation and microclimate. We therefore had to compromise and selected the moraine with an age of 110 years as our youngest moraine (Musso et al., 2019).

Page 7, Lines 24+ Can you describe what the practical differences are between the flow types?

We will include the following explanation in the revised manuscript:

(1) Macropore flow with low interaction: water flow in macropores, where a low permeable or saturated soil matrix leads mainly to water flow within the macropore and only small lateral interactions between the macropore water and the soil matrix occur. The characteristic dye pattern shows narrow but long individual stains.

(2) Mixed macropore flow (low and high interaction): water flow in macropores, where a heterogeneous soil matrix leads to a mix of low and high lateral interactions between the macropore water and the soil matrix. The characteristic dye pattern shows a mix of broader and thinner individual stains.

(3) Macropore flow with high interaction/finger flow: This class includes water flow in macropores, where a permeable soil matrix leads to high lateral interactions between the macropore water and the soil matrix. But also smaller sized finger-shaped flow paths in water repellent soils or at texture transition zones, and finger flow caused by instabilities of the wetting front. This flow type class also includes funnel flow, which results in finger-shaped flow paths.
The characteristic dye pattern shows broader vertical elongated coherent stains.

(4) Heterogeneous matrix flow/ finger flow: water flow in a heterogeneous soil, water repellent soil, coarse textured soils, or soils with texture transitions. Characteristic dye pattern shows an infiltration front that proceeds with instabilities

(5) Homogeneous matrix flow: water flow in permeable soil. Characteristic dye pattern shows a homogeneous staining of the soil matrix

Page 7, Lines 31 Do these indices depend on the effective width of the profile? If there is a flow restriction, for example from a rock, wouldn't that lead to a "compression" of the water flux through the narrower width? Is it possible that water from one experiment gets drawn into another subplot through matric forces?

The indices are related to the actual width of the profile. The presence of stones or larger rock can lead to a redirection or funneling of the water flow. If the water flow is redirected into thinner flow paths, funnel flow will have the same flow pattern as finger flow. Since the focus in our study is on the flow type patterns and not specifically on the mechanisms causing the patterns, we combine both flow type classes "funnel" and "finger flow" into the class finger flow. Thus, the presence of rocks does not interfere with our flow type classification based on the image analysis. We will include a more detailed definition of the flow type classes in the revised manuscript, which we also explain in further detail in the comments to Reviewer 2.

In theory, it would be possible for water to be drawn from one subplot into the neighboring subplot. Due to the high vertical conductivities and the relatively high antecedent soil moisture with correspondingly low matric potentials, this probability is considered to be low. However, to avoid possible boundary effects, i.e. interactions between the subplots or between the plots and the outer (not irrigated) boundaries, we excluded a buffer of at least 6 cm width between neighboring subplots and at least 5 cm to the outer plot boundaries from the image analysis. We will include this information in the revised manuscript.

Page 9, Lines 13 Given the low n, there is a chance that the trend is random, even if it's statistically significant, no?

We actually consider n between ~4400 and ~6700 to be quite sufficient for the statistical test to be reliable. To test for significant differences in observed infiltration depths among age classes and among irrigation intensities we used the non-parametric Mood's median test.
The Mood's median test compares median pairs of two or more groups. A p-value lower than 0.05 indicates that at least the median of one group is significantly different from the other groups. One group of observations consists of the observed maximum staining depth at each pixel column (~400) per profile (5) and plot (3). Which leads to groups with a size between n=~4400 and ~6700.

Fig 4: When there were rocks at or immediately below the surface, what happened to the water that couldn't access that space? Did it run off?

We observed infiltration of water next to the rocks at the surface and along the stones below the surface. So the rocks lead only to very local (micro-topography-scale) redistribution but not to runoff. Significant surface runoff only occurred at the bare plots at the young moraines. However, we relate the higher surface runoff here to the process of structural sealing, since the vegetated plots unaffected by structural sealing had a similar amount of rocks, but lower surface runoff.

Page 12, Lines 16-18: Which appears to be the case for most profiles…?

Yes this is the case for most of the profiles, but mostly pronounced in 110a: plot 3, 4.9ka: plot 1 and 2, 13.5k: plot 1. We consider to exclude the Pdye analysis in the revised manuscript as it adds little value (also noted by Reviewer 2).

Page 13, Lines 6-7 Is it really…?

There is still a reduction in SAD with depth, but SAD values in 50 and 80 cm at the older moraines tend to be higher than at the younger moraines.

