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Comment on hess-2021-242

Anonymous Referee #1

Referee comment on "The impact of soil development, rainfall intensity and vegetation complexity on subsurface flow paths along a glacial chronosequence of 10 millennia" by Anne Hartmann et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-242-RC1, 2021

Reviewer comments on the paper "The impact of soil development, rainfall intensity and vegetation complexity on subsurface flow paths along a glacial chronosequence of 10 millennia" by Hartmann et al. submitted to HESS.

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.

My more specific comments follow:

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

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.

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.

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.

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.

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

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

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.

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.

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

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.

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

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.

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.

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.

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.

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.

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.

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.

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)?

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.

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.

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-pedo-geomorphological system.