Dear Referee #1,

Thank you very much for your insightful comments and extensive suggestions: we will consider all of them while revising our manuscript. Here, we provide some feedbacks about your points. Your comments are in italic for readability. For all the points, we provide answers and we outline our planned changes in the manuscript.

The authors studied the formation of ponding conditions inside the snowpack on microstructural transitions and subsequent preferential flow path formation in laboratory conditions. The laboratory experiments were simulated using the detailed SNOWPACK model, which yielded good correspondence with measurements regarding the ponding conditions, although the overall melt front velocity was underestimated by the model. This is understandable, as the model is 1D only, and it has already been demonstrated that multi-dimensional, or dual domain models will be necessary to adequately describe liquid water flow. The work is interesting in a broad context: from wet snow avalanche formation, to hydrological processes and snow microstructure investigations. In some aspects, the manuscript provides confirmation of previously published results, in other aspects it provides quantitative results for previously published qualitative descriptions, which makes the results useful for other researchers.

My overall judgement: In recent years, advances have been made in modelling of liquid water flow in snow and the understanding of formation of ponding conditions and preferential flow paths. This manuscript fits very well in the ongoing developments. Although the results present a relative small step and entails a relatively small study, the study is nevertheless a very nice, well contained piece of work. It provides significant results, and I can recommend publication in The Cryosphere after a revision, that takes into account, or rebuts, the major and minor issues I'll point out below. A language and grammar correction is also recommended.

Answer
We thank you for this comment. Advancing our knowledge about liquid water flow in layered snow has been one of the reasons why we started designing our experiments. We also aimed at collecting/reporting quantitative observations of capillary barrier effects for evaluating numerical models of liquid water flow in snow. In this perspective, we agree with your general view on the implications of our results.

Major issues:

My main concern for the manuscript is in the presentation of the work. To summarize: the introduction and theoretical background section is too long, contains a lot of irrelevant details and reads more as an introduction to a thesis or a review paper. In my opinion, those sections fail (in the current form) to introduce the context of the laboratory experiments and modelling study. The length of these sections seems to overshoot the size of the actual study performed. This may prevent the diagonal readers from grasping the important aspects, and harms the impact from the manuscript, in my opinion. I think the manuscript would greatly benefit from a thorough overhaul of these two sections, reducing the size of these sections by roughly one third. Some examples where I think a more concise text can be achieved:

P6632, L3-12 doesn't seem to be directly related to the experiments performed, and could be easily summarized by providing the appropriate references.

P6633, L13-16 doesn't seems to be directly related to the work presented here, because as far as I understood, only the classical grain size definition is considered.

P6633, L4-6: Instead of explaining the background of Richards equation, one can just write that "Water flow in porous media is commonly described using Richards equation". The reader can find details in the references provided in the manuscript.

It's not necessary to again write once more that Richards equation is a combination of mass conservation and darcy's law.

P6632, L13-P6633, L12: This section repeats too much information previously published in my opinion. For example, the concept of a pressure head is introduced, although it is not used anywhere else in the manuscript.

These are only a few of many examples how these sections can be made more concise and to-the-point.

Furthermore, the choice of what is discussed in "Results" and what in "Discussions" seems to be rather arbitrary. I think the results section is too short, and the discussions too long. The model simulations are not well presented in the Results section, as well as the measurements of volumetric water content. I suggest either combining both sections in a "Results and Discussion" section, or make sure that both sections get more balanced: "Results" discussing all results from the experiments, "Discussion" the connection to previous research and implications for future studies.

Answer
We agree with you that the structure of our manuscript must be improved. In the revised manuscript, we will shorten and re-focus Sections 1, 2, 4 and 5 starting from your kind suggestions.
Changes in manuscript
We will eliminate Section 2. In fact, we agree with you that 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 relevant 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. We will also merge Sections 4 and 5 in a unique "Results and Discussion" Section.