Figure 6 and Page 14, Lines 6-7: Curious to read why sometimes SAD is greater for lower intensities and sometimes greater at higher intensities.

We discuss this issue on page 23-25:
We observed a decrease in SAD with intensity at all depths for the young moraines and an increase at the old moraines (50-60cm at 4.9ka and entire profile at 13.5ka).
We found, similarly to other studies investigating finger flow, that the number of fingers increases with higher flux rates.
At the rather homogeneous material of the young moraines we observed a decrease in SAD due to the merging of flow paths.

Page 18, flow type classification: A table with the percentages would be good here so that the reader doesn't have to piece together everything. Fig 10 is nice, but it is a little difficult to compare the length of the bar sections after "Matrix flow between rocks". Maybe something for an appendix.

We agree and will include a table with the frequency of the flow types in the revised manuscript.

Page 20, Fig 11: I'm wondering if the percentage of rocks in a profile affects this as well. This harkens back to my earlier comment in which way rocks affect all these flow characteristics.

The impact of rocks is an interesting point. The presence of rocks can divert water transport and create funnel flow, which we classify as finger flow (finger-shaped flow). The presence of larger stones can therefore lead to more preferential water transport. We would also have liked to have done a more in-depth analysis of the impact of stone content. However, a quantification of the real stone content in the soil for such an analysis is rather difficult due to the image analysis method used. The volume density profiles of the stones shown in Figure 4 in the manuscript are only subjective approximations, since the stones were defined manually during image analysis as described in the methods section. The difficulty lies in manually recording all the stones in the coarse moraine material that have a relevant influence on the flow paths.

Page 21, Lines 5-8: I was thinking about this the entire time while reading the manuscript. Can you estimate infiltrated volume or surface runoff? I would imagine the rocks close at the surface play a huge role here and not just the soil properties.

As mentioned above, stones at the surface usually only lead to very local, small scale redistribution and preferential infiltration instead of surface runoff. Surface runoff was only observed at the young moraines. Attempts to quantify this surface runoff unfortunately failed. Thus, we can only provide visual observations of the occurrence and relative amount of surface runoff. We also see the point that the stone content might impact the infiltration amounts especially at the youngest moraines with a high stone content at the surface.
However, in the manuscript we point out that we observed at both young moraines different amounts of runoff at different plots of the same age class, despite the plots having similar stone coverages. Stone coverage thus seems not to be the controlling factor here. Instead we relate the differences within the age classes to the vegetation cover and the soil properties.

Page 23, Lines 1-3: This makes sense to me. It could be a combination of both larger diameter and longer roots.

Indeed, the larger diameters are also a valid point, which we will include in the revised manuscript.

Page 23, Lines 10-16: I like that you bring this up. My initial interpretation would have been that the different external factors of the sites (which also affect landscape evolution) are more important than age.

This is unfortunately the drawback of the chronosequence approach in general. This space-for-time substitution approach assumes that for a sequence of sites (in this case moraines) with similar characteristics such as topography, climate, and parent material on which the soil was formed, time can be treated as the only variable. It is well known that the application of this chronosequence concept has some limitations as landscape development is much more complex than in form of a monotonic progression. The assumption that time is the only factor affecting soil development in a spatial sequence of soils is often the only option for a detailed historical tracking of landscape development at a particular location and thus still a fundamental tool for representing temporal changes in the Earth surface system (Phillips, 2015).

We will also include a statement regarding the assumptions of the chronosequence approach in the methods section of the revised manuscript.

Page 23, Lines 30-31: Didn't you argue on the previous page that hydrophobicity could affect the infiltration patterns…?

That is correct. Despite the fact the matric potential did not drop below field capacity we believe that the release of hydrophobic compounds during the decay of litter or by root activities create a hydrophobic soil matrix, which is due to variations in soil moisture from time to time more or less pronounced.

Page 25, Lines 29-30: This is an important point that needs to be included in the methods (unless I missed it somehow). Does this only refer to the outer edges or to the inner boundaries as well? How much was excluded?

This also accounts for the inner boundaries. In between neighboring subplots a buffer with a width of 6 cm was excluded from the analysis. We will include this information in the methods section in the revised manuscript.

Musso, A., Lamorski, K., Sławi´nski, C., Geitner, C., Hunt, A., Greinwald, K., and Egli, M., 2019: Evolution of soil pores and their characteristics in a siliceous and calcareous proglacial area, CATENA, 182, 104 154, https://doi.org/10.1016/j.catena.2019.104154.