Response to Reviewer comments

Response to Reviewer 2, Nicholas Jarvis

General Comments

This paper presents the results of dye tracing experiments showing how the mechanisms of preferential flow (PF) change with soil development in alpine moraines exposed by glacial retreat. This work culminates in a very nice schematic diagram (Figure 9) that summarizes and illustrates the main findings. The main strength of the paper is that the study of PF from this kind of pedological perspective is still really quite novel.

Nevertheless, the authors are not the first researchers to have taken this kind of pedological approach and it would strengthen the paper if some of this relevant earlier work could be mentioned in the Introduction. The authors could check out Quisenberry et al. (1993), Lin (2003), Cammaraat and Kooijman (2009) and Jarvis et al. (2102).

Another interesting and rather novel aspect of the paper is the demonstration of the importance of stones and rocks for generating and maintaining preferential flow. Maybe the authors could also cite Bogner et al. (2014), who demonstrated the same thing.

The paper is generally well written and presented and easy to read, although the language could be improved further by a native speaker. One minor concern is that the author's use of terminology related to PF is, at times, unnecessarily confusing. I have two other criticisms. First, the methods are not described in sufficient detail. Secondly, the authors could do a better job of discussing their results with respect to the fundamental processes causing the observed changes in flow patterns. These aspects are explained more fully in the following.

Response to General Comments

We thank the reviewer for spending his time to review and improve our manuscript. We will address all concerns and suggestions below.

We appreciate the references to further literature and will include them as suggested by the reviewer.

Specific Comments Methods:

Methods: It was not clear to me what aspects of the vegetation cover were actually measured. For example, you use the term "mapped" on page 4 at line 15, but this is a rather vague. Do you have measurements of anything like above-ground biomass or was only species composition recorded? Please write this more explicitly. The description of vegetation complexity on page 5 at lines 2-4 is also not very helpful. Can you give a brief description here, maybe with an equation? The reader should not have to consult another paper (Musso et al.).

Response to Specific Comments:

We agree with this suggestion and will add explanations on how the species composition and vegetation cover was recorded.

We also agree with the comment on the description on vegetation complexity and will add this explanation:

Three study plots were selected at each moraine, based on degree of vegetation complexity (low, medium and high complexity). Vegetation complexity is characterized by vegetation coverage, number of species and the plant functional diversity. The functional diversity is calculated based on specific leaf area, nitrogen content, leaf dry matter content, Raunkiaérs life form, seed mass, clonal growth organ, root type and growth form. The collection of the required data and calculation of the vegetation complexity was done by the Geobotany Group of the University of Freiburg and is described in more detail in Maier et al. (2019).