Minor issues:

-> I missed the mentioning and demonstration of the prerequisite for the experiment: ensure that the water inflow flux is smaller than the saturated hydraulic conductivity of snow Only then, it is considered that the wetting front is unstable. In natural snow covers, this condition would be generally fulfilled, but for laboratory experiments, it depends on snow type and infiltration rate chosen. See for example Eq. 4 in: [Z Wang, Q.JWu, LWu, C.J Ritsema, L.W Dekker, J Feyen, Effects of soil water repellency on infiltration rate and flow instability, Journal of Hydrology, Volumes 231–232, 29 May 2000, Pages 265-276, ISSN 0022-1694, http://dx.doi.org/10.1016/S0022-1694(00)00200-6.] Actually this reference is probably not the most appropriate here, as it probably has been noted long before this one that this prerequisite is required. Maybe the authors can trace back the original study.

Answer
We thank you for this comment. Actually, our main purpose here was to observe capillary barriers development and *subsequent* preferential flow, so the main prerequisite was an initially dry finer-over-coarser texture, since this is the typical condition where capillary barrier effects develop in porous media. Preferential flow is then observed *as a result*. This is the reason why we did not mention any instability criterion when introducing and designing our experiments. Another reason is that the evaluation of stability criteria of wetting fronts in snow is still an open issue. As an example, Katsushima et al. (2013, reference in the manuscript) note that using saturated conductivity as a velocity criterion (according to the original stability analysis of Saffman and Taylor 1958, see DiCarlo 2013 for reference) performs ambiguously in snow (see their Section 4.2, page 213). In

fact, they report that the saturated water conductivity of snow is usually between 10^4- 10^6 mm/h and therefore the wetting front should be systematically unstable in snow for any (natural) water flow velocity and texture (included those tested here). However, they have observed systematic stable infiltration in very fine snow (mean grain size of 0.231 mm). A more refined velocity criterion may consider unsaturated hydraulic conductivity for water entry suction rather than saturated conductivity (e.g., see Baker and Hillel 1990 or de Rooij 2000, references in the text). However, a few data exist of water entry suction in snow (see Katsushima et al. 2013 and Hirashima et al. 2014, references in the text). An additional complication is that our experiments were in unsteady conditions and the water flux at the interface between the two layers is therefore not known precisely.
Changes in manuscript
Starting from our reply, we will clarify the requisites of our experiments in the manuscript (Section 3.1).

-> *One aspect that I didn't found well discussed: there are also a few studies that seem to indicate that the error made by neglecting preferential flow paths is relatively small, particularly in snow below freezing. See for example Fig. 9 in [Philip Marsh, M.-K. Woo (1984) Wetting front advance and freezing of meltwater within a snow cover: 2. A simulation model, Water Resources Research, December, 1984.10.1029/WR020i012p01865], or the discussion on P1862 in [Philip Marsh, M.-K. Woo (1984) Wetting front advance and freezing of meltwater within a snow cover: 1. Observations in the Canadian Arctic, Water Resources Research, December, 1984. 10.1029/WR020i012p01853]. Similarly, from a hydrological point of view, [Wever, N., Fierz, C., Mitterer, C., Hirashima, H., and Lehning, M.: Solving Richards Equation for snow improves snowpack meltwater runoff estimations in detailed multi-layer snowpack model, The Cryosphere, 8, 257-274, doi:10.5194/tc-8-257-2014, 2014] also report that neglecting preferential flow for seasonal time scales seems acceptable. This seems a particular issue for natural snow covers that are well below freezing during extended periods of time. Probably it also plays a role that natural water influx rates are much smaller than used in experiments, as for example the experiments in this manuscript. It would be good to mention this.*

Answer
We have appreciated this suggestion. Here, we focus on isothermal conditions at 0°C, thus avoiding any investigation about wetting front advancement in subfreezing snow. However, we agree with you that evaluating the added value of including explicitly preferential flow modelling in snow hydrology is an interesting open issue. As an example, Marsh and Woo (1985, reference in the text) tested a multiple-flow path model against a uniform flow model using data from Arctic and Sierra Nevada and reported that this improves model performance as it predicts earlier runoff and reduced peak flow, in agreement with data (see their Figure 7). This outcome could show that considering preferential flow may make a difference for short time scales.
Changes in manuscript
We will mention this issue in the revised Introduction of our manuscript.

