Response to Reviewer comments

Response to Reviewer 2

General Comments

Hartmann et al. present a generally interesting study on infiltration capacities across a moraine chronosequence, where each chronosequence is divided into three levels of vegetation cover complexity and receives three different water application intensities, resulting in 36 different water applications. However, the experiment appears to be (more or less) a replica of a previous study (Hartmann et al. 2020a&b), with the main difference apparently being the parent material, which is calcareous in this manuscript and siliceous in the previous ones, and an apparent focus on vegetation and rainfall intensity influences.
Even some of the figures are largely identical. It is not entirely clear to me what the new contribution of this manuscript is over the previously published study.
Additionally, I do have some concerns with the general study layout and possible interpretations. Each plot is divided into three 50cm wide zones with different rainfall application intensities. These zones are not physically separated from each other and to prevent interaction during application in one zone, the remaining two are covered. This still leaves room for interaction near the zone boundaries where water can be drawn laterally into the drier soil of a neighboring zone. The authors acknowledge as well (see below) that overland flow on some plots might have infiltrated near the zone boundaries, leading to increased infiltration there. Then there is the question of the ages in the chronosequence. The two younger sites are 110 and 160 years old. 50 years difference is not much in a soil chronosequence, especially considering that the other soils are 4900 und 13000 Jahre alt. Unless I missed it, I did not see an explanation of what the authors expect in those 50 years to have happened to the soil.
If this were the authors' sole publication on the topic, I would probably just consider major revisions (i.e., shortening and some restructuring). Given the other two publications, I am having difficulty seeing the novelty in this manuscript, though, and am unfortunately leaning toward rejection.

Response to General Comments

The authors would like to thank the reviewer for spending her/his time on this review.

We respect but regret the decision of the reviewer not to support a publication of our manuscript in HESS.

As we do not agree with this assessment and are convinced that this is largely based on a misunderstanding we provide detailed and hopefully convincing responses to the general and specific comments below.

First of all, we would like to emphasize once again that our study is deliberately a follow-up study. In our first study on the co-evolution of flow paths and soil properties along a chronosequence of hillslopes in a glacial forefield, the experiments were carried out on siliceous parent material. To our knowledge this was the first systematic study on subsurface hydrologic flow path evolution during the first 10000 years of landscape evolution.
The role of hydrologic processes, especially subsurface preferential flow, is mainly missing in soil and landscape evolution modeling, which is mostly due to the lack of observations on temporal changes and dynamics of subsurface flow paths (van der Meij et al., 2018).

With our first study we provided rare data and observations on this topic. However, it is also a very important scientific practice to replicate such a study in different geologies for the verification and generalization of the gained knowledge. Simply taking this single study and assuming that the same processes/evolution occur in a totally different geology seems risky and unscientific. In our opinion, this applies above all to the complex interplay within the hydro-pedo-geomorphological system, where the parent material has a significant influence. Due to our follow-up study it became clear, that parent material does indeed matter in soil development and flow path evolution, since we found significant differences in flow paths after 10000 years of landscape evolution. This is an important finding for the investigation of the feedback cycle of the hydro-pedogeomorphological system. We will improve the highlighting of these findings in the revised manuscript, since it was obviously not clear enough.

To facilitate comparisons between the two studies, we decided to keep the design of the graphics from the first study for reasons of consistency. This should make it easier for the reader to compare the data and results from both publications with one another. The basic design of some graphics is therefore identical in both publications; the data presented therein are not. The contribution of this study lies clearly in the completion of the rare observations when it comes to the development of subsurface hydrology within landscape development. Our two studies provide these rare data and observations, which will help to ensure proper handling of (subsurface) hydrologic processes and their role within the feedback cycle of the hydro-pedo-geomorphological system when it comes to soil and landscape evolution modeling.

