Response to Reviewer comments

Response to Reviewer 1

General Comments

Having conducted research on subsurface flow, in particular preferential flow, for many years, I was very interested to review this paper. I always like to see such investigations that include detailed and well-planned field work. The senior author is commended for this detailed staining field work, but the linkages with other aspects of the study are weak and almost ad-hoc. Also, the planning of this study could have been improved (see comments that follow).

In reading through this paper, I found many similarities with the other two papers recently published by these authors (i.e., Hartmann et al., 2020a,b), one in this same journal and another in Earth System Sci. Data. We really need to be conscious about not republishing similar material; I am sure that was not the intent here, but this paper does come across as having at least some evidence of this issue. I fully realize that new infiltration studies were conducted as part of the paper I reviewed, but the authors did not do a good job in showing how these new findings helped address the key stated issue of the feedback cycle of the hydro-pedo-geomorphological system associated with these glacial chronosequences. As such, the current manuscript is overly wordy and repetitive, and poorly links the dye experiment with the vegetation complexes and soil properties, partly because of the mismatch of scales. Thus, I do not see this paper as acceptable in HESS and recommend that the authors carve out the new piece on the dye staining experiment and submit this elsewhere as a short note.

Response to General Comments

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

We regret that the reviewer was left with the impression that the study presented here as a follow-on to our previous study on different bedrock material does not provide sufficient novelty. Nevertheless, we are convinced that there is value in our study and will reply to both the general assessment as well as the specific comments below.

First of all, the similarity of the experiments presented here with our study previously published in HESS (Hartmann et al., 2020a) was fully intentional. To our knowledge Hartmann et al., 2020a was the first systematic study of the co-evolution of flow paths and soil properties along a chronosequence of hillslopes in a glacial forefield developed on siliceous parent material. The replication of such a study at a site with differing parent material is of great importance for the verification and generalization of the gained knowledge. 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. It was therefore very important for us to make it clear within the manuscript that this is a follow on study on calcareous parent material, which therefore necessarily has corresponding similarities to the study on siliceous parent material. For this reason, we again chose HESS for this submission, in order to facilitate the readers' access to both parts of the study. It should also be noted that two slightly different angles were used in the two studies. 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 looked at the dependence on the irrigation intensity.

In the specific comments, the reviewer adds the justified comment that the results of both field campaigns could have been processed in a joint paper. However, such a single publication would have meant to cut down on the details of the results significantly and focusing only on those which are directly comparable. But given that these types of systematic studies are exceedingly rare we felt that the community was better served by providing the full picture of results found at each of the study sites which necessarily meant to split it into two separate submissions to avoid an excessively long publication.

Furthermore, within this study we also set the focus on the influence of vegetation complexity. This was only considered tangentially in the previous study. The reviewer rightly argues that the different scales of the vegetation complexity for soil sampling and flow path determination appear poorly designed and make a concrete evaluation with regard to this aspect difficult. Our study is an interdisciplinary study in which, due to the complexity of the hydro-pedo-geomorphological system, we not only rely on our own expertise, but also work together with other disciplines such as geobotany, geography and geomorphology. While the advantage of this collaboration is the interdisciplinary exchange of knowledge, there is also the fact that tradeoffs have to be made when designing experiments, partly due to competing interests.
Even if we would have used other aspects from hydrologist point of view for the experimental design and the plot selection, it was of interest to us to use the new data set to investigate whether a common geobotanical proxy for plot selection (combining surface and subsurface vegetation characteristics such as coverage, root type, etc.) has an influence on our observations of soil properties and flow paths. Even though this specific analysis did not produce any outstanding findings, the fact that we did not find significant effects of vegetation is also an important result and we see the discussion of this issue as fruitful, especially in the context of the need for interdisciplinary studies of the hydrological system.
In addition, the reviewer correctly notes that the soil data have already been published in Hartmann et al., 2020b. However, this publication is a pure data publication that only supplies the data set to the community and does not contain any analysis and evaluation/interpretations of the results. The first time that this was done was within this manuscript. Furthermore, the data publication shows a depth-differentiated representation of the data set at each age class, while here we represent a depth-averaged breakdown with regard to the vegetation complexity.

We furthermore take the reviewers' comments on the wordiness very seriously and will definitely address this issue in a revised manuscript.

Specific Comments

The Abstract of this paper needs to focus on the key findings. Currently it focuses most on methodology.

We agree with this statement and will improve the abstract accordingly.