-> *Abstract, L21: "shows high performances" –> "shows high agreement"*

-> *Abstract, L23: It may be good to include the reason for the underestimation. My suggestion: "while water speed in snow is underestimated by the chosen water transport scheme, which is attributed to the 1D nature of the model."*

-> *P6629, L1: "Liquid water in snow originates from". As snow melt is generally more important than rain, I would mention melt first. Also I don't think the references are appropriate, as this is already known for much longer than 2011!*

-> *P6631, L4: "together" –> maybe "concurrently" suits better here?*

-> *P6631, L12: "a wide dataset" –> "a broad dataset"?*

-> *P6631, L18: "reproduced" –> "simulated"*

-> P6632, L16: "This calls" –> "This is called a", and it I think it is too strongly based on a theoretical basis as to call it an "intuitive relation".

-> P6632, L16/17: This statement is mainly true for snow, but for soil, the Brooks-Corey model is also often used.

-> P6632, L19: "As a rule of thumb" –> "Generally"

-> P6632, L20 and elsewhere: "pores shape" –> "pore shape"

-> P6633, L9: "In unsaturated conditions, K_W depends on S_r". The references provided are inappropriate in my opinion. This should rather refer to the Mualem model? Actually, in the Richards (1931) paper, it is already mentioned that the conductivity depends on the moisture content. See P323, near the bottom of the page.

Changes in manuscript
We agree with all these observations: we will incorporate them in our revised manuscript, apart from those referring to Section 2.1 as we will remove this Section from the manuscript.

-> P6633, Section 2.2: Maybe it is a good idea to provide a definition of "ponding". Sometimes, in literature (e.g., in soil science) it refers to conditions where the suction pressure gets positive. I guess this is not the case in your experiments. To give a suggestion: can it be said that ponding in this manuscript rather can be defined as a situation where the capillary forces dominate the gravity term? And the absence of ponding is a gravity flow dominated regime?

Answer
We agree with this suggestion, as a thorough definition of ponding will make our manuscript clearer. We think that a proper definition of ponding in our conditions may be 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 in the paper). On the other hand, capillary forces could dominate gravity in other specific situations (e.g., capillary rise) and this could cause ambiguity in a definition based on the expected relevance of capillarity or gravity.
Changes in manuscript
We will include this definition in the revised manuscript (Introduction).

-> P6633, L26: "one (historical) case". Not clear what is meant by that. Is there only one documented case where fingering arose in an initially dry fine-over-coarse profile?

Answer
We chose "historical" because fingering in an initially dry fine-over-coarse profile in layering has been a frequent condition used to study the general problem of wetting front instability in soils (see de Rooij 2000, Section 2, page 278: "Hillel and Baker (1988) and Baker and Hillel (1990, 1991) analyzed ponded infiltration into an initially dry profile with a fine-textured top layer over a coarse-textured sub-layer). This configuration (introduced by Hill and Parlange, 1972) has often been used in the laboratory to produce fingering". However, we note that this term plays a very marginal role and we will remove it from the manuscript.
Changes in manuscript
We will remove this term from the manuscript.

*-> P6633, L3: "starts ponding. This causes an increase in LWC". For me, ponding *is* an increase in LWC*

Answer
Please see our previous reply about how to define ponding in these experimental conditions.
Changes in manuscript
We will modify this phrasing in the manuscript.

-> P6633, L22: Rather than mentioning that there is a debate (which is quite useless info), what is the debate about (would be more useful to know)?

