Dear Dr. Christoph Mitterer, Referee #2,

We would like to thank you very much for your meaningful comments on our manuscript and your suggestions. We will address all of them in our revised manuscript. Please find here a point-by-point reply to your comments. Your comments are in *italic*. For all the points, we provide answers and we outline our planned changes in manuscript.

Summary

The authors present a set of nine laboratory experiments analysing the flow behaviour of water in a column of layered snow in order to obtain better insights into the evolution of capillary barriers and preferential flow paths in snow. During their experiments the authors used various types of grain sizes and layering and influx rates. In a second step the experimental results are compared to modelled water flow using the 1-D physically based snow cover model SNOWPACK. The model was setup to reproduce the settings of the laboratory experiments. Results show that similar to other porous media (e.g. sand) the water entry suction plays a vital role in the evolution of capillary barriers.

The water entry pressure itself is driven by the layering of different grain sizes. The results on the role of layering concerning the triggering of preferential flow paths remain very qualitative but agree to some extent to other measurements made earlier by a part of the authors team. The only difference observed is that now, the authors do not observe any dependency between preferential flow paths and influx rates. The modelling approach can reproduce the measurements. At the end the authors present experimental difficulties.

Evaluation

In this manuscript the authors present valuable work toward the greater understanding of the very complex behaviour of water flow in snow. Parts of the manuscript are extremely meaningful as it is expected that wet-snow avalanches will become more important in future. In addition, snow as a resource is getting less available and therefore its effective management will become very important in the near future. However, my feeling is that both, analysis and manuscript are still not mature for publication. There are several approaches which I believe are impropriate or need a more in depth reanalysis and/or discussion. Especially the modelling part and the interpretation of the experimental limitations need to be clarified and reassessed. Therefore, I encourage the authors to put more effort into the presented work since the experimental setup has the potential for a new, solid and fundamental contribution on the field of wet snow and water flow in snow. Please recheck the references and check the entire manuscript for stray commas and other minor grammatical inconsistencies. Please put equations into numbered equations, which contrast from the text. In addition add descriptions of the symbols used within the equations.

Answer
We agree with you that advancing our understanding of liquid water flow in layered snow is aimed at providing innovative elements to assess wet snow avalanche risk and/or at improving current description of liquid water flow in snow. We will elaborate on all the points suggested to improve the current analysis of experimental results. We will also check references and grammar, as well as the definition of all symbols used.

General comments

There are three large issues within the manuscript:

- *The use of SNOWPACK to better explore preferential flow paths.*
- *The presentation of the sections Introduction, Theoretical background and the* discussion of the results.

• *The interpretation of the experimental limitations.*

The use of SNOWPACK to better explore preferential flow paths

It is questionable, why the authors decided to use SNOWPACK with the water transport scheme by Hirashima et al. (2010) to reproduce the experiments. SNOWPACK has at the moment the possibility to either use the mentioned and from the authors used approximation of Richards Equation (Hirashima et al., 2010) or to solve explicitly Richards Equation based on different parameterisations (Wever et al., 2014; Wever et al., 2015; Yamaguchi et al., 2010). It is still a matter of debate whether Richards Equation (RE) is applicable when preferential flow patterns prevail and that's what most of the experiments are showing after the capillary barrier. Before the observed flow instabilities or during the evolution of a capillary barrier, Richards Equation might be applicable. Results by a part of the authors team and Wever et al., (2014; 2015) underline this fact, but as soon as preferential flow is prevailing water routing is not well represented by SNOWPACK. This fact is also confirmed by the discrepancy between the arrival times of fast experiments with coarse grain sizes vs. the results of SNOWPACK (Figure 5g-i). Recently, Wever et al. (2015) concluded based on comparisons with upGPR data and manual snow profiles that their RE scheme would unintentionally mimic preferential flow effects. The reason, however, remained unclear. So, if the authors want to use SNOWPACK for exploring their experimental results in more detail, I suggest to use at least the water transport scheme of SNOWPACK implemented by Wever et al. (2015).