Section 1 – Introduction

Pg. 2, L. 12-13: The statement about preferential flow occurring more often at high rainfall intensities really depends on the environment you are working in and the type of preferential flow pathways that exist. In drylands where Hortonian overland flow dominates, few opportunities for subsurface preferential flow may exist, unless large cracks appear on the land surface that intercept surface runoff and route this into the subsurface.

We see the reviewers point and will clarify the sentence in that we are actually talking only about humid areas. Our and the referenced studies were restricted to humid regions.

Pg. 2, L. 19-28: This entire paragraph could simply be restated that most past chronosequence studies where soil development has been investigated are 2-dimensional – i.e., they only examine pedons, not 3-D flow paths, which is basically what was done in this study.

We would prefer to provide the more detailed information on this different aspects covered by previous studies. We are furthermore slightly reluctant to claim that we studied 3-D flow paths as due to the way we excavated the soil profiles the focus is more on a 2-D evaluation, at best quasi-3-D.

Pg. 2-3 last paragraph on pg. 2: You need a transition to this paragraph. Importantly, the reference to your previous and very similar paper in HESS (Hartmann et al., 2020a) summarizes that "observed flow types changed from a rather homogeneous rapid matrix flow in coarse material at the youngest moraine to a mainly finger flow at the medium-age moraines". Given the small plots and the methods for establishing 'horizontal connectivity' among excavated slices of the profile (referenced in a difficult to access thesis), I am not convinced of the generalization you make – particularly the assumption that mainly finger flow occurs in 'medium-age' moraines. Given the heterogeneity that occurs in moraine material, there certainly could have been some preferential flow paths that were missed (at larger scales). Even in compact glacial tills, preferential flow can occur in glaciotectonic fractures and desiccation fractures. It seems to me that while your inferences related to vertical preferential flow are quite good, those related to what you call horizontal flow are rather suspect. I fully realize that such 'horizontal' pathways are difficult to quantify, but here you stress these differences. I really think you could have combined these two papers as they are closely related; this would have reduced the repetitive material in the two papers and made for a much more comprehensive story.

We would like to clarify that we focused on vertical flow paths in both studies. We never made any references to horizontal pathways and it is also not mentioned in the paragraph addressed by the comment. In addition, we are not sure we understand the reviewer's point here as we did find preferential flow at the medium age moraines. Does the comment refer to the fact that we did not identify fractures or macropores but finger flow as the dominant feature? Our experimental design was aimed at covering the expected spatial heterogeneity by repeating the experiments in three different locations on each moraine. As it is nevertheless possible that fractures were present elsewhere on the moraines we refrain from stating that fractures are non-existent. However, extensive excavations and core profiles at additional locations on the same moraines as part of other experiments of this interdisciplinary effort did not reveal anything pointing at the importance of fractures. Desiccation fractures are unlikely due to the high soil moisture content throughout the study.

Pg. 3, L. 5-18: Here you spend an entire paragraph trying to justify why this paper is different from Hartmann et al. (2020a). In the previous paper, you also discuss vegetation effects, and I would pose the question: how is it possible not to consider vegetation (particularly below ground biomass) in discussions related to subsurface flow pathways? And the issue of irrigation intensity is not generalizable, as noted before.

That is correct. We made some effort to clarify to the reader that this is a follow up study which is in some aspects similar to the first study, but also has its unique features. In the previous study we also mentioned vegetation effects, but covered the vegetation complexity only tangentially. As already mentioned in the response to the general comment, it was of interest to us to investigate the impact of a vegetation proxy that combines both surface and subsurface vegetation characteristics in more detail,

since subsurface vegetation characteristics are often neglected when investigating subsurface flow paths in relation to vegetation coverage (Bachmaier et al., 2009, Stumpp and Maloszewski, 2010, Kan et al., 2020).
Even though the impact of irrigation intensity might be not generalizable, it surely is of interest for a specific landscape, environment, etc.

Pg. 3, L. 19-23: This material belongs in the methods section.

We believe that it is appropriate to briefly mention the applied methods in the introduction. But we agree to shorten this methodological description to a single sentence: "We used a statistical method specifically designed for the comparison of soil profiles (Keith et al., 2016) to assess the statistical significance of the differences across the observed dye profiles."

Pg. 3, L. 24-28: Not a well-articulated objective statement if that was what this was intended to be.

This paragraph was not intended to be the objective statement but was meant to provide background on the interdisciplinary study that the dye tracer experiments were part of. To clarify we will rephrase and move this section further up in the introduction.