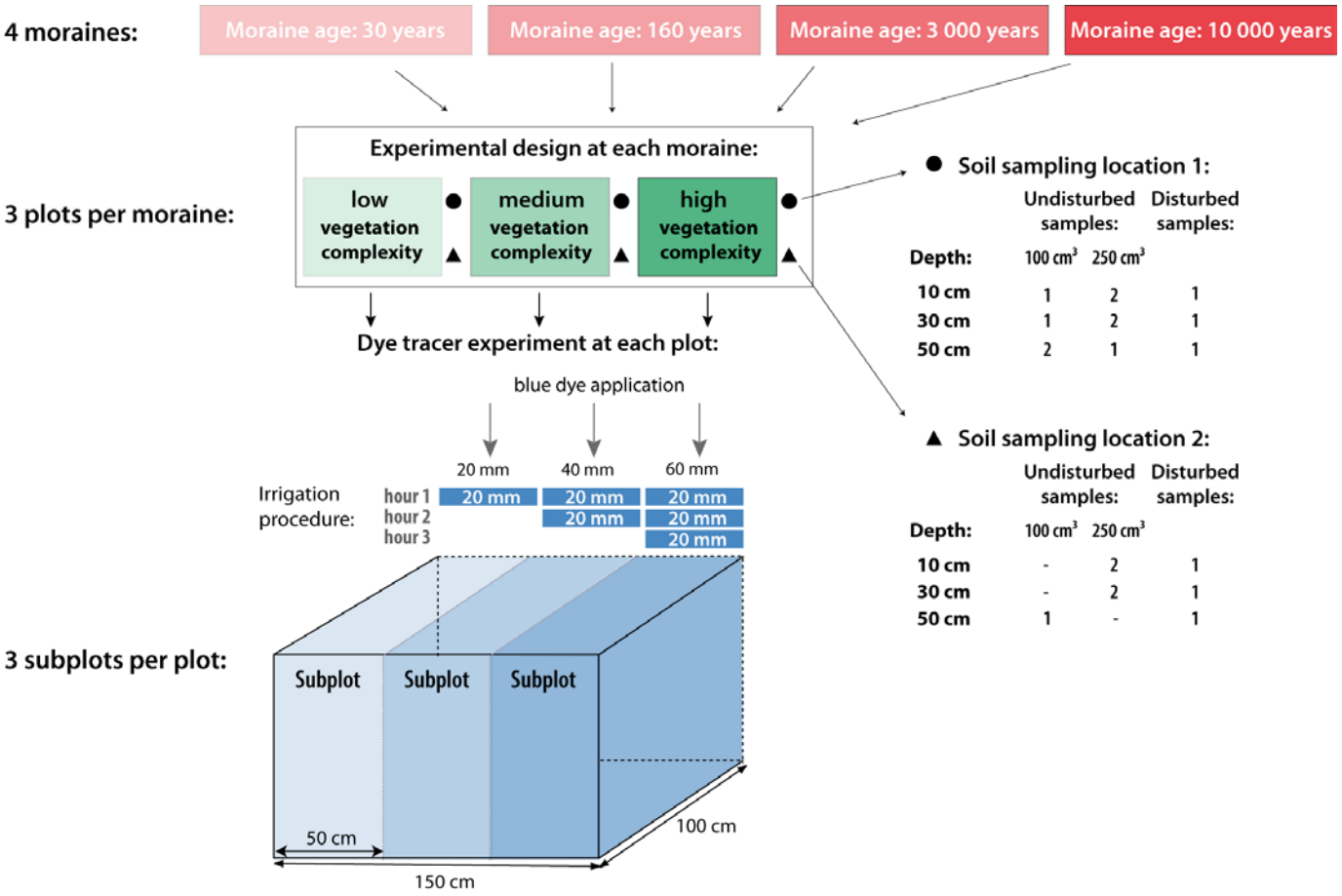
Please note, that we will move this part from chapter 2.1 to chapter 2.2 Field experiments.

The description of the irrigation procedure on page 5 (lines 15-18) was quite difficult to Follow. It seems as if the irrigation pattern was different between the plots. Why was that? Perhaps a schematic figure might help to explain this.

Response to Specific Comments:
We agree with this comment and will clarify the procedure. We will restructure the entire paragraph on plot selection and subdivision of the plots during the irrigation experiment to make it more clear that at each moraine three experimental plots were selected, which differ in vegetation complexity (low, medium, high). For the irrigation experiments each plot was further divided into three subplots for the application of three individual irrigation amounts. We will describe the irrigation procedure as follows:

The tracer was applied with a hand-operated sprayer connected to a battery powered pump which guaranteed a constant pressure for a uniform flow rate of 60 l/h. For a time-efficient irrigation of the three subplots with three irrigation amounts (20, 40, and 60 mm) and an intensity of 20 mm/h, the irrigation procedure was divided into three steps. In the first step all three subplots were irrigated simultaneously for 60 minutes in a sequence of 5 minutes irrigation and 5 minutes break.
This irrigation procedure provides an application of 20 mm to all three subplots. After finishing the first step the first subplot was covered to avoid any additional water input. In the second step, the other two subplots were simultaneously irrigated for additional 60 min in a sequence of 5 min irrigation and 10 min breaks. This irrigation procedure provides an application of additional 20 mm to the two remaining subplots.
In the last step only the third subplot was irrigated for 60 min in a sequence of 2 min irrigation and 10 min breaks while the other two plots remained covered providing an additional 20 mm to this last remaining subplot. After the end of tracer application, the entire plot was covered to avoid any disturbance by natural rainfall.