In my opinion, however, it would be even more logical and interesting to compare the now obtained experimental results with the multi-dimensional modelling approach of Hirashima et al. (2014). Since there is to some extent a mismatch between the results presented in this manuscript and the statements and finding of Hirashima et al. (2014) and Katsushima et al. (2009a; 2009b; 2013), it would make sense to explore more in detail the reasons for the differences. In fact, Hirashima et al. (2014) mention that heterogeneity alone was not sufficient for the development of preferential flow paths. When both, water entry suction and heterogeneity, were implemented, the model could simulate the formation of a preferential flow paths, which represents again parallels to your experimental results. Additionally, the observations that infiltration rate did not show any correlation to flow behaviour is very different to observations in sand or the results by Katsushima et al. (2013). I highly recommend elaborating on the above points. In this way the manuscript would gain much more relevance, since at the moment it represents only incremental knowledge gain on the field of water movement in snow.

Answer
We agree with you that our modelling analysis may be improved by considering other water schemes. As for SNOWPACK: we will include in the revised version of this manuscript comparative results obtained using the scheme presented by Wever et al. (2014, reference in the text). When preparing the first version of this manuscript, we chose the numerical scheme by Hirashima et al. (2010, reference in the text), which solves a water transport model based on van Genuchten formulation and that includes a parametrization of retention properties of snow and unsaturated conductivity, which are necessary variables for modeling liquid water flow over a capillary barrier. Moreover, we are also more familiar with this scheme and this is an important aspect when comparing laboratory experiments with complex numerical models. However, we agree with you that comparing our observations with new numerical schemes is important and this is why this comparison will be included in the text. We have already considered to include a comparison between these observations and the water scheme by Hirashima et al. (2014, reference in the text). Actually, evaluating that scheme by collecting a broad dataset of liquid water patterns around a capillary barrier was one of the reasons why we performed these experiments. In fact, we agree with you that these observations will provide valuable new data to extend the evaluation presented in Hirashima et al. (2014), which is based on experimental observations by Katsushima et al. (2013) in homogeneous snow. However, the main focus of this manuscript is on experimental results, as it should provide systematic, quantitative and repeated experimental evidences that layered snow is subjected to a pause in wetting front advancement over a capillary barrier (this is what we mean with ponding) and to associated preferential flow. These observations are motivated by scarce

quantitative characterizations of capillary barrier effects in existing literature and may be useful to snow scientists in general. In this context, an evaluation of the performance by a well-known operational snow model (SNOWPACK) may be useful to evaluate the relevance of these processes in ordinary modelling practice. On the other hand, an exhaustive analysis of the settings and performance of the scheme by Hirashima et al. (2014) needs an ad-hoc analysis. This is the reason why we are currently working on a separate manuscript on this topic, following seminal analyses in Hirashima et al. (2014a, 2014b, references at the end of this reply). We will also enlarge our discussion in current Sections 5.1 and 5.2. For instance, we will mention that existing literature suggests a relation between water entry suction and velocity (see DiCarlo 2007, reference in the text) and this supports the idea that water input rate and liquid water patterns in snow are related. On the other hand, we will also mention that our experiments were performed in unsteady conditions and therefore the inflow rate in the sublayer is not precisely known as the capillary barrier stopped water flow. This condition might have affected our results, in that the unsteady flow below the interface is comparable for all water input rates. Moreover, considering outcomes of different experiments in sand (saturated permeability equal to 87 cm/min), DiCarlo (2013, reference in the text) reports that finger width increases with both very high and very low fluxes, whereas it keeps constant for a wide range of fluxes between ~ 0.1 and 10 cm/min (see Fig. 2 in DiCarlo (2013)). Water input rates in our experiments span 10 and 100 mm/h, which represents a narrow range if compared with expected values of saturated conductivity in snow (see Katsushima et al. (2013) for data). We therefore suggest that future developments of this work should investigate the relation between flux and wet fractions more extensively.
Changes in manuscript
We will include in the manuscript an extensive discussion about the performance by the water scheme presented in Wever et al. (2014). Moreover, we will also enlarge the discussion in Sections 5.1 and 5.2 about the relation between preferential water flow in snow and water input rate as outlined in our answer.