Section 2.2: This entire section is very poorly connected with the stated objective related to subsurface flow paths. It is not clear why the 'structural vegetation complexity measure' was adopted, nor how this is linked to subsurface flow paths. The second paragraph begins with mentioning soil sampling, but then reverts to vegetation surveys; this is very disorganized and does not connect with subsurface flow. Many issues that seem not relevant to the discussion of subsurface flow are reported here, leaving me to wonder if this latter study (the one I reviewed) was an afterthought.

We agree that this section might have been too heavily focused on explaining the details of the vegetation complexity measure which might be distracting from the main goals of the study. We will completely revise this section to make it more easily accessible and to improve the logical reasoning behind the use of this measure as well as the logical flow of the text describing the sampling and surveying.
However, we do assume that a strong link between vegetation, soil properties, and subsurface flow paths exists as it was also previously shown that root activities and vegetation composition have an impact on weathering and soil water transport (Ivory et al., 2014; Amundson et al., 2007). The measure of vegetation complexity combines both, surface characteristics (vegetation coverage) and functional aspects of the vegetation in one measure. As it includes habitat composition and underground plant traits we hypothesized that it would be helpful in the framework of our study.

Section 2.3: It appears this dye study was conducted a year after the study reported in Hartmann et al. (2020a). However, the same plot design was used (subplots were 0.5 m x 1 m) and this raises the concerns I mentioned previously about difficulties in assessing horizontal preferential flow paths (especially across larger scales). These scale issues also affect other flow pathways, and this can be related to the variable infiltration rates used; the pathways that emerge may thus not be representative of larger scale behavior. Finally, no shortcomings of the Brilliant Blue dye methodology related to soil flow pathways was mentioned – this has been reported in numerous studies. At least this needs to be mentioned.

We have again to make clear that we only used this method to analyze vertical preferential flow paths. We never stated that this method would provide us insights on horizontal preferential flow paths, nor did we draw any conclusion on horizontal preferential flow paths. Our field observations and further measurements (including vertical hydraulic conductivity, retention curves) indicate that in the fast draining soils the vertical water transport was the dominant process.
Regarding the draw backs of the well-established brilliant blue dye method, we are fully aware of its drawbacks and will extend their current discussion in section 4.5 by also referring to the shortcomings brought up by previous studies.

Section 2.4: Repetitive from the Hartmann et al. (2020a) paper which in turn was repetitive from Weiler (2001).

That is correct. In both studies the same method was used, since this is a well-established method to analyze brilliant blue dye tracer images and to classify flow types. For the sake of readers who are not familiar with this method we described it here again.
However, we are also open to the possibility of strongly shortening this section to basically citing the used methods and only pointing out specific differences.

Section 2.5: It is completely unclear how disturbed soil samples and soil cores can help reconstruct subsurface flow pathways. Of course, they can give an indication of vertical soil water movement, but not horizontal pathways.

There seems to be a major misunderstanding with respect to the topic of horizontal pathways: as previously stated we only investigated the characteristics of vertical flow paths. We will make sure to clarify this even more in the revised manuscript.
The soil samples were collected for the analyses of soil texture and physical and hydraulic properties since we evaluate the flow path evolution together with soil development and we also think that looking at the soil properties is important when investigating subsurface flow paths.

Section 2.6: Please remember, dye coverage does not equate to flux.

We are not sure how this comment relates to section 2.6 which is about the statistical analysis. However, we are well aware that dye coverage does not quantitatively equate to flux. We will make sure to clearly state this in the revised manuscript.

Section 3.1: I see no connection between these values of vegetation complexity and subsurface flow paths; again, this seems like data looking for a hypothesis link.

As one of our research questions was on the potential usefulness of this geobotanical proxy, that lumps together habitat composition and underground plant traits in explaining subsurface flow paths, we think it is important to provide the results of the vegetation complexity determination. However, we agree that it might be misleading to provide these details here and will move the description of the plot characteristics into the methods section, i.e. we will move and revise the entire section 3.1.

Section 3.2: Much of this section was already covered in the previous two papers by these authors and, once again, other than the connection made between selected soil properties and vegetation complexity, there is no connection to subsurface flow.