Answer
We thank you for this suggestion. We do not see any reference to a possible debate at P6633, but we suspect that this comment is related to L22 P 6634, where we quote a reference by DiCarlo 2013. In our intentions, this quotation should explain in a few words that describing how and why overshoots occur is still an open issue even in soil science. However, we will modify this sentence as kindly suggested.
Changes in manuscript
We will remove this statement from the manuscript. Our intention is to summarize Section 2.2 in the Introduction. We will therefore be more specific on this point.

-> P6633, L26: "similar process" ... "have parallels". Very vague. Please make the sentence more clear.

-> P6635, L11: As it apparently was not possible to achieve these rates exactly, I suggest writing: "considered are approximately 10, 30 and 100 mm/h"

Changes in manuscript
We agree with these observations: we will incorporate them in our revised manuscript.

-> P6635, L11: What is the reason for this extremely high water input rates? They can hardly be considered relevant for most natural applications. Melt rates and rainfall rates are generally much lower. What is the reason? I can imagine that it has to do with the laboratory setting, as I realize that it may be hard to find a control system that is able to apply low water input rates, like 5 mm/h. Please provide some explanation here.

Answer
We selected 10 mm / h, 30 mm / h and 100 mm / h as reference water input rates in order to explore capillary barrier effects within a broad range of natural input rates. This is because it has been sometimes reported in the literature that water entry suction may be affected by water input rate (see DiCarlo 2007, reference in the text). Moreover, Katsushima et al. (2013, reference in the text) report that preferential flow patterns (namely, the variable *f* that we used in the manuscript) may be also affected by *W*. Exploring the dynamics of capillary barriers for a wide range of *W* may therefore ensure that conclusions are less sensitive to velocity effects. These fluxes were also the result of a trade-off between the range of possible *W* and logistical constraints (such as the duration of experiments).
Changes in manuscript
We will add some comments on this point in the manuscript: first, we will elaborate on Section 3.1 to detail our choice of *W*; second: we will add some comments about velocity effects in the Discussion.

-> P6635, L12: Instead of 3g_s, I suggest writing: "As a result, nine samples were prepared (one for each of the three grain sizes and three water input rates)".

-> P6635, L23-24: Apparently, the statement is too strong, as it is not clear whether the initial condition of the snowpack was dry. (see P6644, L14).

Changes in manuscript
We agree with all these observations: we will incorporate them in our revised manuscript.

-> P6635, L25-28: Maybe just choose one unit and report all results consistently.

Answer
We would like to keep both units as one is experimental and refers explicitly to a mass flux (g/min), whereas the second one (mm/h) is derived and represents a more familiar variable in hydrologic applications. In the text, we systematically use the second one.

-> P6636, L2: "Consequently". It is not clear if the low variation in snow density is by design or by accident. I think "consequently" is not the right conjunction here.

Answer
We did not apply any tamping during sieving operations so we had no direct control on the values of dry density. Given the low coefficient of variation observed, we point out that this work investigates how the properties of capillary barriers and associates preferential flow vary with grain size. Future investigations should focus on the generalization of this work to layers of different density (as e.g. done by Yamaguchi et al. 2010 and 2012, references in the text).
Changes in manuscript
We will modify this passage of the manuscript to clarify this point.

-> P6636, L5: Unless it is irrelevant for replication of the experiments, maybe provide some detail of the "operational reasons".

Answer
We decided to increase the thickness of the lower layer after performing a first set of experiments. The main idea was increasing the distance between the interface between layers and the lower base of the sample in order to increase the amount of observations of our experiments. However, our conclusions are not affected by this difference, as ponding of water occurs in the upper layer.
Changes in manuscript
We will remove this statement from the text. This is because our operational reasons are useless for interested readers. On the other hand, it is important to point out that conclusions are not affected by differences in the thickness of the lower layer: we will add this in the text.

-> P6636, L5-6: It seems that the number 1, 2 and 3 refer to the water influx rate. Please introduce this nomenclature near P6635, L11-12.