We would also like to make it clear again that the experiments in both studies are quite similar, but differ in the following points: In addition to the development of the preferential flow paths with moraines age (investigated in both studies), the development of flow paths depending on irrigation amount was the second focus in the first study, while the current study focused on the irrigation intensity. Furthermore, within this current study we also set the focus on the influence of vegetation complexity. This was only touched on very superficially in the previous study.

Reasons why we claim that the here submitted study is a valuable and novel contribution to scientific knowledge (despite the previous study in a different geology (study 1) and the previous publication of the raw soil physical data in a data publication):

a) The focus of the two studies differs: age sequence and irrigation *amount* in study 1, age sequence, irrigation *intensity* and vegetation complexity in study 2.
b) The parent material of the previous study is siliceous, the parent material here is calcareous. Assuming that all geologies result in similar processes and developments is dangerous and we show here that this assumption is wrong. Given that study 1 was praised as novel from a pedological perspective and interesting to the community and the data set is not only unique, but also essential for any quantitative modelling of water and element balances of such soil ecosystems and highly relevant also for neighboring disciplines we are convinced that a follow-up study in a different geology and with a different focus is still innovative and worth publishing.
c) The smaller age difference between the two youngest moraines in study 2 furthermore shows that significant development and changes can already occur over a period of only 50 years.
d) The soil physical data of the site was published as a data publication. However, data publications do not contain any interpretation and usually no statistical analysis. In the here submitted study 2, we present statistical tests showing which differences between classes and variables are statistically significant and which are not. We also show different groupings compared to the overview plots of the data publication.
e) The similar design of some of the figures in study 1 and 2 is supposed to facilitate the comparison of the two studies and should not be mistaken as a lack of novelty.
f) Additional methodological novelty: Study 2 furthermore contains a novel comparison of the dye profiles based on the bootstrapped LOESS regression (BLR). This approach to compare two data

sets of profile observations was first developed by Keith et al. (2016) who used it to compare profiles of soil organic carbon. The method has never been applied to dye profiles of irrigation experiments.

With regard to the study design, as correctly described, we occasionally suspected that at plots with surfaces influenced by structural sealing (as a result of the high intensity of sprinkling), surface runoff from the irrigated plot onto neighboring, non-irrigated areas occurred. This occurred to a small extent in 5 out of 36 irrigation experiments. In general, after each irrigation, a possible runoff to neighboring areas was checked, but this was only observed at the plots with very sparse vegetation (i.e. the youngest plots). Thus, we can rule out a general influence. We will clarify this in the revised manuscript on page 26, specifically providing the information that only 5 out of 36 experiments were affected.

Our study is an interdisciplinary study in which we work together with other disciplines. Therefore, the selection of the age classes was not only based on hydrological aspects, but also under aspects of geobotany and geomorphology.
Regarding the age gap between the two youngest moraines in this study, we want to point out that we found large changes between the 30 and the 160 year old moraines in our previous study, as developments are especially fast during these early stages. Even between the two youngest moraines in the here presented study which only have an age difference of 50 years, significant changes in flow responses could be observed (Figures 8, 9, A1). We will highlight these short-term changes more clearly in the revised manuscript as this is indeed another piece of important additional information: Changes over a short time span of only 50 years are significant. However, the original reason of selecting the 110 year old as youngest moraine was mainly the result of the local conditions at this site. The actual goal was to select age groups that were as identical as possible in both chronosequences (i.e. the two geologies). This was not entirely possible for the youngest moraines at both locations (with ages of 30 years at siliceous parent material and 110 years at calcareous parent material). The choice of the 110 year old moraine as the youngest moraine is the result of the local conditions at this site, as no adequate moraine with an age of 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 at this site (Musso et al., 2019).

The reviewer suggests shortening and restructuring of the manuscript. We will thoroughly revise the manuscript with this in mind, while at the same time providing all relevant information.

Specific Comments

Moraines are a special type of cover and pedogenesis. Can you hypothesize what can be expected in soils that formed not from direct glacial processes?

