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Interactive comment on "Laboratory-based observations of capillary barriers and preferential flow in layered snow" *by* F. Avanzi et al.

Anonymous Referee #1

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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 quali-

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tative 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.

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

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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.

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.J Wu, L Wu, 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.

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-> 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.

-> 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"?

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-> 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.

-> 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?

-> 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?

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

-> 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)?

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

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-> 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"

-> 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.

-> 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).

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

-> 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.

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

-> 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.

-> 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.

-> 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.

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

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-> 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.

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

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

-> 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 a 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?

-> P6638, L4: How was this achieved? By just taking the incoming longwave radiation equal to epsilon * sigma * T^4, using Stephan Boltzmann's law?

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

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

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

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

-> 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?

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-> 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>"

-> 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).

-> 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.

-> 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.

-> 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.

-> 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."

-> 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

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this is the message to be conveyed.

-> 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.

Interactive comment on The Cryosphere Discuss., 9, 6627, 2015.