Changes in manuscript
We agree. We will add this nomenclature as suggested.

-> P6636, L15: See my earlier comment. I guess the reason is that it is really difficult to exactly apply a specified infiltration rate? Maybe say this then explicitly.

Changes in manuscript
Exactly. We will improve the text here.

-> P6636, L20: "specific travel time". By dividing by a length scale, it rather is a velocity than a travel time. I suggest the term "bulk velocity" here. Or something similar that makes clear it is a kind of velocity.

Answer
The unit of measurement of τ is min / cm, as it is the ratio between time and a length scale. Its reciprocal is a velocity. In this framework, calling it “bulk velocity” may be ambiguous as it may suggest a wrong relation between τ and travel time, as a higher τ means a slower flow, contrary to the relation between velocity and time.

Changes in manuscript
We will specify the unit of τ in the main text.

-> P6636, L27: "starting from these information" –> "These measurements are translated into ..."

Changes in manuscript
We will incorporate this.

-> P6637, L1: "ImageJ" is a rather unspecific software package. Maybe provide more information here how the wet part was determined. Probably this involves some manipulations with contrast/brightness and/or specifying some thresholds how "blue" the image should be in order to consider it to be wet? This information may be helpful for other researchers and for replicating these type of experiments.

Answer
ImageJ (http://imagej.nih.gov/ij/) is a software of image processing that is publicly available on the Internet. Calculating wet areas may be performed by using automatic image processing tools and defining, for instance, a threshold in colour. However, we have performed here a manual detection of wet areas for all the sections considered, as a high degree of human judgement may be more reliable than automatic detection when dealing with complex patterns of liquid water flow in snow. This meant manually delimiting fingers area for all the sections. Then, ImageJ performs an automatic calculation of the extension of these blue areas. This value is then used by the software to calculate the ratio between blue and total areas. Clearly, similar calculations may be performed using alternative software.
Changes in manuscript
We will provide a link to the software and its version as additional information. We will also specify in the revised manuscript that we delimited blue areas manually and that similar calculations might be performed using alternative software.

-> P6637, L11: "FEM": abbreviation is not introduced.

Changes in manuscript
We will include "Finite Element Model" in the manuscript.

-> P6637, L14: "or liquid water content" –> "and liquid water content"

Changes in manuscript
We agree.

-> P6637: I think this is not a complete description of model setup. For example, in Wever et al. 2014 and 2015, also a parameterization for saturated hydraulic conductivity is specified, as well as a model for unsaturated hydraulic conductivity. In Wever et al. 2015, additionally an averaging method is specified for hydraulic conductivity at the interface nodes. Is there a version number for the SNOWPACK version used in the paper? Maybe also include a link to a source code repository or something similar where the source code can be retrieved?

Answer
We agree with you that additional information about model setup will be very useful: we will provide the details of SNOWPACK settings in our revised manuscript. The version of SNOWPACK we are using originates from an older version of the model than the one currently presented in, e.g., Wever et al. 2014 or 2015, and it is not included in open source SNOWPACK, so we cannot link it to a precise version number or code repository. Note that, following some comments by Referee #2, we will add in the text comparative simulations using the water scheme by Wever et al. 2014.
Changes in manuscript
We will provide a table with detailed simulation conditions, such as equations of hydraulic conductivity, suction, residual water content etc. We will provide these data for both schemes used.

*-> P6638, L4: How was this achieved? By just taking the incoming longwave radiation equal to epsilon * sigma * Tˆ4, using Stephan Boltzmann's law?*

Answer
The value of long wave radiation was determined from Stefan Boltzmann's equation with $L=\varepsilon\sigma T^4$, $\varepsilon=0.97$, $\sigma=5.67*10^{-8}$ W m^{-2} K^{-4}, T=273.15 K.
Changes in manuscript
We will add this specification in the text.