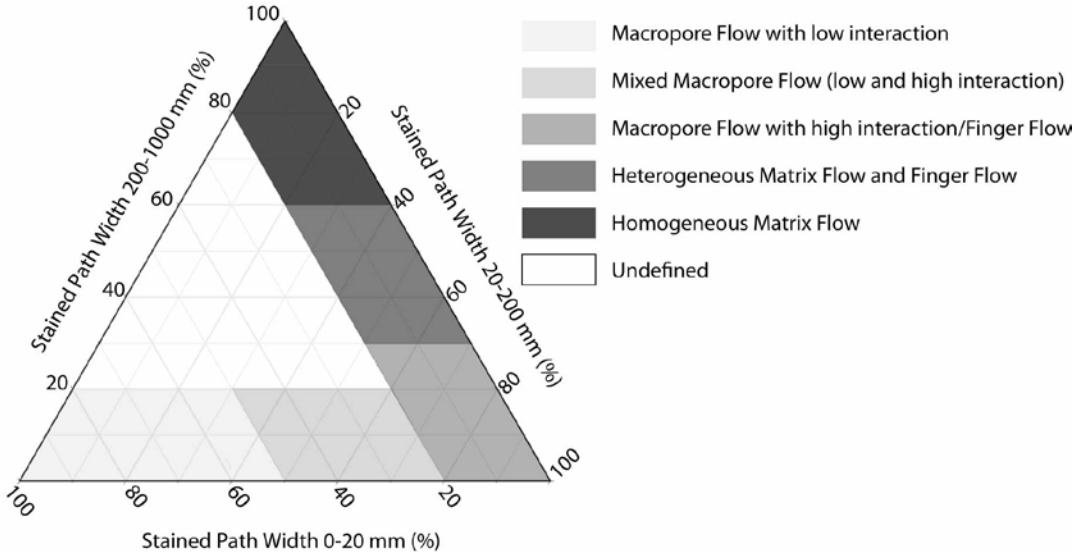
We will also split up Figure 2 in two Figures and include the illustration of the dye tracer plot and the stepwise irrigation scheme into a separate Figure that shows the experimental set up of the field campaign in more detail:



The method to classify the flow patterns into different groupings is described only very briefly (on pages 6/7) and it was also difficult to follow. To complement the text (e.g. at lines 28-30 on page 6), could you give an equation or perhaps include a schematic diagram (or both)? This procedure is quite central to the paper, so it is important to explain this carefully.

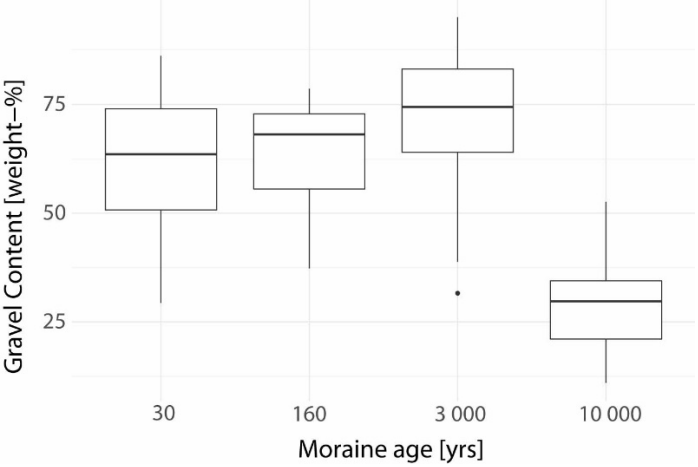
Response to Specific Comments:

We agree with your suggestion and will include a ternary diagram according to Weiler (2001) (see below) that shows which flow type is assigned to which proportions of the 3 SPW classes in terms of volume density.



The description of the particle size analysis on page 7 makes no mention of the gravel/stone fraction (>2 mm). Did you measure the content of stones/gravel? I know this is very difficult in stony soils, so it is understandable if you didn't, but I think this should be stated.

Response to Specific Comments:
We also measured gravel/stone fraction >2mm, but did not show these results. We will report these numbers in the revised manuscript. Stone coverage in our profiles is furthermore shown in Figure 5



The description of how bulk density and porosity were measured (page 7, lines 31-32) is quite vague. Can you describe more exactly (but still briefly) how you measured bulk density and (especially) porosity? It would be good to give some details, because the porosity values are extremely small in the young moraine and in the subsoils. I suppose this is because of the high stone content, but it could also be because air got trapped in the samples during saturation.