The introduction, theoretical background and presentation and discussion of the results

The red thread in this story is missing. The Introduction section is not specific enough for the presented analysis. The authors present on the one hand a too broad summary on water movement in snow and on the other hand miss to mention some very decisive results for the presented topic (capillary barrier and preferential flow). Similar problems arise within the Theoretical Background section. The authors should work more on what are the problems in numerically describe water movement for stable and unstable flow conditions and how their work might help to overcome a long lasting debate on this topic.

The Results section is too short and represents at most a quick overview of the results. I suggest either combining both sections in a "Results and Discussion" section or describing in more details the relevant results and then discuss them with a broader context.

Answer
We agree with these comments. In the revised manuscript, all the text will undergo major structural modifications (see below).
Changes in manuscript
We will eliminate Section 2. In fact, materials in Section 2.1 can be found elsewhere in the literature and this is the reason why we will remove this Section completely. On the contrary, we will include some passages of Section 2.2 in the Introduction in order to make our manuscript more focused on capillary barriers and preferential flow in layered snow. The revised Introduction will explicitly deal with existing limitations in measuring and modelling liquid water flow in snow, with a focus for capillary barriers effects and preferential flow. We will also merge Sections 4 and 5 in a unique “Results and Discussion” Section.

The interpretation of the experimental limitations

I suggest rewriting the entire Experiments limitations section. At the moment this section is very misleading and the interpretation of the authors ruins the outcome of the experiments. Following the statements of the authors, mass balance between measured water influx and measured liquid water content using a portable calorimeter differed up to 434%. As explanation for the mismatch the authors hypothesize that undesired melting may have taken place during the warming of the snow column from -20 °C to 0°C. If this was really the case, all experimental results would be questionable, since this fact means that controlled conditions did not prevail during the experiments. Consequently, no conclusions can be drawn. However, I believe that the explanation for the discrepancy is due to the way the authors measured liquid water content and the inherent measurement error resolution. Absolute measurement errors for the calorimetric method in determining LWC range according to literature from 1%-5% (Kinar and Pomeroy, 2015). By estimating the values of measured LWC with the calorimetric method (Fig. 5) and comparing these values to the mass of water taken from Table 1 and 2, absolute differences in per cent by mass are 0.040.03 (for per cent by volume slightly smaller) and thus slightly higher than the values reported in literature (However, values were only estimated for this calculation! [see xls-file in the supplement]).

The second argument, why the errors seem to be in an acceptable range is that modelled and measured values at least for the experiments with slow and medium velocity are in fair agreement. Since SNOWPACK will not produce any water without additional energy input, I believe that the expected measurement error is the explanation for the differences. The most accurate method to determine LWC so far is the dilution method (Kinar and Pomeroy, 2015). So, if the authors aim for another series of experiments, I suggest using this method, since the measurement errors have to be small in case of fast flowing experiments with MC layering. For the updated version of the manuscript, I would be very nice to see a mature discussion on the experimental limitations and their meaning for the results.

Answer
We apologize if our Discussion in Section 5.4 could be misleading. Actually, instrumental error is also our explanation for the mismatch between supplied and measured liquid water mass. This explanation is discussed between line 20 page 6644 and line 2 page 6645, but we see that the way we presented this discussion could be confusing. Clearly, we exclude accidental melt of our samples as they were stored at -20°C during preparation and at 0°C (in controlled conditions) during experiments.
Changes in manuscript
We will modify Section 5.4. by adding more details about 1) previous uncertainty assessments of the melting calorimetry that we used; 2) the assessment of instrumental error in our experiments, basing on a similar approach to the one kindly reported by you in the xls-file; 3) reasons why we chose melting calorimetry instead of dielectric methods, among others. We clearly agree with you that the dilution method represents a very valid alternative. We will suggest this method explicitly for future experiments.

Specific comments

Abstract

• P. 6628, L. 15-16: Ponding is defined as presence of water with no flow; please change the wording in this sentence and/or define this term more precisely

Changes in manuscript
We will change our wording here.

• P. 6628, L. 18: There is no thickness = 0cm

Changes in manuscript
We will eliminate this specification in the Abstract as it is not key. In the manuscript, we will specify that the thickness *is* ≤3 cm.

• P. 6628, L. 18: Delete "extensive"

Changes in manuscript
We agree.

Introduction

• General remark: I was missing some important conclusions and summaries of topic relevant results from parts of the authors team, e.g. Hirashima et al. (2014) and Katsushima et al. (2009a; 2013).