The results of our two studies have shown that the observations cannot be simply generalized to other locations. The combination of our two studies has shown that the parent material has a significant influence on the dynamics of subsurface flow paths. If we consider soils that do not form on glacial till, it can also be assumed that other time scales must be considered, since weathering may not progress as quickly as with loose glacial till. In addition, other influencing factors such as vegetation and climate must be taken into account. We assume that at least in the upper centimeters, where the influence of weathering is strongest and also leads to a strong reduction in the grain size of other raw materials, heterogeneous flow patterns with a high proportion of finger flow might are dominant after a few years, which might transits to macropore flow at highly developed soils. We will include a sentence in this respect in the revised manuscript.

Fig 5: Are the figures the mean of the five excavated profiles?

Yes, it is the mean volume density as described on page 7, line 16. We will also include this information in the figure caption in the revised manuscript.

P21 L10-11: Is this purely based on the parent material or maybe also a function of landscape position, e.g., aspect, slope, etc.?

The difference is largely due to the different parent material and the associated soil chemistry. Other environmental factors were kept as similar as possible. We will clarify this in the revised manuscript.

P22 L26-28: For the sake of comparability, would the "finger flow and macropore flow (high interaction)" class be classified as macropore flow in the previous study? (only based on patterns, even in the absence of actual macropores)

This might have been a misunderstanding: We used the same classification scheme in both studies, which makes the flow type distributions directly comparable. In both studies the major component of this joint flow type class was finger flow.

A few more details on this flow type class: We introduced this joint flow type class in our previous study as mentioned on page 22 line 27 and explained on page 7 line 31-33, since in our previous study we observed narrow finger like flow paths that were misclassified as macropore flow with high interactions when using the original classification scheme. Macropores with high interactions could not be verified based on the photographs. In order not to completely rule out their occurrence in alpine soils, we decided to put both flow types in a joint class and to identify the major component based on the observations.

P22 L30-32: Given that the top layers contain hydrophobic material in both studies and no overland flow is observed, wouldn't that suggest that most water should make it through this hydrophobic layer?

That is correct. Since little or no overland flow was observed, the water must have infiltrated vertically through the hydrophobic layer. We did not assume that the hydrophobicity of the material generally prevents infiltration, but is also heterogeneous and together with the microtopography of the surface leads to heterogeneous infiltration patterns and thus to the increased development of preferential flow paths in the form of finger flow. We will clarify this in the revised manuscript.

P26 L18-19: This raises a question about the experiment setup. If I understand correctly, the zones for different application intensities were neither separated by a non-irrigated space in between, not by some barrier installed into the soil profile that could have prevented surface flow onto the adjacent zone? If this is the case, couldn't it be possible that the areas where the transition from one zone to the next happens simply receive more water than the rest of the zones? If deeper infiltration is observed below the transitions, that could be a result of more water infiltrating and thus being able to reach greater depths.

During the irrigation experiment we covered the non-irrigated neighboring subplots with a tarpaulin that physically protects the surface from the irrigation water. At the youngest moraines, where the direct application of water by spraying might have caused structural sealing on the currently irrigated (bare) plot, some of this water moved laterally along the surface (and under the tarpaulin) onto the neighboring plot which was still covered and thus not affected by spraying and the resulting structural sealing. So in these locations there might have been slightly more water input, but also, we here see the infiltration under less disturbed circumstances. However, the amount of water running off to the sides is not substantial due to the low inclination of the plots to the sides. We will include this information in the revised manuscript.
Nevertheless, we checked for this possible lateral runoff to neighboring plots after each irrigation, but only observed this to a small extent at bare plots at the young moraines. This was an issue observed in 5 out of 36 experiments. However, the deep percolation at the transition zones thus show that rapid vertical deep infiltration is possible at this age class, when the soil surface is unaffected by sealing as a result of the spraying action.

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