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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Liquid water infiltration into a layered snowpack: evaluation of a 3D water transport model with laboratory experiments" *by* Hiroyuki Hirashima et al.

Anonymous Referee #1

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General: This paper shows a very interesting model analysis of water transport in a snow sample. Both the experimental data and the model have been presented earlier and this paper now compares the model with the data set. This new comparison offers interesting insight into the process and offers a technically correct analysis. The presentation is concise, state of the art is presented in a balanced way and conclusions supported by the analysis. The paper is in scope and quality suitable for HESS.

One major suggestion concerns the SNOWPACK simulations. I understand that the reason for using the SNOWPACK version without preferential flow parameterization in order to show the differences caused by the preferential flow. However, I don't under-

stand why not additional simulations are shown, which use the SNOWPACK preferential flow parameterization. This would add value to the paper and increase its impact for two reasons: i) The 3D model is (computationally) limited to small domains (see also next comment below) and if the reader is provided with an analysis that allows to judge how much of the effect is covered by the SNOWPACK parameterization, then this has a lot of practical value for scientist that need to do larger-scale simulations. ii) The analysis would already give a first indication on how the slightly different treatment of preferential flow path initiation (entry suction) in the 3D model vs. SNOWPACK compares and would therefore add substance to the discussion of the entry suction problem, which is well executed in the paper otherwise. My suggestion is further supported by the fact that the current paper has not already too many new elements or this too long and this additional analysis should be easy to execute. One additional (major) comment concerns the missing discussion of the domain size effect.

While domain sizes have been chosen congruently between measurements and simulations, the generalization of the results may still be suffering from the small lateral extent in both. The dye experiments we know from snow (e.g. the ones from Schneebeli mentioned in the paper) show very significant lateral spreading of flow paths much beyond the scale of the experiments presented here. This aspect should be properly discussed.

Detailed (minor) comments:

p.3 l.27ff: Maybe mention additional snow characteristics (grain type) and how you produced the snow samples?

p.4 l.14: "of" breakthrough on breakthrough

p.4 l.19: Maybe grid points or elements instead of "meshes"

p.4 l.21: "expended" ?

p.4 l.29: "anticipated" measured

p.4 l.32: Here you could add some discussion on the effect of domain size (see above)

p.5 l.4: “85” add minutes

p.5 l.7ff: By comparing to Walter I think you compare two different things: Velocity of water in an existing preferential flow path (Walter) vs. propagation speed of path initiation. Please discuss appropriately

p.5 l.14: Please mention the reason of why it could not be determined

p.7 l.1ff: I would suggest to add a general comment that deviations of both models from measurements have the same order of magnitude

p.7 l.21: Why “in practice”?

p.8 l.8: “too vertically strict” is a funny expression and I suggest to try to explain it (maybe domain size as mentioned above?)

p.8 l.13ff: Here you should discuss that the model does not predict any structural changes in the snow grains. This is discussed further below but this is not sufficient. In reality, grains will grow quickly in contact with water at the walls of preferential flow paths and this will also promote lateral spreading of water, I think

p.9 l.4ff: Metamorphism could help to explain lateral spreading

Figure 1 legend: Explain the term “front grid”

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