• P. 6629, L. 3-4: I suggest rewording; especially the term snow porous matrix has only limited meaning; better porous ice matrix

• P. 6630, L. 1-2: I suggest rewording of this sentence.

• P. 6630, L. 21-22: I think the community is aware of the vital role of capillary effects, so please reconsider this sentence. The limited knowledge is purely based on the very high difficulties in measuring and modelling water in snow.

• P. 6631, L1: add "flow" in front of "instability", otherwise it is not clear which type of stability you mean.

Answer
We agree with all these comments. We will therefore modify our Introduction as kindly suggested.
Changes in manuscript
As already mentioned, we will summarize some relevant passages of Section 2.2 in the Introduction, which will be focused on existing limitations in measuring and modelling liquid water flow in layered snow. We will also include a more in depth discussion about the results of our team. Moreover, we will detail reasons why capillary barrier effects and preferential flow pose important challenges for modellers.

Theoretical backgrounds: Capillarity in snow

• General remark: Please use Equations with numbering and symbol description. In addition, I would expect a more specific theoretical background on capillarity in snow; your explanations are partly very basic and might rather fit into a textbook than into a manuscript with a very specific topic.

• P. 6632, L. 22-24: I think that Daanen presented in his doctoral thesis a similar relation of alpha and n to grain size.

• P. 6633, L. 1-2: I think this sentence makes no sense here. Just before, you explain the hysteresis in snow and now you talk about the conversion of pressure head and pressure potential.

Changes in manuscript
As already mentioned, this Section will be eliminated in a general attempt to reorganize the presentation and motivation of our work. Clearly, our Introduction will specifically focus on capillarity and preferential flow in snow, as suggested.

Theoretical backgrounds: Ponding and water flow instability

• General remarks: You sometimes use capillary barrier and ponding as interchangeable terms, however, I think that there are subtle differences, i.e. you might have a capillary barrier and will get ponding on that barrier, but there can be also ponding without capillarity involved e.g. above a melt-freeze crust.

Answer
We agree with this comment.
Changes in manuscript
In the revised manuscript, we will provide a clear definition of ponding as a pause in the undisturbed

advancement of a wetting front due to capillarity effects and consequent accumulation of liquid water over the boundary. This definition is inspired by the description of fingering in layered soils by Baker and Hillel (1990, see page 20). Moreover, we will also pay specific attention to avoid any confusion between ponding and capillary barriers.

• *Please state more clearly why the saturation overshoot found by Katsushima et al. (2013) is so important.*

Answer
Nowadays, the exact physics of preferential flow is still not known (see DiCarlo 2013, reference in the text). Observations show that, in soils, an unstable infiltration profile is marked by an overshoot profile in terms of LWC (saturation overshoot) or suction (capillary pressure overshoot, see DiCarlo (2004, 2007, 2013); Baver et al. (2014), references in the text). This represents an important point as saturation and pressure overshoots in soils are considered the cause of gravitational flow instability, rather than an effect. This suggests also the need to take into account pore-scale processes in the modelling (see again DiCarlo 2013 or Baver et al. 2014). Examples of pressure overshoots have been observed in homogeneous snow samples during preferential infiltration by Katsushima et al. (2013). In addition, Hirashima et al. (2014) report promising attempts to reproduce similar overshoot dynamics using a model. These results provide evidences that preferential flow in snow at 0°C may be explained (Katsushima et al. 2013) and modelled (Hirashima et al. 2014) starting from the theory of gravity-driven instability of fingers in soils.
Changes in manuscript
We will include a specific passage in the Introduction basing on our answer to this comment.

Laboratory experiments: Preparation of samples and experiments

• *General remarks: Combine Table 1+2 to assure better readability.*

Answer
We appreciated this suggestion. However, we would like to keep Table 1 and 2 separated as one (Table 1) deals with experiments design, whereas the other one (Table 2) reports experiments results. Therefore, they will appear in different parts of the text.

• *P. 6635, L. 12: Delete "As a results,…"*

• *P. 6635, L. 25 – P. 6636, L. 8: Rewrite this small paragraph*

• *P. 6636, L.9-10: Rephrase*

Changes in manuscript
We will elaborate on these points.