In this section we show the results of the soil analyses of the soil samples taken from the choronosequence on the calcareous parent material differentiated by the vegetation complexity. These results got nothing to do with the results from Hartmann et al. (2020a), since they were taken from a completely different chronosequence in a different geology. This first study was conducted on siliceous parent material and it would be dangerous to assume that both soils develop the same way given the strongly different parent material. As soil characteristics can strongly control subsurface flow this soil development has a strong link to the evolution of subsurface flow paths and it therefore seems important to report on the evolution of the soil physical characteristics along the chronosequence. The connection to subsurface flow is addressed in the discussion of the evolution of subsurface flow paths (section 4.2).
Also the data shown in Hartmann et al. (2020b), are not similar. In Hartmann et al. (2020b) the data are published differentiated by depth for the entire moraine and are not interpreted or analysed further, since it is a data publication. The data set presented here is differentiated by vegetation complexity to investigate its possible impact and statistical analyses are carried out both along the age gradient as well as along the complexity gradient as vegetation is known to have an impact on soil weathering.

Section 3.3: These are the most interesting findings of this study and seem to be the most unique findings – i.e., not addressed in the previous two papers. That said, there remain issues of scale associated with the small size of these plots and how representative these are of the broader vegetation complexes and the inferences made herein. Possibly the authors could consider publishing only this part of the paper as a note. The rest of the paper does not strike me as unique. Also, as stated, this part only refers to vertical flow paths and thus has interpretative limits. Please see my comments in several places related to the artificial irrigation applications and the difficulties generalizing these findings as well.

We are glad that the reviewer finds our results of brilliant blue flow pattern interesting. However, we are reluctant to reduce this study to only reporting our observations on the evolution of subsurface flow patterns. We feel that in terms of providing the full picture of flow path evolution we have to accompany these results also with the investigation of soil properties, since these play a crucial role in subsurface water transport. Given the scarceness of these types of detailed studies on flow path evolution as well as the evolution of soil physical characteristics we believe that this second chronosequence study on different parent material is also worthy of publication.

Section 3.4: In an abbreviated version of the paper, which seems appropriate, this section would then be the Discussion. The scale limitations would need to be discussed here.

See our response above why we think it is important to also report the evolution of the soil physical characteristics and thus not abbreviate the manuscript as suggested in the previous comment. We will, however, add a brief discussion on scaling issues to our discussion of the uncertainties in section 4.5.

Section 4.1: This is mostly a rehash of older research, including what has already been presented in these authors papers. Most is not needed.

We find that it is important to put our results into the context of the results of previous studies. This also includes our previous study on siliceous parent material as we cannot simply assume that the soils on different parent materials evolve the same. However, we can easily shorten the comparison with our previous study (which we provided for the sake of completeness) if the editor agrees with this way forward.

Section 4.2: Again, very little new here, compared to what these authors have previously reported. A much more concise version of the material presented from pg. 21, L. 32 to Pg. 22, L. 32 could be included in the Discussion of a modified note or paper, but much of the speculative material and inferences should be removed – e.g., some of the assumption about hydrophobicity, which was not tested.

As stated above we are willing to shorten the comparison with our previous study if the editor agrees that this will improve the manuscript. However, we do think it is important to relate back to these previous results.
Furthermore, our references to possible impacts by hydrophobicity is not just an assumption, since it was actually measured by Maier et al. (2019) and Maier and van Meerveld (2021, WRR in review) and showed and increase with increasing moraine age analog to the increase in preferential flow paths. We will include the reference to these findings in the revised manuscript.

Section 4.3 – Soil structure and texture: Again, this was somewhat covered in the authors prior papers. They mention the inadequacy of the two soil sampling locations relative to the vegetation complexes, and there is still no connection to subsurface flow. I feel this section adds very little.

Again, there seems to be a misunderstanding. While sections 4.1 and 4.2 focus on the evolution of soil characteristics and flow paths with age, sections 4.3 and 4.4 discuss their relationship with vegetation complexity. Neither of the previous papers went into any details in this respect. In section 4.3, it was not our intention to make a connection to subsurface flow. As we state several times and also make clear within our manuscript, we investigate both the soil development as well as the evolution of the subsurface flow paths. We also stated that it is well known that vegetation has an impact on soil weathering. For that reason, it was our intention to investigate whether different findings in soil properties can be related to vegetation complexity. Of course we had to discuss the mismatch of soil sampling sites to the scale of vegetation complexity estimation and the heterogeneity of vegetation complexity within this scale.

Section 4.3 – Subsurface flow paths: This is mostly a repeat of what was presented earlier in this paper. It does not address 3-D flow, rather vertical pathways. Problems with the small plot size in glaciated terrain should have been anticipated prior to designing this experiment – e.g., the large boulders; pg. 25, L. 12-14). Furthermore, the description of the cause of overland flow that occurred during irrigation (pg. 25, L. 1-10) brings into question the artificial irrigation scheme; the explanation provided is does not address this, but rather focuses on the well-known process of surface sealing with no evidence presented. Also, why was this phenomon not observed in the previous study (again, not well explained)?