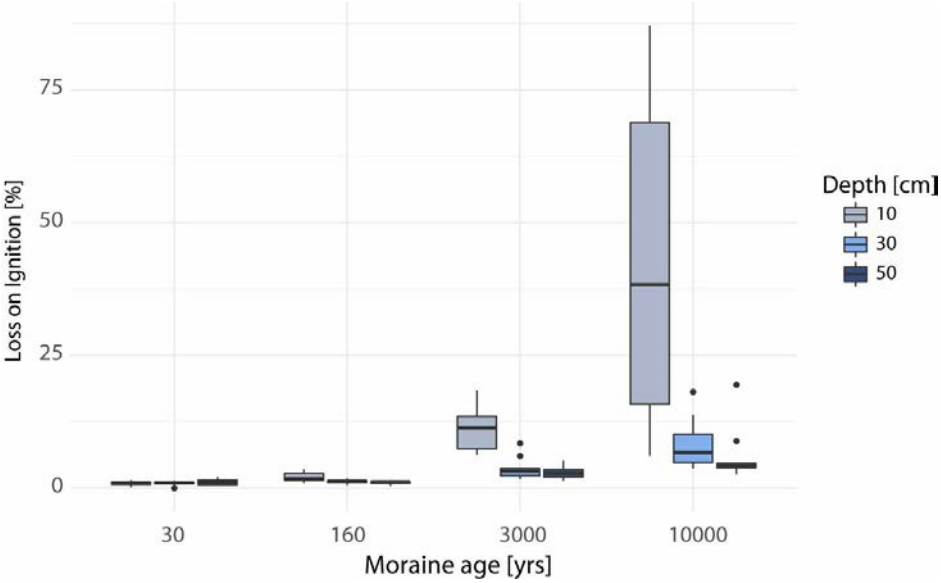
Response to Specific Comments:
We agree with this comment and will include some further explanation on the methods used.

The porosity was determined in the lab using the water saturation method and weighing the samples at saturation and after drying at 105 °C. For saturation, the samples were placed in a small basin. The water level in the basin was increased stepwise by 1 cm per day. When the water level reached the top of the

soil sample and the sample was fully saturated, the bottom of the sample was sealed and the weight at saturation was measured. Bulk density was determined by relating the dry mass after drying at 105 °C to the sample volume.

The authors do not report any measurements of soil organic matter content (SOM). This data should ideally be included in the paper, as the build-up of SOM over millennia due to the growth of vegetation seems to be a very important control on the observed changes in the flow patterns. If SOM was not measured, then I think it should be measured now and the results included in the paper (the analysis is quick and cheap).

Response to Specific Comments:
We will include the SOM data in the revised manuscript:



Specific Comments Processes:
Processes: Can you explain (e.g. on page 16, lines 9-17) why the texture becomes finer with age? Is it due to weathering or is it deposition of fine materials by wind, or maybe both (or something else)? The cause(s) might be obvious to the authors, but perhaps they will not be to all readers.

Response to Specific Comments:
We agree and will include some further clarification that we believe that mainly *Physical weathering due to high fluctuations between day and night temperature and freezing cycles (Birse, 1980) leads to a reduction in grain size down to silt, without changing the particle mineralogy (Ellis, 1992).*

The process(es) operating to increase porosity and decrease bulk density should be explained better (e.g. on page 16, line 20, and lines 33-34). I presume that it is mostly related to a build-up of organic matter in the soil, which is supplied by the litter and roots of the increasingly dense vegetation cover and subsequently processed by soil micro-organisms and fauna, which ultimately results in a more open (aggregated) soil structure.

Response to Specific Comments:
We agree and will include a sentence on that the linkage between the decrease in bulk density and vegetation development is caused by root activities, litter accumulation, and biological activities in the root zone.

The authors associate the homogeneous flow patterns found in the young moraine with "gravity-driven" water flow (e.g. on page 15, line 2; page 18, line 2; page 20, line 21). This is rather misleading to my mind. Fundamentally, it must be the case that both gravity and capillarity were driving the infiltration process in all your experiments, because the soils were (presumably) initially quite dry. In fact, the authors do not really need to discuss whether gravity or capillarity dominated the flow patterns in the different moraines, but if they want to do so, then I think in reality, it is the opposite of what they write. Both macropore flow and finger flow are gravity-dominated processes, whereas a homogeneous flow pattern implies that capillarity was strong enough to prevent the development of any lateral non-equilibrium in soil water pressures. It is this lateral non-equilibrium in water pressures during flow that is a fundamental characteristic of PF.

Response to Specific Comments:
We will remove the reference to gravity-driven flow. However, soils were generally quite wet during the entire field campaign as the total rainfall amount in this region is quite high and hence the soils never really dry out.

Specific Comments Confusion over terminology:
Confusion over terminology: Considering the underlying physical mechanisms, there are three main types of PF (macropore flow, finger flow and heterogeneous flow) and this is indeed the basis of the classification system that the authors make use of in the paper.
However, the authors unnecessarily introduce some confusion at a couple of places in the paper by referring to another classification scheme, one that is not especially useful in my opinion:
i.) page 2 (lines 18/20): There is no good reason to distinguish crack flow from burrow flow (does burrow flow include flow in channels created by root decay?). These can all be lumped into macropore flow (as you do later). If you want to define some subgroups according to the origin of macropores, you should talk about flow in biopores (which includes both root and faunal channels) not burrow flow.

ii.) page 17, line 34: "In the clay layer, no significant macropores were identifiable, which is why it is assumed that the water is transported in cracks …..". Cracks are also macropores. You should replace the term macropores by biopores.