Laboratory experiments: Data collection

• *General remarks: Why do you express τ as the inverse of velocity?*

Answer
We use min/cm as the unit of dimension of τ as it is a ratio between a time interval and a length scale, thus it is defined as the reciprocal of velocity.
Changes in manuscript
We will include a clearer definition of τ by isolating this Equation from surrounding text and specifying which is its unit of measurement.

• P. 6636, L. 20-21: Rephrase.

Changes in manuscript
We will elaborate on this point.

The comparison with the SNOWPACK model

• General remarks: Keep past tense

• P. 6637, L. 6: You do not give any predictions, but rather compare modelling and measurement approaches.

• P. 6637, L. 8: Please define what you mean with ponding.

• P. 6637, L. 11-25: Please be more specific and shorten this paragraph.

Changes in manuscript
We will elaborate on these points.

• P. 6638, L. 1: Why did you chose this value of 1.51 çC? In SNOWPACK it is possible to set the threshold value of air temperature when snow should turn into rain.

Answer
In our simulations, we have set the rain-snow threshold value to + 1.5 °C. By then setting air temperature to +1.51°C, all incoming water is classified as liquid. This value did not affect sensible and latent heat because wind speed was set to zero in simulations. However, we agree with you that the temperature threshold in SNOWPACK can be adjusted: setting this threshold to -0.01 °C and air temperature to 0°C will allow us to reproduce the same controlled conditions used during cold laboratory experiments.
Changes in manuscript
We will elaborate on this point in the text. Moreover, we will re-run our simulations by considering a rain-snow threshold value of -0.01 °C.

• P. 6638, L. 19-20: Delete this sentence or move it to the Intro.

Changes in manuscript
We will delete this sentence.

Results

• General remarks: see major issues above

Discussion: the ponding process

• General remarks: see major issues above

Changes in manuscript
We will modify the text accordingly, see our answers above.

• P. 6639 L. 18-20: Are you sure that there is no ponding behaviour? In my opinion the chosen resolution hampers the detection, but as far as I can see it from Figure 5 g,h,i there is a pronounced increase in LWC at the transition of the two layers. In Fig. 5i the increase is hardly detectable due to the chosen resolution of the x-axis. If you enlarge the resolution, you will probably see a distinct increase of LWC too.

Answer

In our first version of the manuscript, we report that MC tests reveal no definitive result. This means that ponding behaviour in our MC samples was very varied, although evidences of ponding are visible in all the samples. We apologize if our wording here was misleading. We agree with you that samples MC2 and MC3 show clear ponding and water diversion over the boundary. On the contrary, only localized redistribution of water over the boundary is evident in MC1 (see Fig. 2 in the paper). In the same sample, LWC over the boundary is around 4.5 vol%, which represents a peak if compared with LWC values immediately above (2.7 vol%) and below (1.7 vol%). However, this difference is very reduced with respect to MC2 and MC3.

Changes in manuscript

We will clarify our analysis on this point.

• *P. 6639, L. 21-25: How can you explain the differences between the values of LWC given in Figure 5 and the fraction of blue colour in Fig. 1 for MC1 and MC2?*

Answer

Values of LWC in MC1 and MC2 are quite comparable at all depths apart from 8 cm. Here, LWC in MC2 is higher than LWC in MC1, and this is supported by a wider blue area in MC2 in Fig. 1. However, note that blue areas in Fig. 1 are poorly correlated with values of LWC in Fig.s 4 and 5 for some sections as areas in Fig. 1 represent just a small fraction of the volume of each acrylic ring, whereas values of LWC represent bulk LWC within each acrylic ring.

• *P. 6639, L. 26 – P. 6640, L. 4: Can you please report more quantitatively on the differences of for the various layer transitions, or at least, can you qualitatively show WRC curves and estimate the magnitude of difference for the various transitions? Since you know grain size and density, you can use the parameterisation of Yamaguchi et al. (2010) to determine all necessary variables for modelling a WRC according to van Genuchten (1980).*

Answer

We agree with you that a quantitative analysis may help.

Changes in manuscript

We will include some values of suction for reference LWCs in all the three types of snow considered to evaluate the magnitude of the differences, as kindly suggested. We will also compare these differences with expected water entry suctions in medium and coarse snow.