-> P6638, L1: Not clear what the relation is between the air temperature and the incoming water flux?

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 Referee #2 that the temperature threshold in SNOWPACK can be adjusted: setting the 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 clarify this point in the text. Moreover, we will re-run our simulations by considering a rain-snow threshold value of -0.01 °C.

-> P6638, L21: "We report" –> "We show"

Changes in manuscript
We agree.

-> P6639, L19: "no definitive results" Please expand on this.

Answer
We will clarify our conclusions on this point. When considering FC and FM samples, horizontal redistribution of water and ponding over the capillary barrier was systematic: we observed a highly saturated section even 2 cm above layers' interface. Results for different water input rates are also very coherent. On the other hand, MC samples returned more varied results. For instance, water spreading is restricted for MC1, while a marked ponding effect is visible in MC3.
Changes in manuscript
We will improve our presentation here. Starting from Referee #2's suggestions, we will also provide some quantitative analyses about the differences in suction for layers of different grain sizes in order to discuss this outcome in details.

-> P6639, L21 and elsewhere: "4/6" –> I prefer "4 out of 6"

Changes in manuscript
We agree.

-> P6640, L8: For interpreting the value of 33% (and the values mentioned later), it may be really useful to have a kind of error estimation for this measurement. If it is not possible to get a quantitative error measure, maybe the authors can use their expert judgement to provide the reader with a kind of "poor-man"'s error estimation?

Answer
We agree with you that a quantitative error estimation may be helpful. We will therefore add some comments on this point (see below).
Changes in manuscript
In the revised manuscript, we will completely rewrite Section 5.4 by adding more details about 1) previous uncertainty assessments of the melting calorimetry that we used (the so-called Endo-type snow-water content meter proposed by Kawashima et al. 1998); 2) the assessment of instrumental error in our experiments, basing on a similar approach to the one kindly reported by Referee #2 in his xls-file; 3) reasons why we chose melting calorimetry instead of dielectric methods, among others. This new focus of Section 5.4 should provide enough details for interpreting LWC measurements in our paper. Thank you.

-> P6640, L8: "interlayer plane" –> "interface"

-> P6641, L7: "We measure this" –> "We describe heterogeneity using the variable f"

-> P6641, L15: I don't think that "e.g." can be used in the middle of a sentence, only as "e.g., <text>"

Changes in manuscript
We agree. We will improve the manuscript.

-> P6641, L28: Does it mean that the debate is between Schneebeli (1995) and Waldner et al. (2004)? Actually, I don't agree that there is a debate, I just think both observations have been done, and apparently both cases can occur (i.e., preferential flow paths following the same path, or creating new paths).

Answer
We agree with this comment and with your idea. Clearly, this sentence does not mean that the debate is between Schneebeli (1995) and Waldner et al. (2001).
Changes in manuscript
We will welcome Referee #2's suggestion: we will remove this discussion from the manuscript.

-> P6642, L3: I couldn't find the value of 13% in Waldner et al. (2004). I could only find a value of 1.3% in Fig 13 in that paper, or on P7 in the text (my understanding is that 0.013 mˆ3/mˆ-3 = 1.3%). Note that additionally, i.e. should be e.g.

Changes in manuscript
We accidentally misinterpreted the value of 0.013 mˆ3/mˆ-3 in Waldner et al. (2004). We will modify the discussion accordingly.

-> P6642, L12: "It follows" is maybe too strong, as direct comparison of infiltration rate is rather difficult. "It suggests" suits better.

-> P6642, L12-L16: I had some difficulties understanding the sentence, I would recommend to break it into smaller sentences, because it is a rather important point.

Changes in manuscript
We agree with these comments. We will modify these passages.

-> P6642, L27: I'm not sure if Wever et al. (2014) is the suitable reference here. Doesn't this refer to their analysis of the melt water front progress measured via the ground penetrating radar, which was published in Wever et al. (2015)? In any case, please specify what "field observations" you are pointing to.