Please also see our response to the comment above. We only intended to use these dye tracer experiments to investigate vertical subsurface flow paths and never claimed to fully resolve 3D flow. We also argue that our plot size is sufficient for our research goal. It is furthermore also a tradeoff between the desired scale of flow path observation and the required effort for excavation and minimum level of landscape disturbance. However, as stated above we will add a brief discussion on scaling issues to the revised manuscript. Our interdisciplinary field campaign required in both studies a very large number of profile excavations, but only in two cases large sized boulders were found to interfere with the experiment evaluation.
We discuss the impact of artificial irrigation in section 4.5 and will extend this discussion according to our response above.
We also explain why we think, that structural sealing only occurred in this study (calcareous parent material) and not in the previous study (siliceous parent material) by referring to the higher clay content and lower vegetation coverage at the respective moraine at the calcareous parent material. Both crucial points for initiation of structural sealing. In a revised manuscript, we will underline our assumption with the findings of a lower aggregate stability at the moraines of calcareous parent material (Greinwald et al., 2020 in review), which was measured during our field campaign.
Additionally the phenomenon of structural sealing was also observed by Maier and van Meerveld (2021, WRR in review) during larger scale irrigation experiments (briefly mentioned in the method section) on the same moraines in very close proximity to our experimental plots.

Section 4.4: You simply cannot compare the effects of different rain intensities in different biogeoclimatic areas and with different application methods. Thus, this section is of little value.

One of our research questions was focused on the potential impact of irrigation intensity on subsurface flow paths for our study site. We therefore think it is justified and desirable in scientific practice to look at what other studies have found on this topic.
In this section, we also discussed whether our results are directly comparable with those of previous studies. We agree with the reviewer that it is advisable to add 1-2 sentences on the difficulty of comparing the impact of rainfall intensities in different biogeoclimatic areas and will do so in the revised manuscript.

Section 4.5: Pg. 26, L. 9-13, Finally there is some mention of the drawbacks of using Brilliant Blue dye. Also, there is some acknowledgement of the obvious role that high energy water applications had on surface sealing (but why only in this study?) (pg. 26, L. 16-25). This was undoubtedly a major problem in this study design.

As we described in line 16 page 26, due to weather conditions the sprinkler had to be guided close to the soil surface at the two youngest moraines, which was not the case in the previous study. As already described in the manuscript and here in the response above: due to lower clay contents and a higher vegetation coverage we did not encounter this problem during the first study conducted on siliceous parent material, which again underlines our statement that parent material matters and a follow on study on a different geology is worth showing.

Section 5: All previous comments apply; in addition, while the statement on pg. 26, L. 30 may be true ("This shows that the influence of preferential flow paths increases with soil age"), the lack of robust 3-D evidence and a larger-scale perspective put this in question. I did not think the authors made a good connection (at appropriate scales) between vegetation complexity and subsurface flow paths, but now this is stated in the Conclusions as "We saw a direct relationship between vegetation complexity and subsurface flow paths at the old moraines and a relationship of vegetation complexity and soil properties at the 110, 4 900, and 13 500-year-old moraines" – this simply was not verified. Certainly, some inference could be made for vertical pathways, but even these were rather subjective. One of the concluding sentences – "…we still suggest that a more in-depth consideration of vegetation characteristics beyond coverage and land use types will provide useful insights for hydrological process research", leaves the reader hanging and asking what was really accomplished here that was not reported in the previous two papers by these authors. And I do not see convincing evidence of the stated feedback cycle of the hydro-pedogeomorphological system.

While we do agree that we should rephrase the first sentence cited above to only refer to "vertical preferential flow paths", we do not agree with the overall gist of this comment. In this follow on study we conducted a study on a glacier forefield of calcareous parent material. We see from our results but also from previous studies by different authors that parent material matters in soil development and flow path evolution and that it is worth repeating similar studies on different geologies to get a better understanding of the feedback cycle of the hydro-pedogeomorphological system.

With our work in both glacier forefields, we provide important but currently still 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.

The repetition of our experiments in a different environment is part of a scientifically recognized practice, to avoid undue generalization of the knowledge gained at one specific site and to identify possible influencing factors. We are thus convinced that it is worth providing these additional insights and observations to the community (similarly as hydrological catchment studies have been carried out in various catchments with different characteristics to investigate different controls on runoff generation).

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