• *P. 6640, L. 8: I think that "In particular : : :" is the wrong wording here.*

Changes in manuscript

We will elaborate on this.

• *P. 6640, L. 10: What is an interlayer plane? Do you mean layer boundary?*

Answer

We mean the interface between the two layers. The word "interlayer plane" was originally used by Baker and Hillel (1990, reference in the text) for describing fingering during infiltration into layered soils. However, we see that "interface" or "layer boundary" may be clear as well.

Changes in manuscript

We will replace "interlayer plane" with "interface" or "layer boundary".

• *P. 6640, L. 11-13: You link to the findings of DiCarlo (2007), but it is not clear to which findings you interpret your link.*

Answer
We agree with you that this point could be clarified. Actually, we do not aim at linking our findings to those by DiCarlo (2007). We just aim at pointing out that the imbibition curve may represent the generalized relation between capillary entry pressure and liquid water content, as suggested by DiCarlo (2007): "instead of a single valued capillary entry pressure […], the capillary pressure is simply given by the imbibition curve."
Changes in manuscript
We will modify this sentence to make it clearer.

• P. 6640, L. 13-16: I think it is quite keen to assume that 33-36

Answer
We think this comment is probably incomplete. However, in the revised version of the manuscript we will clarify this sentence.

• P. 6640, L. 17-18: I think this is not true; in 2 out of 3 experiments you show a distinct increase of LWC for your MC samples.

Answer
We apologize if our wording was misleading. Actually, we agree with you that our MC samples reveal a peak in LWC. However, as we note in the text, this peak is smaller than the peak in FC and FM samples (this is understandable, as suction differences between fine snow and medium or coarse snow are high), thus revealing that ponding (i.e., accumulation of liquid water over the boundary) is limited in this type of layering if compared with FC and FM samples.
Changes in manuscript
We will clarify this sentence.

• P. 6640, L. 19 – P. 6641, L. 4: This sounds like a mix of Introduction and Conclusions. Please rewrite this paragraph.

Answer
We agree with you.
Changes in manuscript
We will move passages of this Section in the Introduction

Discussion: Preferential flow patterns and travel time of water in snow

• P. 6641, L. 6-26: Please rephrase both paragraphs since it is not clear what you want to explain here.

Changes in manuscript
We will elaborate on these points.

• P. 6642, L. 1-4: I think it is again keen to report on the stability of position of preferential flow paths since your experimental setup does not provide the possibility to explore this; I consider to skip this interpretation.

Answer
We agree with you.
Changes in manuscript
We will remove this interpretation.

• *P. 6642, L. 5-17: I do not get the argumentation of this paragraph: It is obvious that a capillary barrier will decrease your propagation velocity. In addition, τ is in the same order of magnitude, so maybe it is only subject to measurement errors?*

Answer
The main idea behind this paragraph is comparing specific travel times measured in fine over medium snow with those measured in either fine or medium (homogeneous) snow. The first outcome is that the specific travel time in FM snow is higher than the specific travel time in medium snow. This is clearly expected since permeability in medium snow is higher than in fine snow. Also, τ in FM snow is higher than the τ observed in a homogeneous sample made by fine snow. This is less expected as permeability in fine snow is very low. This comparison helps to quantify the relevance of capillary effects in ruling water speed in snow and the arrival time of meltwater at snow base and highlights the reason why including capillary effects in modelling liquid water flow in snow is important.
Changes in manuscript
We will clarify our discussion.

Discussion: the comparison with SNOWPACK

• *Please rewrite this sections after examining the results with the above suggested water transport model*

Experiments limitations

• *Please rewrite this sections after examining the above mentioned suggestions*

Changes in manuscript
We will elaborate on these points.

References

H. Hirashima, S. Yamaguchi and Y. Ishii (2014a). Application of a multi-dimensional water transport model to reproduce the temporal change of runoff amount. Proceedings to International Snow Science Workshop, Banff, 2014.

H. Hirashima, S. Yamaguchi and Y. Ishii (2014b). Simulation of liquid water infiltration into layered snowpacks using multidimensional water transport model. Proceedings to International Snow Science Workshop, Banff, 2014.