Answer
In this section of text, we refer to the comprehensive evaluation of different water schemes proposed by Wever et al. (2014). They evaluate a bucket type approach, an approximation of Richards Equation (NIED scheme, the scheme we consider here) and the full Richards Equation by using lysimeter data from Weissfluhjoch (WFJ) and Col de Porte (CDP). In that paper, Wever et al. (2014) note that "An analysis of the runoff dynamics over the season showed that the bucket-type and approximated RE scheme release meltwater slower than in the measurements, whereas RE provides a better agreement (Abstract)", or that "The simulations with RE produce runoff soon after the first measured runoff, whereas the bucket and NIED simulations show some delay. For the rest of the melt season, there are no important differences. Because the bucket and NIED simulations withhold the water too much in the snowpack compared to the lysimeter and the simulations with RE, the daily outflow near the end of the season becomes higher than in simulations with RE". (Section 4.1, Daily time scale). We relate this to our observations as an overestimation of runoff timing may be related to an underestimation of liquid water speed in snow due to the absence of a specific treatment of preferential flow. However, we see that this link is not clearly expressed in the text and that the current version of the manuscript may suggest a discussion of water speed in snow rather than runoff timing at snow base. We will therefore elaborate on this discussion.
Changes in manuscript
We will clarify this passage in the text as outlined in our answer.

-> P6643, L5: "decrease in LWC". At first sight, this sounds as a temporal decrease, but I guess it is about the vertical shape of the profile? Maybe write then: "The model predicts correctly the low values in LWC below the boundary."

Changes in manuscript
We agree. We will improve the manuscript on this point.

-> P6644, L5: "a heavy parametrization can play an important role". This sentence is a bit vague. It sounds like that a heavy parametrization is having so many degrees of freedom, it can fit everything and thereby plays an important role, but I don't think that this is the message to be conveyed.

Answer
We apologize for this misunderstanding. The main idea here is that the water scheme by Hirashima et al. (2010) includes the prediction of several parameters/variables, such as suction, unsaturated conductivity or permeability. Clearly, other water schemes need similar information. The predictions of all these parameters/variables rely mostly on experimental parameterizations that are clearly affected by statistical and experimental noise. This problem is paramount in the case of snow as performing experiments with a material undergoing phase change is very challenging. This noise may cause uncertainty in the prediction of parameters and this may affect the performance of any model used to predict liquid water flow in snow. We will improve our discussion on this point.
Changes in manuscript
We will clarify this passage in the text as outlined in our answer.

-> Experimental limitations: nice section to have here.

-> Conclusions: The first paragraph is a too long summary of the introduction, which is not necessary at this point. Basically, in my view, P6645, L4-12 can be removed.

-> Where Table 2 is explaining the symbols in the caption, Table 1 is not. I prefer that the symbols used in the table are explained in the table caption, so the Tables are self-explanatory.

-> Figure 1: it would be helpful if the caption mentions the diameter of the rings, in order to interpret the figure.

-> Figure 2: it would be helpful if a scale is added to the figure, for example: a vertical bar denoting the 2cm extent of each ring.

-> Figure 3, 4 and 5: in print, some lines didn't show up properly. Particularly the axes were bad in print. Please increase the thickness of the lines.

-> Figure 3: It would be helpful to explain symbol f in the caption. Maybe also mention that f is observed.

-> Figure 4: Maybe write: "in terms of measured volumetric liquid water content"

-> Figure 5: It would be more logical if the dots are plotted in the middle of the ring, as it concerned the LWC in the ring, rather than at the top of the ring.

Changes in manuscript
We agree with all these observations. Figures will be improved as suggested. The only exception is Fig. 5, where we would like to plot dots as a function of the elevation of rings top surface (i.e., to keep this Figure as it is). The main reason is that this enables a direct comparison between Fig. 5 and all the other figures, as the vertical coordinate is the same in all the plots. This is nonetheless clearly explained in the caption.