We agree with this comment and will correct our terminology accordingly.

Specific Comments Corrections:
1. The text at the end of the Introduction should be re-arranged. The hypotheses at lines 6-11 don't make much sense at the moment, because they are specific to the case of glacial moraines. It's not clear to the reader where these hypotheses come from. If you move this text to line 19 (after " … impacts water flow paths"), I think it will make more sense, especially if you add "… in glacial moraines in the Swiss Alps" after "… landscape evolution", and delete the last sentence in the first paragraph.
We agree
2. Abstract, Line 1: you should delete "The presence or absence of …"
We agree
3. page 3, line 1: add "volcanic" after "…younger"
We agree
4. page 4, lines 15-16: delete "by the project partners … Germany"
We agree
5. Page 7, line 10: maybe you could add "... and flow mechanisms" after "different properties"

We agree

6. Page 8, line 1: add "…. moraines of differing …" after "four"

We agree

7. Page 8, line 6: replace "the entire" by "all"

We agree

8. Page 8, figure 3 caption: I presume that these results are % of the fine earth fraction (< 2mm). It would be good to state this here.

We agree

9. Page 10, line 5: I don't think you should talk about hillslopes as you haven't mentioned anything about site topography. You could just replace "hillslope" here by "moraine"

We agree

10. Page 12, line 7: This is ambiguous, but I think you mean: "For all four moraines, the volume density is largest in the top half of the soil profile"

We agree

11. Page 13, line 2: interpreting dye tracing patterns can be tricky, since you only get a snapshot in time of a dynamic process. In this particular case, I think it's possible that even if the staining was homogeneous, it doesn't necessarily mean that PF didn't occur. PF could have occurred from the soil surface, but the signs of this may have been obliterated by the later (slower) downward movement of a uniform wetting front in the soil matrix. I am not saying that this is what happened (I'm confident that your interpretation is correct), but I think you could recognize this possibility.

We agree and will add a sentence to account for this.

12. Page 16, lines 26-27: I don't understand how the decrease in bulk density in the first 160 years can be related to a change of particle sizes, since this was marginal. It must be primarily due to the increase in SOM content.

We agree that this sentence is misleading. We already stated in Line 19 that the changes in porosity cannot be traced back to the changes in particle sizes and the same accounts for bulk density.
The sentence is a general statement that reductions in bulk density can be caused by changes in particle sizes and organic matter accumulation. We agree that we need to clarify that at this particular age class accumulation of organic matter is the primary cause for the reduction in bulk density.

13. Page 16, lines 26-34: there is no need to have separate discussions for porosity and bulk density, because they are very closely linked (via the particle density). You could simplify and shorten the text between lines 18 and 34: you only need to write that the increase of porosity and decrease of bulk density was presumably a result of organic matter build-up in the soil due to the development of a denser vegetation cover.

We agree

14. Page 17, lines 2-3: it should be briefly explained (with a supporting reference) how the change in texture could affect bulk density. Presumably the finer particles fill the spaces between the coarser particles? However, I think that the effects of texture on bulk density are usually considered to be relatively small. I think that the increase in SOM content (and associated biological activity in the soil) must be the main reason for the decrease in bulk density.

We agree with this comment and will include some further details that the break down in grain size also influences the bulk density and porosity of the soil. The breakdown of particles leads to an increase in total pore space (porosity) and thus to a reduction in bulk density. But we will also state, that the strong decrease in the topsoil is mainly caused by the increase in SOM.

15. Page 17, line 20: “texture” in this context is quite a vague term. Was it clay content? Please be more explicit.

We agree. We will change the sentence to: *“Saturated conductivity was found to be negatively correlated with the fraction of fine particles. The decrease in gravel content and the increase in silt seem to have an even a stronger effect on the saturated conductivity than the root network development (Maier et al., 2019).”*

16. Page 17, line 30: the coarse nature of the material must be important too?

We agree

17. Page 18, line 27: replace “lower” by “shallower”

We agree

18. Page 18, line 32: should be: “… cover was removed to decrease …”

We agree